



LONDON MEDICAL GAZETTE.

NEW SERIES.

VOL. I.

FOR THE SESSION 1837-38.

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THE

London

MEDICAL GAZETTE;

BEING A

Weekly Journal

OF

MEDICINE AND THE COLLATERAL SCIENCES.

Handwritten: 1837-38

(NEW SERIES.

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Handwritten: 427253 / 29.8.44

LONDON:

PRINTED FOR LONGMAN, ORME, BROWN, GREEN, & LONGMANS,
PATERNOSTER-ROW.

1838.

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THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF
Medicine and the Collateral Sciences.

SATURDAY, SEPTEMBER 30, 1837.

LECTURES
ON THE
PHYSIOLOGY AND DISEASES OF
THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL
AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of
1836 and 1837,

At the Anatomical School, Kinnerton-Street,
near St George's Hospital,

By CHARLES J. B. WILLIAMS, M.D. F.R.S.

—
LECTURE I.

Introductory remarks on the claims of diseases of the chest to be separately considered—Importance of studying their physical as well as their general signs—Illustrations—Practical insufficiency of either class of signs separately—Advantages of studying both rationally—Analysis of the mechanism of respiration—Forces and mechanism concerned in dilating the chest, upwards, outwards, and downwards—Forces which diminish the cavity of the chest—Modifications.

GENTLEMEN,—A course of lectures devoted specially to the consideration of the viscera of the chest in their healthy and diseased state, may be open to the objection, that any partial study of a set of organs and their diseases tends to engross the mind with a disproportionate attention to these organs, to the neglect of general disorder of the system, and of affections of other important viscera. In answer to this objection, we might urge the great prevalence and fatality of thoracic diseases in this country; constituting nearly one *half* of the fatal cases, and perhaps quite one *half* of the slighter disorders; and we might strengthen

this argument by the fact, that many other diseases, both local and general, owe their serious or fatal character to secondary thoracic lesions. In fact, such is the immediate relation between the thoracic organs and life, that when diseases of other parts prove fatal, it is by arresting the functions of those organs, and when the process of this arrest is slow, morbid conditions are often produced in the lungs and heart, resembling those of their primary diseases, which speedily accelerate the fatal event by their superadded influence. It is thus found that in many of those who die of lesions of the brain, abdominal viscera, &c., especially those of a chronic character, the lungs are congested, and even hepatized, and effusions of serum and of lymph are found in the thoracic cavities.

But the great prevalence and fatality of thoracic diseases are not sufficient reasons for making the lungs and heart exclusive objects of study; the condition of extra-thoracic organs in these as well as in other disorders requires notice; and it will be a leading aim of the following lectures to direct attention continually to the general states of the system, as well as to the local disorder, and to show how important it is for the practitioner to instruct and guide himself by all the symptoms, general and local, vital and physical, in combination. I would, therefore, rather give another character of thoracic diseases, as a sufficient reason for treating of them separately. They are, in the present state of our knowledge, more than any other internal diseases, susceptible of clear and conclusive illustration; and this for two reasons:—First, because the structure and functions of the organs within the chest are, for the most part, simple and intelligible; secondly, because these organs, in the performance of their functions, have certain mechanical and physical relations, which may bring their conditions, healthy and morbid, under the more or less immediate cognizance of our external senses; so that

our study of the most serious internal affections may be approximated to that of simple external disease.

Studied merely through their general or animal signs, thoracic diseases are often as equivocal in their character and insidious in their progress, as they are serious in their tendency. Thus, *pain* is often absent in affections of the worst character, whilst it may be acute in trivial complaints. *Cough* depends on an undue irritation, direct or by sympathy, of the lining membrane of the air-tubes, a part only of the respiratory machine, and is by no means proportioned to, or determined by, the extent of the thoracic disease. *Dyspnoea* also depends on the sensibility of the nerves with regard to the change of the blood which should take place in the lungs, which sensibility we know to be influenced by a variety of causes foreign to the lungs themselves. The *pulse*, that "*res fallacissima*," is often not proportionately influenced by a visceral lesion, or when it is so, does not indicate the seat or extent of that lesion. So also *fever* is an affection of the whole system, and is neither a measure nor a constant concomitant of the maladies of individual organs. Thus we may have inflammation of the lungs, nay, even of the pleura, without pain, without dyspnoea, with little or no cough, without any constant character of pulse, or degree of fever. We may have cough, severe pain of the chest, dyspnoea, strong and quick pulse, and febrile disturbance, without inflammation of the lungs.

I will give you an example, which is not a very rare one:—An intimate friend of mine, a physician in the country, who was distinguished no less for professional abilities, than for a highly conscientious candour, was called to see a gentleman of middle age, who had long been suffering from dyspepsia, and who appeared to my friend to have one of the attacks of his usual complaint. There was no other pain than what he had been often accustomed to in his common ailment; he felt no dyspnoea, but only temporary oppression, as often before, from flatus; he had no cough; his pulse did not exceed 80; and there were nausea, and a feeling of general uneasiness, but no more fever than what frequently accompanies what is called a bilious attack. Such the complaint was considered to be, and as such it was treated. After several days of no remarkable change in the symptoms, the physician was suddenly called to the patient, whom he was shocked to find pale, cadaverous, bathed in a cold sweat, with an intermitting, thready pulse, and obviously dying. The change, or *sinking*, came on only six or seven hours before death; and yet, on inspection, almost the

whole of one lung was found in an advanced stage of hepatization; a sufficient proof that extensive inflammation must have been carried on for several days, although it gave no indication of its presence by general signs.

I will give you an example of a still commoner case, in which, if the general signs were less negative, they were still insufficient to attract the attention of the medical attendant. A practitioner asked me to see a patient, whom he had been treating for a *liver affection*. It was a labouring man, and he complained of great weakness, with some pain low down on the right side, which was full and tender in the region of the liver. The skin was slightly jaundiced, and he had profuse perspiration every night. The complaint began gradually, and obliged him to leave off work, he said, through weakness, which was increasing in spite of the tonic remedies which he then used. Although I was fresh from Paris, and the wards of Laennec, I was almost ready to fall into the opinion of the surgeon; but finding, on more minute inquiry, that there was a slight cough, I began to examine the chest. One tap of my fingers was enough to reveal where the chief disease lay. One side of the chest, the right, was full of water, the effusion consequent on a latent pleurisy. This extensive local disease, although not evident by distinctive general signs, had already begun to oppress the functions and reduce the strength, and if still neglected, it would most probably soon have taken on a chronic form, accompanied by changes of structure, over which medicine exerts very little control, and if then discovered, the body would have been wasted and worn down before any beneficial influence could have reached the real seat of the lesion. As it was, a series of large blisters to that side, mercurial diuretics, followed by tonics and similar appropriate treatment, soon effected a cure.

Such cases as those now mentioned are by no means very rare; many examples have happened within my own observation, where a latent pneumonia or pleurisy has been revealed only by the scalpel after death. In many instances, too, the same lesions have been discovered during life as unexpectedly by auscultation; and if the discovery has not been always followed by a successful treatment, it has at least saved the practitioner from errors which would have degraded his skill in the eyes of others—errors with which his conscience would have been often haunted in hours of despondency and self-distrust. But in many cases, such a discovery of the true nature of the disease has prompted a line of treatment which has proved palpably successful; and

the certainty of the information thus obtained gives to the practice a decision and an energy that a conscientious man can never bring to bear in cases of perplexity and doubt.

It has occurred to me in a good many instances, to see patients supposed to labour under complaints called bilious, dyspeptic, nervous, or designated by the more convenient term general debility; and these patients do truly exhibit many symptoms of biliousness or dyspepsia, nervous irritability or weakness: but instead of being benefited by the remedies usually resorted to in such affections, calomel and black dose upset them, peptic remedies do them no good, active exercise aggravates all their complaints; tonics and stimulants are equally prejudicial; and the patients have gone from doctor to doctor, getting nothing better than temporary relief, and oftentimes getting something worse. In these cases, the liver, the stomach, the nervous system, the general strength, have truly suffered, but they have suffered secondarily. A disease, either functional or structural, of another organ, has been the true *fons et origo mali*; and that organ is the heart. This, by failing in its important duty of carrying on the circulation, that spring of all functions, whether its propulsive force be insufficient, or whether its valvular apparatus admit regurgitation, involves other organs in disorder; viscera are congested, secretions depraved, sensibility deranged, functions disturbed, muscular power impaired; and all these effects may be obvious and distressing, whilst the original cause is either imperceptible or eclipsed amid the crowd of mischief that it has raised. Now I do not mean to assert that in all such cases physical signs will discover the cause, or by discovering it will enable us to remove it; but I can state with some confidence that they will often discover it, and they will then generally enable us to direct our practice more successfully and more safely than we could do without them.

I might easily multiply illustrations, but let one more suffice. The late epidemic, so injudiciously designated by the single name influenza, in many instances comprehended the most serious inflammations of the thoracic viscera, which were masked by the general symptoms, and were to be detected among the catarrhal and bilious affections common to the disease, only by means of the physical signs.

For the sake of my junior hearers, it may be well to explain here that *physical signs* are those which the diseased part can impress on our external senses, directly or through some physical medium. Thus the ocular examination of an external dis-

ease—the feeling of a tumor within the reach of the fingers—the listening to sounds depending on the form, position, or density of internal parts—all furnish us with *physical signs*; which, as they are produced by invariable and intelligible physical laws, are free from the fallacies of general symptoms, which depend on the varying and more mysterious properties of animal life—such as sensation, irritability, and sympathy.

While I would thus maintain the importance of attention to physical signs, I am very far from wishing to supersede the study of constitutional conditions. Let physical signs be taken according to their worth. They inform us of local lesions of structure or of function; but they do not tell us of the relation of those lesions to the body at large, or to other parts of the body: diseased function and condition may be in other parts also; and whether as cause or as effect of the local physical lesion, they are as much as this to be considered in estimating the condition of the patient and in determining the method of treatment. Thus, in the typhoid form of inflammation of the lung, the physical signs scarcely indicate any difference in the condition of the lung from that in common inflammation; but the general signs—the weak thready pulse, the clammy surface, the brown tongue, the oppressed senses, and the extreme general prostration—these show a state of the body of far more importance than the local disease as declared by the physical signs, and in a more imperative degree requiring its treatment.

Again, some morbid conditions which give physical signs are not enough characterized by these alone; but when their constitutional symptoms are also considered, the physical signs then appear in their true light. Thus œdema of the lungs has the same physical signs as the first stage of pneumonia; but the absence of fever, of pain, and the presence of anasarca in other parts, will generally supply what is wanting to distinguish the former from the latter affection.

Further, the positive indications of physical signs, although in themselves accurately detecting disease of an organ, will often be insufficient alone to guide the treatment. Thus organic valvular disease of the heart I may detect by physical signs alone, and I may through them specify with exactitude its nature and extent, but the course of treatment will depend on the general state, the bodily strength, the vascular fulness, the degree of nervous excitability, and the condition of other functions; whilst, at the same time, the organic disease of the heart which the physical signs alone may have discovered, must

also be held in view as a permanent influence to be duly respected in the whole course of the treatment.

Even thus limited, the importance of physical signs will still appear to predominate; for if positive in character, they tell us far more of the real nature and amount of the organic disease than any assemblage of general symptoms; they are more truly the representative of the lesions themselves; in fact they are absolutely part and parcel of these lesions. To illustrate this, a patient with peripneumonia may have severe pain, little pain, or no pain at all—much or little cough; he may be oppressed by dyspnoea, or not be sensible of it at all; his pulse may be soft or hard, and variously quick; there may be thirst, anorexia, heat of skin, and other indications of fever, or they may be scarcely present; and these several symptoms are so far from marking the disease, or bearing proportion to its extent, that they may all exist in a high degree without pneumonia, and pneumonia may be present without enough of these symptoms to attract attention. But pneumonia cannot exist without changing the physical condition of the lung—the physical condition of the lung cannot be changed without an alteration of its physical properties—and these physical properties may, in the majority of cases, be made more or less evident to our senses. We study these physical properties by the senses that can reach them; we watch and feel the motions of the respiratory machine; we listen to the sounds which they produce, and to the sounds which we can excite in them. When we detect in these movements and sounds those modifications that experience and reason have shewn to depend on inflammation and condensation of the pulmonary tissue, we know, almost with the certainty of seeing, that pneumonia is present; and the extent of the signs thus shewn, will be a pretty accurate measure of the extent of the lesion. On the other hand, although there be many general signs which seem to denote the existence of peripneumony, if, on careful examination, none of the physical signs are found, we may well distrust the affirmative general symptoms with regard to the diagnosis, although, if positive, they are not to be disregarded in the general treatment. Both sets of signs have their value; and it will be my especial object, in the course of these lectures, to point out modes of appreciating each in the study and treatment of the diseases—to shew as much as possible what they are severally worth, by examining them more fundamentally than has generally been done—and, whilst we pay due respect to the records of experience, in whatever

shape they may come before us, to make them still more profitable and instructive by careful analysis and moderate generalization.

Now, then, what must be our course to attain this knowledge that shall be at once rational and available? We must imitate the practical philosopher, who wishes to use, repair, or improve a complicated piece of machinery: he studies the springs, the wheels, and all the parts of his machine, their uses and relations to each other, and the laws according to which they act severally and in unison. We must likewise study our machine; we must duly consider its construction—the form, position, and connexion of its several parts; their relations to physical laws, and the combinations of these relations in the rest and in the working of the whole. But in doing this, we shall soon discover that the object of our study is more than a mere machine; we shall find springs in it that are not elastic—chains that are not mechanical—phenomena present themselves which imply properties other than those of mere matter. The laws which regulate these properties beyond those of mere physics, claim also to be carefully studied. We have the *vital* properties, sensibility, irritability, contractility, added to the mechanism—we have a vital chemistry pervading the materials. Besides the chest, which is mechanically enlarged and diminished, and the bag of the lungs and their tubes, which are in like manner expanded and compressed, and the heart and its hydraulic pipes, through which a liquid is propelled, there are in these several parts also the properties of feeling and sympathy, of irritability and the power of secretion; and these not only bind together, in new and complicated relations, the various parts and organs of the chest, but they connect these severally no less with the other organs and members of the body; and the affections resulting from these varied relations, must have their proper share of our attention.

Thus, then, again arises the division of signs into physical and general; the one class confined to the organs and their physical properties, as manifested in these organs; of the other, although a few may be located in the parts, many, through the various vital properties that link its parts together, are expanded over the whole frame; and thus becomes further evident the necessity of duly appreciating the study of both classes of phenomena in the investigation and treatment of disease.

With these objects in view, we proceed, then, to examine briefly the general structure of the chest and its organs, with such a concise account of their functions as

may be necessary to prepare us more fully to understand their diseases. We shall then be able to study the relations of these organs to physical laws, by which their condition may be examined, and the physical signs of health or of disease may be detected. As we enter this subject, it may be necessary to illustrate familiarly a few points of a branch of physical science, acoustics, which is by no means generally understood, and which is yet quite necessary to the right comprehension and appreciation of the phenomena of auscultation.

Now you see that we have chalked out for ourselves no trifling undertaking; and for me to succeed in it, and for you to profit by it, continued attention on your part will be as necessary as in any other study which is progressive and systematic. I have been asked by several, "When do you begin your lectures on the stethoscope? we want to know how to use the stethoscope." Why, gentlemen, the stethoscope stands in the same relation to this course of lectures, as that in which the telescope does to astronomy. To know how to use the stethoscope is as soon learnt as to know how to look through the telescope; but unless your only end be curiosity or wonder, you will use these instruments to no purpose, without studying, and understanding also, the phenomena which they reveal, and the laws which govern these phenomena.

It is only by this fundamental mode of study that we can attain any thing like a complete or really safe knowledge of the physical signs of disease. To burthen the memory with a long and unintelligible list of dogmas, that such and such phenomena indicate the presence of such and such lesions, without endeavouring to instruct the understanding as to the connexion between them, will be as useless and fallacious in practice, as it is unworthy of a branch of science.

The chief advantage of physical signs is in their being *intelligible* and *explicable*; that is, referable to known laws or classes of phenomena. To understand these laws and their fulfilment in the various phenomena of matter, is the object of natural philosophy; and if we expect physical signs to aid us in the study of disease, we must examine them by those modes in which simpler nature and her signs alone become intelligible, in their relation to fixed principles and simple laws. The Hippocratic physician, who studies only general signs, must often content himself with registering them in his memory, because many of these signs depend on vital properties which are little known, and occur according to laws which have been but imperfectly developed; but to extend

this uncertainty and imperfection to *physical signs*, the phenomena of *known laws*, the results of well-defined properties, is to carry into physics the doubt and mystery of medicine, and to deprive pathology of the only advantages of certainty and simplicity which physical science is capable of bringing to it. Vital properties, moreover, also have their laws; and the more we can keep these in view in the study of general signs, the more useful and instructive will be the information which we derive from them.

We have not time to enter into a minute detail of the anatomy of the chest, nor, indeed, is it necessary. I presume that you are already familiar with its structure, and I have now only to advert to the chief constituents of the machinery of respiration, and the mode in which these work.

The machine of respiration was compared by Mayow to a bag within a bellows, the bag alone communicating with the external air, and becoming full or empty as the bellows expands or closes. The comparison must be admitted as only a rough one, for there are many points of material difference. The alternate enlargement and diminution of the chest are certainly the chief movements in respiration; and in proportion as these are complete, and those of the lungs harmonize with them, the mechanical part of respiration will be effectually performed.

Now the chest is, as you know, enlarged essentially by muscular action, but its diminution is chiefly effected by the elasticity and weight of the parts, occasioning their collapse or subsidence from that condition to which the muscular action had brought them. Thus in ordinary breathing, inspiration is active, and expiration in a great measure passive, and the period of action is alternated with a much longer period of rest. As these motions of the respiratory machine constitute an element important in the production of many signs, we may well bestow a few more minutes upon them, especially as they do not seem to be fully understood by some modern pathologists.

Before we describe the means by which these motions are effected, let us illustrate their nature by those of the organs on which they operate. Here are healthy lungs, and observe the character of their expansion when I inflate them: their summits rise, their middle portions expand, and their bases and margins descend and spread. Their motion is, therefore, peripheral, or radiating from a centre: now where is this centre or most fixed point? Why, not at their summits,

as some modern writers have erroneously supposed, but at their roots, where they grow, as it were, from the bifurcated trunk of the trachea, which, where it again divides, becomes in a measure fixed by its attachment to the great blood-vessels and the spine. So also shall we find the enlargement of the chest to be peripheral nearly in all directions, the spine being the fixed centre. Its chief increase is indeed downwards and around its lower portions; but if we watch the upper parts with relation to the spine, we can plainly see them also rise and expand considerably. This general enlargement of the chest may be analysed into its enlargement *downwards, upwards, and outwards.*

The enlargement of the chest *downwards* is, as you know, effected by the action of the diaphragm, which in its passive state projects in a very convex form upwards into the chest, but when acting, its muscular margins and pillars draw its tendinous centre downwards towards their attachments to the lower margins of the ribs and sternum, and the upper lumbar vertebrae. In the same contraction the muscular portions of the diaphragm being straightened towards the centre, touch the ribs at fewer points, and form angular spaces, in which the thin marginal lobes of the lungs become expanded downwards. Now as this downward action of the diaphragm cannot take place without pressing on the contents of the abdomen, it causes an outward movement of these contents, and the swelling of the abdomen at each inspiration therefore becomes an index of this action.

The enlargement of the chest *upwards*, by which the apices of the lungs are expanded, is effected by the drawing upwards of the upper ribs, sternum, clavicles, and scapulae, by the sterno-mastoid, scaleni, and other muscles, which connect this upper frame work of the chest directly or indirectly with the upper end of the spinal column. This motion, although not great in ordinary respiration, is yet in proportion to the extent of the upper lobes of the lungs. It has been overlooked by many physiologists, but it is highly important as a source of diagnosis; and you may perceive and measure it if you keep steady the spinal column by standing with the back against a pole, or the edge of a door, or the corner of a wall; you will then see that the upper ribs rise to the amount of half an inch or more in each inspiration; and so far from being, as Haller and others maintained, almost fixed, they are, in proportion to their length, actually more moveable than the lower ribs. If you examine their attachments to the vertebrae, you will there perceive that the upper ribs admit of more

motion than the lower. Magendie, I believe, first pointed out this important part of the movements of respiration, and you now see how it accords with the expansion of the lung which I have just exhibited to you.

The *outward* enlargement of the chest is mainly effected by the contraction of the intercostal muscles. I dare say that you may have heard or read of the long discussions which were formerly excited by this apparent paradox, that of a cavity *being enlarged by the contraction of its walls.* It is, indeed, a mechanical process hard to be described, and yet how plain and simple it is on inspection! See how these ribs, especially the lower, in their collapsed state, are convex downwards in the portions anterior to their centres. Well, now, these are their most moveable portions, and if they are drawn upwards, the ends being comparatively fixed, you see that their downward convexity is diminished, and their outward convexity or inward concavity is enlarged, and thus the horizontal diameter of the chest is increased.

But observe, for this effect it is necessary that the ribs should be drawn *upwards*; and what is it that makes the contraction of the intercostal muscles draw the ribs upwards rather than downwards? Haller and others say it is because the upper ribs are fixed, and cannot be drawn downwards; but we have just seen that they are not fixed: the fact is that they are drawn upwards themselves by muscular action, and this upward traction is communicated and increased to the ribs below by the contraction of their respective intercostal muscles. If instead of drawing up, or fixing the upper ribs, you draw down, or fix the lower ones by the abdominal muscles, then the intercostal muscles become means of diminishing the diameter of the chest by drawing the ribs downwards; and they actually do contribute to this end in forcible acts of expiration. Now here is a case illustrating the necessity of learning the mechanism of the chest rationally, and not merely by rote. You see that both sets of intercostal muscles, external and internal, must tend to elevate the ribs as long as the upper ribs are either fixed or drawn upwards; but when the lower ones are fixed, the contraction of the intercostals must approach them to this lower fixed point.

Well, now, let us go back a moment, and apply the same question to the diaphragm, whose *ordinary* action we found to be to enlarge the chest downwards by the drawing down of its central convexity. This supposes its tendinous centre to be the most moveable attachment of the muscular portion of the diaphragm, that to

the lower margin of the ribs and sternum being more fixed. But the centre of the diaphragm is sometimes more or less fixed, and prevented from descending, by tumors or excessive tenderness of some of the abdominal viscera below it. How will its muscular margin then act? Why, it will draw upwards its lower attachment with the ribs and sternum, and thus increase the capacity of the chest by raising it from the fixed convexity of the diaphragm. When, therefore, you see a patient breathing merely by the heaving of his chest, you are not to suppose that the diaphragm is inactive, for it may thus contribute to inspiration that is wholly *thoracic*. This upward action of the diaphragm is not considerable in common cases; but it must always be something—that something being the amount of resistance offered to the descent of the diaphragm by the contents and walls of the abdomen.

There is another particular in the ordinary action of the diaphragm which is worthy of your notice, because it may prove a source of physical signs. You see, by these diagrams, that considerable parts of the upper abdominal viscera, the liver, the stomach, and the spleen, although below the diaphragm, yet are contained within the walls of the chest. Now the portions of these walls which contain them differ from those above the diaphragm, in being subjected to the ever-repeated outward pressure of these viscera, pushed by the descending diaphragm; and the result of this pressure is a permanent bulging or prominence in the lower part of the chest, and a slight furrow or depression above it. This depression generally marks the situation where the lung begins, and where the abdominal viscera cease to be in contact with the walls, although they generally rise above it towards the centre of the chest. The existence of this furrow, and its use as a sign of limits, were first pointed out to me by my friend Dr. Edwin Harrison, and I think I may explain its production by the outward pressure of the sub-diaphragmatic viscera. The position of this furrow varies in different subjects, but may generally be traced from the lower end of the sternum running horizontally around the chest, about, but not parallel with, the seventh and eighth ribs at the sides.

The effect of the reiterated outward pressure of the subdiaphragmatic viscera is remarkably seen in the eversion of the lower ribs and sternum in ricketty children, the bones and cartilages being permanently bent by it. If the belly in these cases be also tumid, the upward action of the diaphragm will draw the sternum forwards and upwards, forming what is called a *chicken-breast*.

We need not dwell long on the ordinary means by which the capacity of the chest is diminished. When the diaphragm ceases to contract, the weight of the viscera and walls of the abdomen force back the diaphragm to its wonted vaulted projection into the thorax: the same weight to the lower ribs, together with the elastic torsion of their cartilages and ligaments, make the ribs collapse on the relaxation of the intercostals and the muscles which raise the upper part of the chest. The merely mechanical constitution of the chest is in favour of its diminution, and so is that of the lungs, which we shall notice by and by; but expiration may be most powerfully assisted and increased by a great many muscles, especially the abdominal muscles, and all those connecting any of the ribs with a part of the spine *below* the attachments of these ribs.

So, likewise, forcible inspirations are assisted by the action of all those muscles connecting any of the ribs with any part of the spine *above* the attachments of these ribs.

A BRIEF ACCOUNT
OF
IRREGULARITIES IN THE HUMAN
ARTERIAL SYSTEM,
OBSERVED DURING THE SESSION OF 1836-37.

To the Editor of the Medical Gazette.

SIR,

You will oblige me by inserting the accompanying paper in your widely circulated journal.

Your obedient servant,

HAMILTON LABATT,
Demonstrator of Anatomy, Royal Coll.
of Surgeons, Ireland.

Dublin, Sept. 7, 1837.

On taking a view of the different accounts which have been given of the varieties in the arteries of the human body, it is somewhat remarkable to observe the very few instances in which the state of the arterial system on both sides of the body has been specified. If, for instance, we examine the splendid work by Tiedemann on this subject, we find 19 cases of irregularities in the arteries of the upper extremity delineated, 14 of which were on the right side, and but 5 on the left; no mention, with the exception of two or three examples, is made of the opposite extremities. Now, inasmuch as the arterial distribu-

tion of one side is by no means an index of that of the opposite one, so we must immediately see how desirable it would have been that the state of both had been more frequently alluded to. Thus we should have been enabled to draw some average calculations which might prove useful in a practical as well as a physiological point of view.

Arterial irregularities are generally symmetrical, but we may have the arteries of one side perfectly normal, while the corresponding vessels of the opposite side are abnormal; or lastly, irregular distributions may exist on both sides, yet totally different from each other. A remarkable instance of the last occurred to me during the season, and I procured a drawing of it, in consequence of a peculiar disposition of the thyroid arteries, which, as far as I know, has been hitherto unnoticed. On the *left* side I could not discover a vestige of those vessels; on the right, the superior came off from the carotid at its bifurcation, and the inferior from the first stage of the subclavian; no thyroid axis existing. Next with respect to the mammary arteries, the right proceeded from the subclavian in its third stage, and passing inwards, under the interior scalenus, sought its usual destination. The left mammary arose from the second stage of the subclavian. Lastly, in the same case, the left vertebral passed from the arch of the aorta between the left subclavian and carotid. This irregularity of the left vertebral is by no means uncommon in the human subject. According to Tiedemann it is normal in the *Phoca vitulina*.

Before I pass on to another division I may mention a second remarkable deviation in the thyroids. It was an instance of the *inferior* thyroid being deficient on the left side, and the *superior* on the right. Meekel and Green have recorded examples where there was no inferior thyroid, but I do not find any of their cases corresponding with the above.

With the exception of the case already mentioned, of the vertebral proceeding from the arch of the aorta, I have had an opportunity of noticing but two other instances of irregularities in the vessels proceeding from that great trunk. In both, the left carotid arose from the innominate, and passing across the trachea ascended, as usual, on the left side of that tube. In one of them, the artery

was given off immediately at the origin of the parent trunk, and the unusual size of the vessels in this case particularly attracted me. The diameter of the aorta, found by taking $\frac{1}{3}$ of the circumference, exceeded $1\frac{1}{2}$ inch, and the innominate nearly equalled a regular aorta. The subject was an adult male, and the vascular system throughout the body was developed in an equally remarkable degree. The preparation is preserved in the College Museum, and the drawing in my possession represents the above measurements.

A similar example occurred here in the course of last season, and was brought before the Chirurgical Society by Mr. Williams, who presented the preparation to the School Museum. In his case, however, the left carotid was more connected with the front of the trachea *above* the sternum, so as to interfere with the line of incision practised in tracheotomy. In neither of my cases was it so disposed, being, in both, completely *substernal* in the whole of its transverse course, and not liable to interfere with the success of such an operation as long as the mesial line above the sternum was observed. In one of them the left vena innominate might have been endangered, as it was situated higher than usual. If we consider the above irregularity in another point of view, it will be interesting to remark that it represents the normal type in some animals, as the marmot, guinea-pig, &c.

Connected with the axillary artery, I met with an example nearly similar to one given by Tiedemann, of an irregular branch, in size equalling the ulnar, (analogous in many respects to the profunda femoris,) proceeding from that trunk at the lower border of the lesser pectoral, and, after giving off the subscapular and circumflex, passing between the roots of the median nerve, and running down the arm internal to the main trunk, to terminate in the anastomotic, after having given off the profunda. In this subject, the brachial plexus was quite posterior to the axillary artery.

High bifurcation of the *brachial* artery is of frequent occurrence: according to Cuvier it is normal in the kangaroo, and other marsupial animals; and Tiedemann has observed it in some of the quadrumana, as the *Lemur gracilis*. So frequently is it met with in the human subject, that Dr. Barelay says we can scarcely call it an anomaly. Four

cases, all differing from each other, were noted by me during the season, and I shall briefly describe them from the drawings which I took at the time.

In the first, the bifurcation was about two inches above the bend of the elbow. The ulnar artery, instead of pursuing its deep-seated course, as is usual in the high division, passed superficial to the pronators and flexors, covered only by the palmaris longus, fascia, &c.; and, instead of giving off the interosseal, the radial supplied that artery, which reminds us in some degree of the type in birds, where the radial, after supplying the muscles about the radius, is lost in the interosseal arteries. The second case was similar, except that the ulnar passed immediately under the fascia over all the muscles. In the third, the bifurcation took place immediately after the origin of the superior profunda, and in this example the brachial plexus was posterior to the axillary artery*. The fourth and last case was the most remarkable, and is of rare occurrence. The brachial divided at its origin; the internal or ulnar branch, larger and deeper seated than the external or radial, after giving off the profunda and three small muscular arteries, communicated with the radial by means of a transverse branch, which crossed the tendon of the biceps. An inch and a half below this, the interosseal arteries proceeded separately from the ulnar; the lower one, the anterior interosseal, as large as the radial, gave off a median branch, which, piercing the median nerve, accompanied it down the fore-arm, and, becoming superficial towards the carpus, passed under the annular ligament, to terminate in two muscular branches for the thumb: no superficial palmar arch; digital arteries of index finger and external side of the middle finger supplied by the radial, as it was about to form the deep palmar arch: the other digitals proceeded from the ulnar. This irregularity, with some modifications, has been observed by Tiedemann, Meckel, and Green, and the last remarks that the transverse communication is only to be met with in the high, or axillary, bifurcation. The median branch sometimes comes from the transverse one, and forms the palmar arch with the ulnar†.

Before I leave the upper extremity I may mention a case where the superficial palmar arch was deficient, and the ulnar supplied all the digital arteries, except that of the external side of the index finger, which proceeded from the radial. This variety is by no means frequent; it assimilates the type in birds where there is no palmar arch, and the ulnar is the principal trunk for the wing. The arteries on the left side were regular.

With the exception of one example, I have not taken notes of any varieties in the abdominal arteries. They are of very frequent occurrence, but of no practical importance. In the case of which I have taken a drawing, a second hepatic artery proceeded from the aorta, about an inch below the celiac axis, and passed under the vena porta to reach the right extremity of the transverse fissure, where it supplied a cystic branch. The regular hepatic divided as usual. The above distribution is, I believe, rare.

I shall now describe two very unusual varieties in the obturatrix artery, connected with equally remarkable ones of the epigastric. In the first, of which I have preserved a drawing, the obturatrix arose from the external iliac, above Ponsart's ligament, and coasting along the anterior margin of the crural ring (in the centre of which it gave off at right angles considerably smaller vessel, which proved to be the epigastric,) descended into the pelvis to reach its destination. About a quarter of an inch before its exit from that cavity it gave off the dorsal artery of the penis, which, running along the side of the prostate gland, pierced the triangular ligament a little above the urethra, and sent off the artery of the crus. The pudic of this, the left, side terminated in the artery of the bulb. The vessels on the right side were perfectly regular. I need not here mention how formidable such a case would be for the lateral operation for stone, as also the danger of operating on such a patient for crural hernia.

The above is of exceeding rare occurrence; it is the first instance I have seen of it. I distinguish it particularly from examples of the obturatrix arising from the epigastric, which was to be seen frequently during the season in our dissecting rooms, and must be familiar to every anatomist.

In one of the many cases of the obturatrix proceeding from the epigastric

* This disposition of the nerves I have met with on three occasions during the season. In one, the arteries were regular.

† The above irregularities existed on both sides.

which came under my notice, I remarked a very peculiar course of the epigastric vein. This vessel, of considerable size, passing downwards and outwards at some distance internal to the artery of the same name, crossed the gland in the crural ring (dividing the opening as it were into two) to reach the internal side of the external iliac vein. If a crural hernia occurred in such a subject as this, and if the sac passed between the epigastric and external iliac veins, we should be in danger of dividing in our operation, not only the obturatrix artery, but also a vein rendered of peculiar importance by joining so large a vessel as the external iliac vein. I have preserved a drawing of this.

Besides these varieties I have seen one instance during the season of the epigastric and obturatrix arteries arising from the femoral about half an inch below Poupart's ligament, and it was remarkable that the former, instead of ascending directly from its origin, first formed that arch downwards which it usually does. The obturatrix passed upwards on the pectineus behind the gland in the crural ring, and descended into the pelvis. Meckel states this variety to be very rare, and Tiedemann has seen but two cases of the epigastric proceeding from the femoral.

Although the epigastric artery sometimes varies in its point of origin, I believe it is seldom found to deviate with respect to its relative anatomy. I have seen but one instance when it was not subjacent to the transversalis fascia in its course up the abdomen, and in that remarkable case, which occurred early in the season, it passed through a distinct opening in that fascia immediately after its origin from the iliac, and remained superficial to it in its course upwards and inwards*.

Irregularities of the femoral artery are fortunately of rare occurrence. In the 4th volume of the Dublin Hospital Reports a very interesting case of high division of the femoral and union within the tendinous canal, has been recorded by Dr. Houston, and the preparation may be seen in the College Museum. I have but two cases connected with this vessel to mention. Of the first I have made a preparation for the School Museum. It was an instance of an

unusual size, origin, and, I may add, distribution of the external circumflex, which arose from the femoral under cover of Poupart's ligament, about a quarter of an inch below the circumflexa ilii, and passed down external and parallel to the parent trunk for about four inches; it then bifurcated under the rectus muscle into muscular branches. The proper circumflex vessels came off at its centre. In such a case we would be very liable to mistake the circumflex for the femoral in cutting down for the latter, more especially as the former was quite as large as the profunda, which, it may be well to state, came off from the crural as usual.

In the second, the profunda arose from the femoral about an inch below Poupart's ligament, and ran external to it for about two inches. Besides this, five irregular branches of considerable size come off from the crural, almost equidistant from each other, before it reached its fibrous canal. Here two points occurred for consideration: first, the liability of taking the profunda for the femoral in the operation first alluded to; and secondly, even though we should secure the proper trunk, the danger of secondary hæmorrhage occurring *if the neighbourhood of a considerable branch to the point of ligature can really be the cause of such*. I put the point hypothetically, because I believe considerable doubt now exists on the subject; and I may adduce, as bearing on the question, a case in which the carotid was tied with perfect success by Professor Porter immediately at the starting off of the subclavian. It is most probable that an internal coagulum would not form in such a case, but we have no grounds for concluding that our success should be interfered with in consequence of its absence. It is true that operations have been said to have failed because of the neighbourhood of a large branch to the point of ligature, but when we come to consider the many causes of failure which may escape our notice, it is after all very difficult to settle on the true origin of the failure.

I shall conclude by describing a distribution of the parts in the popliteal space, such as I have not before met with: a very correct drawing of it, which is in my possession, (which I hope soon to place before the profession, with other sketches,) represents the gastrocnemius with three heads; the superadded one

* "L'artère (epigastrique) est constamment placée derrière le fascia transversalis."—*l'écuyer, Surg. Anat.*

arose from a distinct eminence on the posterior part of the femur, between the condyles, by a thick bundle of muscular fibres, and, passing downwards and inwards, distinct for about three inches, joined the internal head. Between these two heads, the artery, vein, and nerve, were closely impacted; the vein nearest the bone, and the artery lying on it covered by the nerve. This case needs no comment; the difficulty which would be experienced in securing the artery, if such was attempted, must be apparent.

With respect to the many varieties which we meet with in the human muscular system, it would be interesting to inquire, how far we find such adopted as normal in other animals.

A REPORT OF

THIRTY-FOUR CASES

OF

PUERPERAL CONVULSIONS.

BY ROBERT LEE, M.D., F.R.S.

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CASE I. —, *æt.* 22; first pregnancy; ninth month; unmarried. Edinburgh, 1816.—Numerous violent fits of convulsion at short intervals, without a return of consciousness, for twelve hours. Orifice of uterus soft and dilatable; but no sign of labour. The pulse was rapid and feeble. The fits continuing, with coma, the child was turned without difficulty and delivered. Death in six hours. Blood-vessels of brain distended. No other morbid appearance observed.—V.S. ad 3l.; head shaved; cold lotion; calomel; enemata; artificial delivery.

CASE II. —, *æt.* 26; first pregnancy; ninth month. 12th July, 1823.—Fifty hours in labour. Head of child low in the pelvis. Vagina and perineum rigid; pulse full and strong; face flushed; occasional incoherence and slight convulsive tremors of the face and extremities. Venesection, followed by two severe fits of convulsion and insensibility. Unsuccessful attempts to deliver with the long forceps. Craniotomy. No fit after delivery. Consciousness soon returned. Uterine inflammation. V.S. 3xviii. Cathartics. Cured.

CASE III.—Mrs. L.—, *æt.* about 26. January 22, 1827. First pregnancy; seventh month.—Eight weeks before delivery suddenly seized with coma, from which she recovered after copious V.S., &c. Headache, giddiness, and partial loss of speech, but consciousness and memory have remained. Slight hemiplegia of the right side. Pulse 90. Went to the full period; labour natural. In a few hours, convulsions, coma, dilated pupils; retention of urine; and she died on the 29th. Upper surfaces of both hemispheres partially coated with a thick firm layer of lymph. Softening of the brain below. The veins distended with firm coagula. Ventricles filled with serum. The ventricles did not collapse after the fluid had flowed out. Copious V.S. and cupping, head shaved, cold lotions, and blisters; cathartics; low diet.

CASE IV. —, *æt.* 20; 1827. First pregnancy; seven and a half months; unmarried.—Had attacks of epilepsy for several years during early life. Headache, drowsiness, loss of memory; paralysis of right inferior extremity after a slight fit of convulsion and coma. Labour natural; child alive. No return of symptoms after delivery. V.S.; cupping; head shaved; cathartics; low diet. Cured.

CASE V. —, *æt.* 24, 21st June, 1828; third or fourth pregnancy; seven and a half months.—Subject to epilepsy in early life. After suffering for several days from an uneasy sensation of weight in the head and giddiness, was suddenly attacked with convulsions, of which she has had several fits, and has little or no consciousness in the short intervals. Os uteri closed. No symptom of labour. 2d June: no fit, but considerable stupor continues. Pulse 80; copious alvine evacuations. 24th: no return of convulsions; and she went to the full period, and was safely delivered of a living child. V.S. 3xx.; head shaved; lotions; enemata; cathartics; V.S. 3xii.; low diet. Cured.

CASE VI. —, *æt.* 18, 24th Jan., 1829; first pregnancy; 9th month.—Delivered at 11 A.M.; labour natural. The expulsion of the placenta was soon followed by a strong fit of convulsion. V.S. was immediately employed. At 4 P.M. frequent severe fits, without any intervals of consciousness; V.S. re-

peated. At 8 P.M. the fits and stupor continued, when forty drops of laudanum were prescribed in my absence.

25th.—Fits continue; twenty drops of laudanum, and sinapisms to the legs, were then ordered by her medical attendant.

26th.—Several fits of convulsion in the course of the night. Has taken sixty drops of laudanum at three doses, which appeared to calm the violent agitation after the paroxysms.

27th, 10 A.M.—Severe and frequent fits during the night; breathing stertorous; pulse rapid and feeble.

Died on the 28th.

I examined the brain, but except a slight turgescence of the blood vessels of the pia mater, no morbid appearance was observed.

V.S. \mathfrak{Lxxxv} .; V.S. \mathfrak{Lxxij} . Head shaved; enemata; calomel; ol. ricini; tinct. opii; sinapisms, &c.

CASE VII.—Mrs. —, æt. 24, first pregnancy; ninth month unmarried. 1828. A weak delicate woman; had several fits of convulsion in the first stage of labour, in consequence of which she was severely bruised in different parts of the body. She was completely insensible in the intervals. The pains ceased, and the os uteri being only partially dilated, and the head of the child being too high in the pelvis for the forceps, the delivery was completed by craniotomy. The fits soon after ceased. V.S. \mathfrak{Lxxxv} .; do. \mathfrak{Lxxij} . Cured.

CASE VIII.—Mrs. H., æt. 24, first pregnancy; ninth month. Sept. 16th, 1828. Constipation and headache for several days; severe fits of convulsion; insensible in the intervals. Pupils dilated; pulse 80, full and strong; face flushed; os uteri slightly dilated; feeble irregular uterine pains. After venesection and free evacuation of the bowels the fits ceased, and she was delivered the next day, without assistance, of a living child; but it died in 30 hours with convulsions. V.S. \mathfrak{Lxxxv} .; hirudines; head shaved; calomel; enemata. Cured.

CASE IX.—Mrs. W., æt. 33, second pregnancy; ninth month. Oct. 6th, 1828. Had convulsions during her former labour. Headache, giddiness, and drowsiness, during the latter months of pregnancy. Venesection recommended, but not employed. In the first stage of labour, during the night, seve-

ral severe fits at short intervals; muscles of left side most affected; face flushed; pupils dilated; pulse rapid, feeble, irregular; os uteri widely dilated; head pressing through the brim of the pelvis.

7th.—Child born alive this morning without artificial assistance. The fits soon after ceased, and consciousness partially returned. Left side slightly paralysed; pupils dilated.

11th.—No return of fits, and the paralysis is gradually disappearing. From this period she recovered the use of the arm and leg.

On the 4th of January, 1829, she died in a fit of convulsion, with which she was seized soon after taking an emetic, without advice.

Serum was found in the ventricles of the brain. A portion of the upper part of the right hemisphere was in a state of complete ramollissement. Both the cortical and medullary parts of the brain were changed into a thin substance like custard. There were tubercles in the lungs.

V.S. \mathfrak{Lxx} .; cupping, \mathfrak{Lxxij} .; calomel, &c.; head shaved.

CASE X.—Mrs. P., æt. about 25; eighth month. Jan. 27th, 1828. After a violent quarrel with her husband, who came home intoxicated, complained of headache and general indisposition. At 7 A.M. seized with strong convulsions, of which she has had several paroxysms. 11 A.M. insensible; tongue lacerated; a bloody foam issues from the mouth; fits continue, with short intervals, when the muscles are affected with spasm. Pulse slow, full, and strong; os uteri dilated; head of child low in the pelvis. During the continuance of the fits the child was expelled without artificial assistance, at 8 P.M. Afterwards had puerperal mania. V.S. \mathfrak{Lxx} .; V.S. \mathfrak{Lxxvij} . Head shaved; stimulating enema; cupping \mathfrak{Lxxij} .; do. \mathfrak{Lxxij} . Cured.

CASE XI.—Mrs. B., æt. 30; ninth month. April 15th, 1829. Headache, vertigo, great depression of spirits, during the seventh and eighth months of pregnancy. Convulsions: hemiplegia of left side took place seventeen days before labour. Labour natural. Died comatose three days afterwards.

Serum in the ventricles of the brain. A small scrofulous tumor adhering to the basilar artery. A portion of the right anterior lobe of the cerebrum softened, and of a yellow colour.

V.S. $\xi xvj.$; cupping, $\xi xvj.$; do. $\xi xij.$; head shaved; lotions, blisters, cathartics, &c.

CASE XII. —, æt. 30; fourth or fifth pregnancy; ninth month. 1829. Violent convulsions, and insensibility in the intervals, for twenty-four hours, without any sign of labour. After repeated copious venesection, &c., and the fits continuing with undiminished violence, Mr. Stone agreed with me in the propriety of artificial delivery, which I immediately performed by turning the child. Considerable time and force were required to dilate the orifice of the uterus, which grasped the neck of the child so firmly after the body and extremities were delivered, that great force was required to extract the head. The fits ceased as soon as the delivery was completed, and she soon recovered. V.S. from $\xi l.$ to $\xi lx.$; cupping; enemata; calomel; artificial delivery; ice in a bladder to the shaved head.

CASE XIII. —, æt. 20; first pregnancy; eighth month. 1829. Delicate and hysterical; headache and giddiness for several days. From twenty to thirty severe fits of convulsion during fifteen hours; insensible in the intervals; pulse 80; face flushed; bowels costive. Labour came on twenty-four hours after the first attack, and a dead child was soon expelled. No fit after delivery. Consciousness did not return for several days. Uterine and crural phlebitis followed. V.S. $\xi xij.$; V.S. $\xi xx.$; head shaved; ice to the scalp; calomel; enemata. Cured.

CASE XIV. —, middle age; first child; near the full period. March 23d, 1829. Labour commenced four hours before the first fit, which was long and severe. Complained of headache and giddiness for several weeks before. Os uteri fully dilated at 10 P.M.; about the half of the head in the cavity of the pelvis. The pains completely ceased, after the convulsions occurred, till 1 A.M. of the following morning, when they returned, and at 2 o'clock a dead child was expelled.

26th. — Partially conscious; no fits. Attacked on the 27th with uterine inflammation, and died in three days.

Body not allowed to be inspected.

V.S. $\xi xxx.$; leeches, $xij.$; head shaved; lotions, enemata, blisters, &c.

CASE XV. —, a young woman; first pregnancy; ninth month. Sept. 1829. Had frequent and violent fits of convulsion soon after the commencement of labour. Four pints of blood had been drawn from the temporal artery by her medical attendant before I saw her. Os uteri slightly open; no pains. Convulsions continued five hours, when a dead child was expelled. No fit after delivery; but she continued comatose, and died soon after. Copious V.S.; head shaved; enemata; calomel.

CASE XVI. —, æt. 25; first pregnancy; ninth month. April 8th, 1830. Hysteria at the age of 15. At the end of the ninth month frequent fits of convulsion in the course of twelve hours. Consciousness returned after venesection. Severe headache, and occasional spasms of the face and extremities. Labour natural. Uncertain if the child was alive. V.S. $\xi xxx.$; calomel, gr. x.; enemata; cathartics; cold to the shaved head. Cured.

CASE XVII. —, æt. about 20; first pregnancy; ninth month; admitted into the St. James's Infirmary, Jan. 1, 1831. Incoherence, followed by convulsions towards the end of the first stage of a protracted labour. Labour pains strong and regular, and the greater part of the head in the cavity of the pelvis. The fits were relieved after venesection, and she was delivered in a few hours of a dead child, without help. V.S. $\xi xvij.$; V.S. $\xi x.$ Cured.

CASE XVIII. —, æt. 30. May 9th, 1832. Had epilepsy when a child. Labour began at 8 A.M., 7th May. Membranes ruptured in the night; os uteri dilated to the size of a crown on the morning of the 8th. Pains feeble; complained of headache; pulse full and slow. Venesection; enema. Labour continued till the morning of the 9th, when severe convulsion fits supervened. Venesection repeated. Fits and unconsciousness continued for several hours, and the pains went entirely off. The head being still high up in the pelvis, and the os uteri rigid and undilated, craniotomy was performed. No fit after delivery. V.S. $\xi xvj.$; enema; cath.; V.S. $\xi xxxv.$; head shaved; cold lotions; enema; craniotomy. Cured.

CASE XIX. — Mrs. B., æt. 30; first pregnancy; ninth month. Autumn,

1831. Six hours in labour, under the care of Mr. Girdwood, of Paddington. At the end of the first stage of a labour, incoherence, stupor, and several slight fits of convulsion. The symptoms were immediately relieved by venesection, the pains continued, and a living child was soon expelled. V.S. \S xxxvj. Cured.

CASE XX. —, age not ascertained; patient in St. Marylebone Infirmary. December 1831. Had a number of severe fits of convulsion soon after the commencement of labour. No relief was obtained from venesection; and the pains having entirely ceased for many hours, and the head of the child being above the brim of the pelvis, and the os uteri only partially dilated, craniotomy was performed. Only one slight fit occurred after delivery, and consciousness returned gradually. Copious V.S. Cured.

CASE XXI. —, middle age, October 1833, first pregnancy. Had been long in labour, when convulsions came on, without any complaint of headache. A feeble child, born alive. Convulsions ceased immediately after delivery. V.S. \S xxx. Cured.

CASE XXII. —, æt. 20, 30th December; first pregnancy; unmarried. Was called to see a patient, æt. 20, in an adjoining parochial infirmary, who had been attacked with furious fits of convulsion sixteen hours after the commencement of labour. Os uteri fully dilated; head of child jammed in the brim of the pelvis. An ear could not be felt. Fits continued still more frequent and violent after V.S. Pulse rapid and feeble. Labour pains have entirely ceased. Head perforated, and great force required to draw it through the pelvis. No fit after delivery. Sensibility returned the day after. V.S. \S xxx. Craniotomy. Cured.

CASE XXIII. — Kirby, æt. 30, St. Marylebone Infirmary, May 23, 1834. Second child. Pelvis distorted by rickets. Delivered by craniotomy two years before, after a tedious labour. Two strong fits of convulsion took place on the 23d May, when she had been six hours in labour. Fætal head above the brim of the pelvis. Meconium passing. Uterine contractions incessant. Abdomen tense, hard, and painful. Craniotomy. No fits after. V.S. \S xii. Cured.

CASE XXIV. — Mrs. G —, æt. 28, Feb. 25, 1833. First pregnancy, 9th month. After eating roasted pork for dinner and supper, was seized with vomiting, convulsions, and insensibility, at 3 A.M.; after V.S. and an enema the fits became slighter; the pulse extremely rapid and feeble. The fits, however, returned occasionally till 10 A.M., when labour-pains came on. At 1 A.M. a dead child was expelled. Fits and insensibility continued four hours after.

26th. — The fits had ceased and consciousness had returned, though imperfectly. Retention of urine. She died five days after, with symptoms of uterine inflammation. Body not allowed to be opened. V.S. \S xx. head shaved; enmata, &c.

CASE XXV. — A young woman. St. Marylebone Infirmary, July 5, 1833. First pregnancy. Delirium and slight convulsions came on suddenly, after the labour had lasted upwards of 24 hours. Vagina rigid, hot, and tender. Os uteri not fully dilated. V.S. procured no relief. The head being beyond the reach of the forceps, the operation of craniotomy was performed. The fits immediately ceased. Consciousness was not perfectly restored for several days. Copious V.S. Craniotomy. Cured.

CASE XXVI. — A young woman, delivered at 3 A.M., on the 20th May, 1828. Several fits of convulsion soon after, of no great violence.

1 P.M. — Fits have ceased; consciousness partially restored; appears heavy and oppressed, and complains of headache. Pulse 50, full and strong. V.S.

10 P.M. — No return of convulsions. V.S. \S xxv. Calomel, gr. vi. Haust. Sennæ. Cured.

CASE XXVII. — In December 1829, I examined the body of a woman who had died after puerperal convulsions. She was not seen by me during life. Insensibility and convulsions came on during labour, which was protracted. The pulse was stated to have been rapid and feeble. Delivery was completed by craniotomy, and she died comatose three days after. A table-spoonful of serum was found, on inspection after death, at the base of the brain. The pia mater around the tuber annulare vascular. Brain healthy.

CASE XXVIII. — Mrs. M —, æt. 28,

was suddenly attacked with convulsions, eight days after a natural labour. She had ten severe fits in less than two hours. In the intervals she was completely insensible, with stertorous breathing; dilated pupils; the pulse 110, feeble. The fits went off in a few hours, but she remained for several days in a drowsy confused state. The attack followed the use of very indigestible food. Has since been twice confined, and had no convulsions. Cupping 3xii. Calomel; Cathart. enema. Head shaved. Blister. Cured.

CASE XXIX.—Mrs. P——, æt. 26, April 1835. First pregnancy, full period. Returned home after midnight, from a large dinner party, at which she had partaken of a variety of dishes and wines, and had been seated near a large fire. Labour came on at 4 A.M., and soon after she became incoherent, and said she felt her teeth falling out of her head. On attempting to drink some warm tea, she bit a large piece from the edge of the china cup, and crushed it between her teeth. Convulsions of great violence immediately followed. Copious V. S. and an enema gave no relief. In an hour and a half the head of the child was within reach of the forceps, and it was applied, and the child was soon extracted alive. She died at 11 A.M. The perineum was extensively lacerated, from the impossibility of retaining her for an instant in the same position. The child was known to be alive before delivery, from the cord being around the neck and felt pulsating. Body not allowed to be examined. V. S. 3xxx.; V. S. 3xii. Enema. Head shaved. Delivered with the forceps.

CASE XXX. —, æt. 18, unmarried; first pregnancy; end of eighth month. March 3, 1835, St. Marylebone Infirmary, 11 A.M. Has had seven fits of convulsion. Neck and face swollen. Os uteri fully dilated. Face presentation. Pains have nearly ceased. An attempt was made to deliver with the forceps, but it failed, from the impossibility of keeping her steady.

3 P.M.—Fits continuing with undiminished severity, the operation of craniotomy was performed.

4 P.M.—Four fits since delivery, from which she was greatly exhausted. Forty drops of liquor opii sedat. administered, after which the fits became much slighter. The dose was repeated

several times, and the fits gradually went off. V. S. 3xxx.; V. S. 3x. Cured.

CASE XXXI.—A lady, about 26 years of age, who had been in labour with her first child for no long period, was seized with convulsions, for which V. S. was immediately resorted to by her medical attendant. The fits continued with violence till the head was pressing upon the perineum, and it was resolved in consultation to deliver with the forceps. While placing the patient in the proper position for performing the operation, the child was expelled alive by the natural efforts, with the funis round its neck. The fits instantly ceased, but she remained in a state of stupor for ten hours. V. S. 3xxx. Head shaved; enema; cold lotions; cathartics. Cured.

CASE XXXII. — August 1836. A young woman in the St. Marylebone Infirmary had fourteen fits of convulsion in the first stage of labour. It was her first pregnancy. Copious V. S. was employed without effect. I delivered her with the forceps. The child was dead. She had only one slight fit afterwards. Cured.

CASE XXXIII.—Mrs. A——, æt. 25; eighth month; fourth pregnancy. Aug. 17, 1836. Yesterday (the 16th) dined on currie and rice, and ate bacon and eggs at tea.

17th.—Awoke at one o'clock in the morning with violent pain in the back part of the head and sickness, for which she took a strong cathartic. A physician was called to her soon after, and ordered five grains of calomel and an antispasmodic draught, which relieved the symptoms. During the forenoon she remained in a drowsy state without complaining. At mid-day a fit of convulsion occurred. At 3 P.M. another and more violent fit followed. I saw her soon after this. The pulse was extremely rapid and feeble, and it became altogether imperceptible at the wrist on the abstraction of eight ounces of blood from the arm. More blood would not flow from a large orifice in the vein. Orifice of the uterus slightly open; labour pains commencing. Membranes ruptured artificially, and liquor amnii discharged. An hour after, four ounces of blood were removed from the temple by cupping, when the pulse again became imperceptible. At 6 P.M., the os uteri being dilated, and the head in the

pelvis, I delivered with the forceps. The child was dead. The fits continued, and she died at 8 p.m. V. S. 5viii. Cal. gr. x.; Haust. Antip. Cal. gr. xv.; Cupping, 4 ounces. Head shaved. Eucema.

CASE XXXIV. — Mrs. P —, æt. 35, a widow, in the eighth month of her third pregnancy. For fourteen days had influenza and severe headache. At 1 p.m., 8th February, 1837, attacked with convulsions. At 8 p.m. she had had 16 severe fits. V. S. had been employed, and the hair cut off. At 9 p.m., pulse 110, and feeble; hands and feet cold; stertorous breathing. When the fits occurred, the muscles on the right side of the body became first affected. In about a minute the spasms left this side, and the muscles of the opposite side became affected. Pupils dilated. Membranes ruptured. At midnight the fits continued, and the head not being sufficiently advanced for the forceps, she was delivered by craniotomy. Only one slight fit occurred after delivery, and consciousness was restored in the course of the day. V. S. 5xl. Head shaved. Calomel. Encmata. Cured.

THE PURULENT OPHTHALMIA OF INFANTS OF TWO SPECIES.

To the Editor of the Medical Gazette.

SIR,

THE accurate discrimination of species between which there is the greatest resemblance, is one of the achievements of the naturalist which tends most powerfully to promote the advancement of knowledge. Medicine should be considered as a branch of natural history rather than a science; and the distinction would not be without its value, if it led us to imitate the minute and patient observations of botanists in separating allied species, grouping them as new relations are brought to light, and thus step by step advance to general principles; rather than to emulate Newton, and the cultivators of the physical sciences, and mislead ourselves by the hopes of generalizing all our knowledge into one universal fact. Some,

perhaps, may fear that such minute distinctions as the following would tend to overwhelm us with the multitude of species; but the entomologist is not embarrassed by the hundreds of thousands in his department, nor can we help ourselves if nature will not be restricted to our convenience.

In the well-known affection of infants—the purulent ophthalmia—there are reasons which seem to establish the existence of two distinct species, the due discrimination of which has its value:—1. the one depending upon common causes—cold, the presence of some irritating matter insinuated into the eyes during the process of birth, feces, urine, vaginal discharges, &c.; 2. the other arising from the presence of gonorrhœal matter.

The former is of less severity, and most practitioners will agree that it is usually easily and effectually cured by various means. For the most part stimulating collyria of various kinds are used, some preferring one thing, others another. There is no better or more rapidly efficient means, than the old formula of sulphate of copper and bole armeniac, in camphor mixture, termed Bates's lotion.

But not unfrequently a form of the disease is met with of greater intensity, where the matter secreted by the inflamed surface is in immense quantities, the swelling of the lids considerable, and the pain very severe: such cases often resist the above means; and during the attempt to check it, some permanent mischief is done to the eye. There is another serious evil to be apprehended in these cases—the communication of the infection to others of the family, unless obviated by the almost continual removal of the matter, and the very careful separation of other children from the one infected.

When the symptoms of this disease set in with unusual severity, or when our usual means are not speedily answered with success, we may suspect a specific infection; then, and when we know the parents have been infected, more active means must be at once resorted to for its cure. A solution of nitrate of silver, varying in strength from five to ten grains to the ounce of water, according to the severity of the case, will be required. This should be dropped into the eye, and care taken to bring it into contact with the whole sur-

face. It is best to be done by the practitioner himself, or his assistant, twice a day, with strict injunctions to the parents to use very frequent ablutions. If we get the case after it has existed some time, pencil the inner surface of the lids over with the solid caustic, and it will more speedily stop the disease.

This, too, is a known remedy; but I doubt if it be rightly apprehended how generally severe cases are gonorrhoeal, and how much more efficient the nitrate of silver is in these cases than any other remedy. These, however, I think I can confidently affirm, are facts, whatever may be their value. With this impression, the late communications of Dr. Hannay to the *MEDICAL GAZETTE* had a greater interest to me than they would have had if they had stood alone, valuable as they unquestionably are in themselves; and some of your readers may, perchance (when these ideas are suggested), feel the same.—I am sir,

Your obedient servant,

JOHN GARDNER.

49, Great Portland-Street,
Sept. 23, 1837.

EDUCATION OF THE APOTHECARY, IN ANSWER TO DR. KERRISON'S LETTER.

To the Editor of the Medical Gazette.

SIR,

HAVING been for many years a subscriber to your valuable journal, you will, I trust, oblige me by inserting this letter. Dr. Kerrison, in his able letter, has, I fear, not sufficiently considered the degree of attainment necessary in the general practitioner. The Doctor in one part observes, "But it has been doubted whether the routine of study required of candidates for examination has not been too extensive: it might be urged with some plausibility, that the life and health of a poor man are of as much value to his family as those of a rich man, and that, consequently, the qualification of the medical adviser for the poor ought to be as high as that for the rich;" and continues, "the temporal affairs of a man in moderate circumstances may require the judgment of a barrister, yet he must be content to avail himself of the services of a solicitor or attorney, on general occasions, and take the barrister as a referee in cases of importance.

"The spiritual wants of a poor man may require the learning of a bishop, but he must be satisfied with the consolations of religion administered by a curate. The sick poor (not parochial paupers,) and persons in moderate circumstances, may require the judgment of a physician, or skill of an hospital surgeon; but they must be satisfied with the attendance of a gentleman who administers relief in cases of disturbed health and accidental injuries, from which he is called a general practitioner."

Now I consider, Mr. Editor, there cannot be two opinions as to the extent to which the education of the general practitioner ought to be carried: his services are frequently required in cases where the highest skill is necessary, and in which, to delay until a physician or hospital surgeon could be procured, would be highly dangerous to his patient. Any reasonable man who considers this subject, will see directly that it is to the interest of the rich as much as the poor, that the education of the general practitioner should be of the highest stamp, as he is in most cases the first called in, and is expected to be able to treat competently every case.

The Doctor's letter not being intended to provoke controversy, I will here conclude, and have the honour to be, sir,

Your obedient servant,

EDWARD NORTON.

16, Gloucester Place,
Aug. 28, 1837.

DEFENCE OF ANIMAL MAGNETISM.

To the Editor of the Medical Gazette.

SIR,

YOUR journal of the 16th instant, contains an article on Animal Magnetism, in which I am made the object of a personal attack (although my name is suppressed,) and the discovery which I am endeavouring to propagate in this country is represented as a downright imposture. I have been told that, in England, the love of justice is as strong as that of liberty; if this be true, I have every reason to believe that you will not refuse to insert this letter containing my justification.

Accustomed as I am never to flinch

before the opinions of men, but to yield to truth only, the entire press, with its thousand engines of publicity, the whole scientific world, armed with the most plausible and judicious arguments, could never convince me that I am now in error; for, during twenty consecutive years, I myself have witnessed, on more than 6,000 patients, the very facts which are now formally denied.

As my pen alone would be utterly inadequate to the task of defending my name against the powerful attacks of able writers, I shall employ the only means within my reach; I shall exhibit before the multitude the identical facts which are now contested by the learned, and teach every one willing to learn, the right method of obtaining similar results.

I shall prove that the magnetic influence is the more powerful as the individual submitted to its action is less conscious of his being operated upon; and that every human being, when in his natural sleep, is sensible to the magnetic power directed upon him at a distance of several feet.

The opponents of magnetism have found out a word which explains every phenomenon, and consequently saves them the trouble of investigating the subject, *imagination*, a sounding word which only proves the emptiness of their intellect; for, if imagination could heal the sick, it would be their duty to avail themselves of this means. Again, if imagination could produce the extraordinary, not to say wonderful results attributed to magnetism, they should carefully study its powers, since the knowledge of what man is cannot be obtained by any other means.

They shout victory because one magnetizer in a thousand did not succeed in his attempt to prove what he had asserted; but he will be followed by hosts of others who will be more successful, and possess more perfect instruments. They will know better than M. Berna the causes which prevented the success of that gentleman's experiments, and it is those causes which we propose to make known without any reserve.

Science, at the present day, is grown so powerful and omniscient, that no fact can possibly escape her searching scrutiny; but, notwithstanding, we shall claim her aid. We shall proclaim in the face of the world that such

men as Lavoisier, Bailly, Franklin, and a host of scientific men next in merit, unanimously pronounced their verdict against animal magnetism, and yet that species of magnetism does nevertheless exist.

Ye shades of Christopher Columbus, Galileo, Harvey, Jenner!—ye also were condemned; and yet your discoveries still survive, and your names are as imperishable as that of Mesmer is destined to be. What matters it to you if you were misrepresented for a time, when immortality was to be your reward?

The cup of sorrow administered to the man who discovers an important truth, tempers his soul like steel, and only serves to prepare for his brow the crown of immortality.

When men can cope with error and conquer it, humanity has cause to rejoice; but when the learned swerve from the paths of truth, we should not pity, but desecrate them, for the evil they create is beyond all calculation.

As for me, being as fully convinced of the reality of animal magnetism as I am of my own existence, I shall accomplish the task I have undertaken by promulgating the discovery of Mesmer, and spreading the knowledge of its important benefits. I should be wanting in the duty I owe to myself and to truth, should I now neglect answering the virulent attacks which have been levelled at the science I profess, and the public may rest assured that my answer shall suffer no delay. At a future period, which is not very remote, it shall be my turn to attack the adversaries of magnetism, who are well known to me. I shall then expose the secret motives which actuate them; and those enlightened and unprejudiced men who have no private interest in the grand question at issue, shall pronounce their verdict. From this present day, however, I invite all honest and impartial sceptics to come to my residence, and there witness the phenomena produced by me, and watch the progress of the medical treatment already begun on a variety of nervous patients.—I am, sir,

Your obedient servant,

BARON DU POTET DE SUMEROY.

25, Orchard-street,
Portman Square, Sept. 21, 1837.

[We have thus inserted the letter of

M. du Potet; and in answer to his remarks, have only to refer him back to our article which called them forth, and which he has left wholly untouched.

We perceive that a verbatim copy of the letter was also sent to the *Globe* newspaper, the Editor of which states that he gave it insertion for "*amusement.*" We trust the worthy Baron duly appreciates the compliment. — *Ed. Gaz.*]

MEDICAL GAZETTE.

Saturday, Sept. 30, 1837.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

A PROSPECT AND A RETROSPECT.

WHEN we published the last number we had completed our tenth year, and the twentieth volume of this journal. Our readers will, we hope, bear with us while, before entering upon another period, we cast a rapid glance back on our past course, and some of the events that have occurred in its progress.

When the publication of this journal was commenced, it seemed to many impossible that a weekly journal, conducted on other principles than those involving the destruction of existing institutions, and without personal rancour, could exist for a single year. Although we thought otherwise, we were still not so confident in our opinion, but that, in our original address, when speaking of some seemingly popular practices which we expressed our determination not to imitate, we said — "Whether our experiment will succeed, time only can tell."

Time has told what we can now with the greatest satisfaction repeat to our readers—that the *experiment* has succeeded completely. Without one check,

without the slightest vacillation, the circulation of the *MEDICAL GAZETTE* has regularly and steadily increased, from the first year of its publication; and this, be it remembered, while in the most unwearied manner we have adhered to the principles and plans with which we commenced our career; not wantonly attacking any, yet never withholding the honest expression of our opinions on public men and measures, when they appeared to us to require it; nor ever relaxing in our efforts to be really useful to our profession at large.

Our prosperity is a strong presumptive proof in favour of the means we have employed to obtain it; and such a result will always, we believe, attend such means. Others may thrive by following a very different course. But at this who will complain? Not we. With gratitude for our past prosperity, we shall *repeat the experiment*, on the same plan, and with similar materials. We have now the useful experience of ten years to guide us; the surrounding atmosphere, too, is far more favourable than when we commenced our task: the storms which we had then to encounter have lulled into what might seem by comparison a perfect calm; the scorching and rancorous heat has given way to a more mild and equable temperature. *Deo volente*, we hold it certain that the success of our second decennial period will even surpass that of the first.

In proof that we had striven to deserve success, we may briefly enumerate some of the most important matters which we have published, as contributions to the stores of medical science.

Of systematic courses of lectures, we have given those on Surgery, by Mr. Lawrence—on Medicine, by Dr. Elliotson—on Midwifery, by Dr. Ramsbotham—on *Materia Medica*, by Mr. Pereira—on Forensic Medicine, by Pro-

fessor Amos and Dr. Cummin—the latter alone remaining as yet incomplete.

Of Clinical Lectures, we have published several series by Sir Charles Bell and Sir B. Brodie, and numerous others by Mr. Arnott, Mr. B. Cooper, M. Dupuytren, Mr. Earle, Dr. Graves, Mr. Guthrie, Mr. C. Hawkins, Mr. Key, Dr. Latham, Dr. Mackenzie, Dr. Seymour, Mr. Travers, and Dr. Watson.

Among the shorter courses of Lectures, or Essays, will be found those of M. Andral on the Brain, Mr. Bacoet on Syphilis, Baron Heurteloup on Lithotriety, Dr. Birkbeck on Mechanics, Messrs. Brett and Bird on Urinary Concretions, &c., Dr. Bright on Abdominal Diseases, Dr. Buchanan on the Animal Fluids, Dr. G. Burrows on the Pathology of the Blood, &c., Dr. Chambers on Fever, Dr. Davies on Diseases of the Chest, Mr. J. W. Earle on Inflammation, Sir H. Hallford's College Orations, Dr. Bisset Hawkins on Medical Statistics, Dr. F. Hawkins on the Brain, Dr. Hope on Diseases of the Thoracic Viscera, Dr. Hugh Ley on the Pathology of the Nerves, M. Magendie on Physiology, Mr. Middlemore on Diseases of the Eye, Mr. Mayo on Morbid Anatomy, Dr. Wilson Philip on Digestion, on the Use of Mercury, and various other subjects, Dr. Prout on the Medical Applications of Chemistry, Dr. Rigby's Obstetrical Reports, Dr. Roget on Sensation and Perception, Dr. Roupell on General Pathology, Dr. Seymour on the Ovaria, Mr. Stanley on Diseases of Bone, Dr. Williams on Animal Heat.

These gentlemen have all, too, been contributors to our pages on various subjects not connected with their lectures; and among a crowd of more or less frequent correspondents, it may be sufficient, as a sample taken from memory, to mention Dr. R. Lee, Mr. Swan,

Dr. Gregory, Mr. Copland Hutchison, Mr. Owen, Mr. Shaw, Dr. Clendinning, Dr. Marshall Hall, Dr. Baron, Mr. Robertson, Dr. Hamilton. In short, it would scarcely be easy to find a name of repute, either in the metropolis or provincial towns, that might not be placed in one of these lists.

Surely it will be granted, then, that we have fulfilled the promises that have at various times been made in regard to this portion of our plan, and have at the same time afforded some important assistance to medical science. For it will be observed that many (of the lectures for example), which are mentioned above, have been, as it were, snatched from most unmerited oblivion; because, in some instances, they are the works of those who have never published at all in any other form. Many others, it will be seen, are the first fruits of the labours upon which have since been formed some of the most esteemed and standard works in modern medical literature.

One other remark we may make here. On looking over the list of names given above, it is impossible not to be struck with the fact that there is scarcely one among them whose possessor has not increased in reputation since the appearance of his works in our pages. We see the names of many there, who, when they were first contributors to our journal, were scarcely known; some who were only "rising men," are now placed at the very highest elevation in their profession, and most of the others are rapidly following them in their successful careers. We are not so absurd as to claim to ourselves the merit which belongs to them; but the more rapid progress of some has so immediately followed the publication of their early works here, as at least to show that the seed had not fallen on an unpropitious soil.

Among the subjects more immediately belonging to our own department, and which have been handled during the last year, we flatter ourselves that the exposure we made of the filthy puerilities and incalculable absurdities of homeopathy, had considerable effect in extinguishing that folly in this country; and we have recently put a spoke in the wheel of those who would roll themselves into notoriety by means of animal magnetism. To quackery in every shape, it must be acknowledged even by those who differ from us in politics, that we have ever been determined and active opponents; while we have exerted ourselves, and we trust not unsuccessfully, to support the welfare of the profession at large. We may on this point more particularly instance our exertions in connexion with the Poor Laws, which so severely affect the interests of the general practitioner. A very large number of our articles on these and other subjects have been deemed of sufficient interest and importance to be copied into the public papers.

The limits of the present article would be insufficient to give even a sketch of the progress which medical science has made in the last ten years; but on looking through our pages, we are forcibly struck with the idea that, in no period of the history of scientific medicine, could any equal length of time be found during which so many and so valuable individual facts were collected and recorded, and yet so few great discoveries or generalizations achieved. And it must be allowed, that in the present condition of medicine, this is by far the most satisfactory state. Till lately, the department of medical literature rendered most conspicuous by the talent and reputation of its authors, consisted of systems almost as theoretical as they professed to be complete; and the facts which they contained, collected solely with reference to one or

other of the theories, were what Cullen called all but those that served his own—"false facts." Hence the majority of the great discoveries in medicine were accidental, and, with but few honourable exceptions, the records of facts could not safely be made use of for the establishment of sound generalization. The history of medicine only agrees with that of other sciences in showing a deficiency of positive evidence to be an abundant source of theories. It is remarkable that the error of the system was not earlier discovered, and that the last ten years should have been the first to witness the prevalent existence of a spirit tending to correct it.

The prominent character of the medical records of this period has been the complete detail of facts which they afford. Physiologists have attended comparatively little to life, to vital principles, to nervous influences, and other such subjects, which not long since occupied unprofitably their chief attention; and proximate causes, nosologies, and *modus operandi*, are comparatively obsolete terms: all are now engaged in collecting actual and indisputable facts. Already a vast body of evidence in all branches of medicine is accumulated, and though the labour may be less agreeable than the more exciting and self-complacent occupation of former days, the workmen are already receiving their reward in the establishment of important and well-determined results: if persevered in steadily for but a few years more, it cannot be doubted but that medicine will by this plan be placed among the more certain sciences, and that on the best foundation of inductive reasoning, a number of general principles of the highest importance in their application to practice will be attained. Every student may be happy that he has fallen on what will in future ages be called the spring-time of the philosophy of medicine; and in re-

turn for the benefits which he derives from the improved system of its study, let him contribute in the same spirit his share of the evidence which alone is now necessary to secure the advance of the science.

Passing from medical science to another subject which has occupied the remainder of our pages—medical politics and medical institutions—we have no less cause for satisfaction. It was predicted that if we did not fall by our own weakness, we should soon be buried under the ruins of the institutions which we supported, and with which some of our principal contributors were connected: but this has been as false a prophecy as the rest. The great and only important innovation is the establishment of a New University, which it has occasionally been imagined might exercise an injurious influence on the older institutions. This, however, is now no longer probable, and we shall only further remark, that while we, in common with many others, strongly advocated the expediency of a University in London, yet, so far does the present one differ in almost every feature from that which the original advocates of the change had proposed, that like us they have silently, but almost entirely, withdrawn their support. We still, however, live in hopes of seeing what appear to us to be its blemishes removed, as we do to certain vices in the older establishments being thoroughly reformed. It would be the height of injustice, however, not to admit that the College of Surgeons has made great and rapid strides in the course of utility and improvement.

In medical education, the only important alteration has been the increased amount of study demanded by the Apothecaries' Company—a change of very doubtful utility; but on this point we expressed our opinions in a very recent number.

Amid this gratifying retrospect, one thought alone occurs to mar our satisfaction: how many has this same period removed from their scene of usefulness! Abernethy, Lynn, Babington, Blizard, Maton, Warren, Tuthill, Pearson, Gooch, Ley, Darwall, from our brethren and familiar friends; and then again, Wollaston, Davy, Young, Turner; and abroad, Scarpa, Dupuytren, Boyer, Hufeland, Rudolphi, Portal, Gall, Spurzheim, and Lobstein. When we look back on the past, which from the other points of view seems lengthened by the number of events which are crowded in it, it seems in this aspect but as a day; all these seemed to have "gone in a moment,"—and they are but a few, and only such as were known and esteemed by the world; but how many thousands more have passed from the stage on which, though it were smaller, they shone with equal lustre, and where they have left not less endearing memorials.

But in the household of science it is the children alone that are mortal. Increasing age sees the mother only more prolific, and the offspring of each succeeding year more vigorous and worthy of their parent. Journalists, her and their servants, so long as they are faithful to their duty and to science, share the perpetuity of the mother; and though with her they may lament the loss of each of her departed sons, it becomes them to be no less anxious for the welfare of the present than of the past. For ourselves, as we have done in times gone by, so shall we continue to do in those which are to come—still holding to our old motto—"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

MR. PEREIRA'S LECTURES.

THIS very valuable course of lectures on *Materia Medica* and *Therapeutics*, was concluded in our last number. Although they have been contained in seventy-four numbers of the *GAZETTE*, yet as in many instances two or more lectures were given, extending through ten or twelve pages, the actual amount of the course exceeds 100 lectures, each occupying between six and seven pages of the *GAZETTE*, and illustrated by above two hundred woodcuts. They thus form by far the most complete system of *Materia Medica* ever published in this country.

BRITISH ASSOCIATION FOR THE
ADVANCEMENT OF SCIENCE*.

WE gave a short account last week of the papers connected with medical science, read at the late meeting at Liverpool. Elaborate reports of all the proceedings have been published in several papers, and we shall therefore content ourselves with giving a sort of catalogue *raisonné* of the chief contributions connected with medicine.

Tuesday, September 12th,

Dr. BLACK read an essay on the *Epidemic Influenza*, as it occurred in January, February, and March, 1837. This paper chiefly referred to the rate of mortality, and the meteorological condition of the atmosphere, for which purposes several elaborate tables were exhibited. It appeared, in comparing the mortality of the period above mentioned, with the corresponding months of previous years, that the increase amounted to 45 per cent., and taking the month of February alone, to the prodigious extent of 160 per cent. Dr. Black's observations fully confirm those previously made, of the very heavy manner in which the epidemic bore upon those advanced in life: and he referred particularly to the reports published in this journal, by Drs. Clendinning, Heberden, and Graves.

* By rather a ridiculous oversight, the account of the meeting of this institution in our last number, was headed "British Medical Association," whereas the club, which rejoices in this name, is one of a totally different nature, and having totally different objects.

Wednesday, September 13th.

Sir JAMES MURRAY. — A paper, the object of which was to prove that various diseases of the nervous system arose from the presence of certain salts in the blood giving rise to crystalline deposits, such salts being those which are usually voided in the urine. Such depositions he has found on the nerves in *tie-douloureux*, and has also detected uric acid in the discharge from ulcers in *tinea capitis*, and some other cutaneous affections. Our readers will remember that M. Gluge has recently described microscopic crystals in the evacuations of persons labouring under fever (see last volume, p. 246.)

Dr. MACINTOSH made some observations on *Dysmenorrhœa*, which he endeavoured to shew originated in a peculiar condition of the os uteri—viz. some degree of stricture, with thickening and the deposition of false membrane, and accumulation of mucous discharge in the uterus itself. He stated that he had frequently seen great benefit result from the application of leeches to the os uteri.

Dr. MADDEN — Experiments on the *Connexion between Nerves and Muscles*, the result of which he regards as shewing that narcotics do not always produce any obvious effect upon contractile tissues; that sedatives, when applied to nerves, are absolutely inert, if such application be rigidly confined to the nerve; and that muscles retain their irritability long after the nerves have lost their power of exciting them.

Dr. BELLINGHAM on the *Motions of the Heart*, with especial reference to the order of their succession, which he holds to be as follows:—1st, the systole of the auricles; 2d, diastole of the ventricles, and the simultaneous impulse; 3d, the systole of the ventricles; 4th, an interval of repose, followed by the systole of the auricles. This order is different from that given by Dr. Hope.

Sept. 14, 1837.

A report was read by Dr. CARSON, on the *Composition of the Animal Secretions*, giving the results obtained by a provisional Committee appointed to examine the subject.

Mr. HARE read a paper on *Curvature of the Spine*, particularly the lateral, which he attributes to the present mode of dress—in a word, to tight stays. The means proposed by Mr. Hare consist of

an inclined plane, with weights and pulleys, similar to those which have been so much used during the last ten or twelve years.

Dr. HOLLAND, of Sheffield*, communicated the result of some investigations regarding the *Influence of the Mind on the Heart*. This paper chiefly had in view the influence of respiration on the circulation, the former of which he stated to be greatly under the control of emotions which secondarily affected the circulation. The positions were illustrated by various arguments.

Dr. MACINTOSH communicated some observations (which were understood to be from the pen of a medical student), on the *Diseases of Stonecutters* about Edinburgh. In one case, which was particularly detailed, the patient having died after cough and other pectoral symptoms, of two years' duration, hard black tubercles were found in both lungs; the bronchial glands enlarged and indurated, grating under the knife, and containing portions of substance like bone, which, being analysed, yielded carbonate of lime, silica, and albumen. The stone of the quarry where he had chiefly worked was also examined, and found to consist of carbonate of lime, silica, and alumina; from which it is inferred by the author that the deposition in the lungs was derived from the stone, and imbibed during inspiration.

Sir JAMES MURRAY made a demonstration of a machine for forming a kind of *Bath in Vacuo*, by which means the surface of the body, even when cold and collapsed, soon became warm and turgid. It admitted of being used locally, as to a paralysed arm, &c.

Two cases of *Malformation of the Brain* were communicated by Dr. CARLISLE, and some remarks on the crania of a tribe of Indians in North America, by Dr. WARREN, of the United States.

Dr. MACINTOSH on the *Morbid Anatomy of Cholera*.—He observed, that he had made 300 dissections of patients who had died of this disease. He found the capillary system generally distended with blood—the brain loaded—the bones themselves vascular; the ganglia healthy, but vascular; the heart and its vessels turgid; false membranes lining the arteries, the

finer the nearer the heart; an appearance which he regarded as somehow connected with an effort on the part of nature to restore the deficient serum.

In the department of Zoology, Dr. RICHARDSON communicated a paper from Dr. BELLINGHAM, on the frequency of the occurrence of the *Tricocephalus dispar* in the alimentary canal. He regarded it as premature to attempt the decision of whether this parasitic animal was or was not formed in the intestines.

Mr. JENYNS communicated a paper from Dr. WILLIAMS, on a species of *Linax* found in the human intestines.

The meeting seems altogether to have been a very good one. Next season the Association is to assemble in August, at Newcastle-upon-Tyne; on which occasion the Duke of NORTHUMBERLAND has consented to act as President, and the Bishop of DURHAM as Vice-President.

ST. BARTHOLOMEW'S HOSPITAL.

Rupture of a Muscle.

SEPTEMBER 13th.—A butcher came to the hospital, carrying in the palm of his hand about half the tendon, and a portion of the muscular tissue of his flexor longus pollicis. He said, that jumping down from a raised board he had caught himself on one of his meat hooks by the palmar or surface of his wrist; it had hooked under the tendon of the flexor longus pollicis, and had passed, tearing its way along the ball of the thumb; and the tendon not breaking through with his weight, suspended from it, its flattened expansion had been pulled completely out from the middle of the muscle attached to it. There was no wound of the fore arm visible externally, and the distal end of the tendon still remained attached to the last phalanx of the thumb, close to which it was cut off, and the wound of the palm dressed. Some few short portions of muscular fibre were still attached to the tendon, and life was not yet, after several minutes had elapsed, extinct in them; for when he first arrived they were lying soft and flaccid on the tendon; but when thrown into cold water they crimped up, and still, after five days, remained at right angles to the surface of the tendon, and firmly contracted.

Corpus Luteum six months after delivery.

September 11th.—On examining the body of a young woman, 22 years of age,

* It was *this* Dr. Holland's paper of which we gave a short notice last week.

who had died of dropsy, with disease of the kidneys, supervening on scarlatina, an elevation being observed on the surface of the left ovary it was cut open, and found to present the cicatrix of a corpus luteum, though it was now six months since she was delivered of the child which she was suckling, till within a short time of her death.

The elevation visible externally was raised about a line and a half from the surface, and formed part of a rounded body, between three and four lines in diameter, of a firm and dense structure, presenting, in its centre, a white radiated cicatrix, surrounded with a dark-greyish substance passing between and marking the outline of the rays of white. In the centre of the cicatrix there was an indistinct grey small spot. On the surface of the elevation a small piece of tissue, like a membranous adhesion, hung loosely, and in the same ovary there was a cyst filled with a grumous yellow matter.

The appearance of the cicatrix was almost precisely that described and figured by Dr. Montgomery, in his work on the "Signs of Pregnancy and Delivery," as found in the body of a female who had died five months after delivery. The minute yellow spot, however, was not visible here, and the outline of the radiated cicatrix was better marked.

The kidneys were the only organs found diseased; they were of very large size, presented the morbid yellowish deposit described by Dr. Bright, in an advanced degree, and on injection were found far less vascular than usual.

Irreducible Dislocation of the Hip.

August.—A fine young man, of 33, was brought up from the country with a dislocation of the hip of six weeks' standing. The surgeon who had first seen it had not been able to detect any such injury. The day before his admission an attempt at reduction was made, without success. Two days after, a second was made by Mr. Stanley, with every possible care; he was bled to fifty ounces from the two arms; and had, while extension was being made, four grains of tartar emetic given him, which together reduced his muscular strength to such a degree, that the pressure of his robust hand could scarcely be felt. Constant and steady extension by the pulleys was maintained for an hour and ten minutes, and the head of the bone was drawn from the dorsum of the ilium completely to the level of the acetabulum; but no effort could force it into that cavity; and on suddenly cutting the pulley-ropes, and simultaneously abducting the thigh, it passed back to its previous situation.

Notwithstanding that in this attempt every favourable circumstance seemed to have been present: the man expressed so strong an anxiety that further efforts should be made, that in the next week a third endeavour was made, but with as little success; a fourth was added a few days after, but it only tended to confirm the probability, that some mechanical obstacle (perhaps a portion of the torn capsular ligament) is lying over and closing the acetabulum.

He has, therefore, left the hospital unrelieved, though the head of the femur seems to have formed a new articulating surface; and there is tolerably extensive motion of the limb; but shortened as it is nearly two inches, he is of course deplorably crippled.

Strangulated Hernia.

September 11.—Mr. Lawrence operated on an old woman, for a small femoral hernia in the left side, which she said had been down only two days.

It had been intended to attempt a reduction of it without opening the sac, but this was found not possible, and there was some difficulty in determining when the sac was opened, for on exposing what seemed to be intestine, its surface was found rough and filamentous. This was discovered to be a firm but thin layer of organized lymph, closely adherent to the peritoneal surface of the intestine, and uniting it tightly to the walls of the sac. A portion of coagulated blood effused into it, had at first had the appearance of a gangrenous spot on the gut. The adventitious membrane being dissected off, and removed from its connexions to the two serous surfaces, the exceedingly light structure formed by the anterior edge of the superior process of the fascia lata was divided, and the intestine now easily returned. A considerable quantity of serous fluid flowed out of the abdomen by the wound.

The case was remarkably one of those in which it would have been impossible to return the intestine except with great violence and harm, without opening the sac.

Rather severe peritonitis coming on at night, six dozen-leeches were applied at different times, and the patient has since gone on well.

LONDON HOSPITAL.

To the Editor of the Medical Gazette.

SIR,

You did me the favour some time since to publish a monthly report of the frac-

tures admitted into the London Hospital: and as I understand the subject has interested some persons, I have endeavoured, as well as I could, to arrange at one glance the whole of the cases admitted during six months entire. I admit freely that it is still very imperfect; but I have ascer-

tained as accurately as I could, the ages of the respective patients. But as I, in many instances, could not arrive at this point, I have drawn the conclusions from those whose ages are authenticated. If you deem it of sufficient interest, you will oblige me by its insertion.

Case.	No.	Comparative Ages.
Cranium	12	Varying from 9 years to 48. From 28 years to 52. From 17 years to 71. { From 19 years to 74: of these, four-fifths were above 40, at which period the elasticity of bone may be considered as much decreased*. Adults.
Upper jaw	3	
Lower jaw	3	
Spine	5	
Pelvis	4	
Ribs	50	{ From 9 months to 60 years of age; half of which are under 12 years, and the bulk of the remainder from 20 to 30. From 4 years to 50, of which one-third are under 12: of the remainder, the greater number range from 18 to 40. { From 4 months to 70 years; nearly one-third under 12. From 19 months to 79 years. One-sixth occur under 12, and a very large proportion above the age of 50; many of these cases being fractures of the cervix. { From one year and a half to 72; one-ninth occur under 12; one-fourth above 50.
Sternum	2	
Scapula	7	
Clavicle	50	
Humerus	34	
Fore-arm, one or both } bones	43	{ From 19 months to 79 years. One-sixth occur under 12, and a very large proportion above the age of 50; many of these cases being fractures of the cervix. { From one year and a half to 72; one-ninth occur under 12; one-fourth above 50.
Hand, metacarpus, &c. ..	17	
Femur	27	
Leg	66	
Patella	4	
Foot, &c.	6	
Total	333	

In attempting to form a comparison as to the side of the body more liable to particular fractures, it appears that, taking a general view of the whole, the left side seems most obnoxious to fracture; and this is more especially observable in the fractures of the ribs; whilst nearly an equal preponderance in favour of the right side exists in reference to the leg and forearm.—I am, sir,

Your obedient servant,
JOHN ADAMS.

31, New Broad-street,
Sept. 21, 1837.

* Half are above 50 years of age.

WESTMINSTER HOSPITAL.

Purpura Hemorrhagica—Fatal.

WILLIAM HUNT, æt. 45, admitted January 13th, under Dr. Bright. On the elbows, down the course of the spine, inside of the thighs, and generally over the legs, the purple *maculæ* are dispersed: in some places the patches are so large as to seem like the result of bruises. The colour is most decided in the centre of each spot, and gradually fades towards the margin. The pulse is 84, feeble; tongue clean; bowels torpid, motions clay coloured; urine scanty and high coloured, depositing a sediment like carmine. The man has a sallow complexion; the white of the

eye is tinted gamboge. There is no tenderness in the hypochondrium. The abdomen is tense over fluid. The legs are œdematous.

Hunt is an old soldier; he served in Spain and at Walcheren. At this last place he had the well-known epidemic. Being an habitual drunkard, he was exposed, about six weeks before admission into this hospital, to cold, whilst in a state of inebriation. When he came to himself, he had pain in the right hypochondrium and throughout the muscles of the body; these symptoms gradually evanished, and were succeeded by dropsy of the belly and legs. Contemporaneously an attack of hæmoptysis occurred. About a week before he showed himself at the hospital, the petechiæ just described made their appearance. Ordered the following medicines:—

R Ext. Coloc. Comp. gr. x. hæc nocte et pro re nata sumend.

R Pulv. Rhæi, gr. iij.; Pulv. Cinchonæ, ʒj. M. fiat pulv. ter die sumendus.

Decoct. Cinchonæ et Acid. Nitric. 6tis horis.

January 14th.—The abdomen is still tense; water not increased; bowels confined.

Rep. Pulv.

15th.—The petechiæ unaltered; copious rosaceous deposit falls from the urine; anasarca swelling of the legs diminished.

16th.—Fresh petechiæ have appeared on the calves of the legs. The older petechiæ are of a paler colour; the œdema has diminished; abdomen still tense; urine of a dark madder colour, depositing the purpura of ammonia. During the day considerable hæmorrhage took place from the lungs. *Calomel and squill pills* to be taken daily, and cream of tartar solution to be drunk *ad libitum*.

17th.—The purple spots have diminished as regards intensity of colour. Epistaxis took place to some extent this morning. The bowels torpid. To take a dose of spirit of turpentine, qualified with oil of peppermint.

18th.—Complains of some pain in the left side, increased on coughing. The purpura has appeared in some fresh places. The bowels are freely open.

20th.—Bowels again torpid, the maculæ spreading generally over the back. Urine increased in quantity, but of the same madder-like colour, and still depositing the purpura.

23d.—The patient much harassed by cough. The sputa consist of mucus, floating in a greenish fluid. The yellowness of the eye has remained undiminished. The urine has increased to three

pints daily. To be purged with jalap and calomel.

24th.—Abdomen less tense. The maculation diminishing in colour and extent; urine clearer, and more lightly coloured.

25th.—This morning about nine o'clock sudden hæmorrhage from the mouth occurred. It proceeded apparently both from stomach and lungs. The blood was of a bright vermilion hue, and streaked with dark lines. The face of the patient is contracted. Pulse very weak. Laudanum and dilute mineral acid is administered in proper doses.

10 A.M.—The hæmorrhage returned in ten minutes after the exhibition of the draught, to the extent of nearly a quart. No pulse was perceptible at the wrist. A glass of wine given. The hæmorrhage remained uncontrolled, and the patient died at half-past ten o'clock.

Sectio cadaveris, thirty six hours after death.—The entire body was dotted with petechiæ, and all the surface was of a yellowish tint; the lungs were slightly œdematous, but in other respects healthy. The mucous surface of the stomach, and small intestines throughout, covered with blood: this membrane, when sponged, was of a red colour, and infiltrated with blood. The liver was granular, and adherent by membranous bands to the adjacent viscera. The gall bladder distended with bile; the cystic duct thickened, and almost imperious. The contents of the gall bladder consisted apparently of blood mixed with cystic bile of a sweet mawkish taste; staining red rather than yellow. The surface of the gall bladder was ecchymosed; all the other viscera were sound, except the muscoli recti abdominis, which were completely infiltrated with blood.

Gangræna Pulmonum.

Francis Wright, admitted 22d January. This man, 58 years old, complained, when admitted, of great debility and unceasing nausea, bringing on frequent vomiting. He had pain in the epigastrium, and occasional expectoration of blood; the sputa had an offensive smell, and the patient had a troublesome cough; there was no appetite, and no food but milk was taken; pulse 90, and jirking; the countenance was natural, not indicative of suffering.

About a week previously, whilst walking in the street, he was suddenly seized with sickness. This feeling was almost immediately succeeded by a sense of suffocation, and a hæmorrhage from the mouth of bright arterial blood. For two months previous to this event he had felt a constant and dull pain under the sternum; this was much increased on motion, but it almost

entirely ceased on lying down. He could recline on either side.

The breath of the patient has an offensive fœtor. The sputa consist of little, rounded, blackish grey masses, occasionally tinged with blood. Percussion elicits sounds clearer than natural over the superior part of the *right* lung; over other parts of the thoracic hemisphere the resonance is good. Cavernous respiration is evident in the apex of the lung. In the subclavicular and mammary regions of the same side, the vesicular murmur is indistinct. In the lower portion of the lung this sound is perfect.

The sounds brought out by percussion on the *left* side are quite normal. The respiratory murmur is of a *purile* character. Bronchophony is evident in the apex of the lung.

21th.—The odour of the patient's breath is so great, that the ward is offensive to the other patients. The bowels have been opened with castor oil. He is ordered infusion of roses and muriatic acid, and a blister is applied to the sternum. The chloride of lime in solution is placed under the bed. Strong beef-tea is to be allowed twice a day.

27th.—The patient's breath is not so fœtid as before; the cough and expectoration unchanged; the pulse 50, and feeble; the bowels are open. He states that he feels better. The auscultative symptoms are unaltered.

Milk diet, and the following medicine:

R Sol. Chlor. Sodæ, ℥ss.; Aquæ Distillatæ, ℥viiss. M. sumatur ℥j. 4tis horis

29th.—He cannot take the chloruret mixture because it nauseates him. He is to recur to the acid mixture. The breath is as offensive as ever. Bowels regular; tongue moist and clean; pulse small and feeble; expectoration viscid, purulent, and of a dark blown colour. The dull sub-sternal pain has increased.

31st.—Great thirst is now added to the other symptoms. He is nauseated by the offensiveness of his own expectoration.

Feb. 4th.—The patient complains of great prostration. Diarrhœa has supervened on the already described symptoms. The stools are fluid and graveolent; the pulse 90, weak.

6th.—Very weak; he is rapidly emaciating. He is ordered to have quinine and rose infusion.

9th.—The debility is getting hourly more obvious. The fœtor of the expectoration produces such continual nausea that he cannot take any nourishment; the breath is most repulsive. The respiration is short and hurried; 40 per minute. The

tongue clean, red, and moist; pulse 110, very small.

6 P.M.—The patient cannot articulate two words; he is rapidly sinking.

10th.—He died this morning.

Autopsy, 24 hours after death.—The patient's body is a good deal emaciated. On opening the thorax the appearance of the lungs, *in situ naturali*, was not unhealthy. These viscera did not collapse, and their superficies presented their usual bluish mottled appearance. Bands of recent lymph connected the inferior lobes to the diaphragm; patches of lymph were also deposited on the middle lobe of the right side. The *left* lung was not adherent to the costal pleura. On the *right* side the apex of the lung was attached by old inseparable adhesions; and the attempt to break through them opened a cavity in the substance of the lung, near the apex, about the size of a *tea cup*. This contained a dark green viscid fluid, in which were pieces of sphacelated lung as large as hazel nuts, some detached, and some adherent to the flimsy remains of the bronchial ramifications. The fœtor was scarcely tolerable. A smaller cavity of the like kind, and having similar contents, was found at the inferior margin of the middle lobe. A few miliary tubercles were dispersed through this lobe. The apex of the left lung contained also a few tubercular deposits, and there were generally traces of the grey hepatization.

The liver was enlarged and granulated. No other pathological phenomenon worthy of notice existed in any part of the body.

NEW TEST OF THE PRESENCE OF MORPHIA.

M. LAFARGUE has presented, during the last month, papers to the French Institute, to show that an invariable and very delicate test of the presence of morphia in fluids, is to be found in inoculation. He has tried infusions of several of the species of poppy which contain that principle, and of their congeners which do not; and he finds invariably that a small quantity introduced under the skin, as in vaccination, is followed by the development of a papula of determinate character.

SWEATING OF BLOOD FROM THE FEET IN TERTIAN FEVER.

DR. KAMINSKY has communicated a very curious case of this affection.

The father of a girl in the country, who was suffering from tertian fever, was alarmed, not only at the fever, but parti-

cularly at a transudation of blood from the feet from one day to another. On an accurate inquiry, it appeared that the girl, who, from description, had been of a strong and plethoric habit of body, had never menstruated. The blood flowed in considerable quantity on the days of the fever, with the accession of the hot stage, from both feet, from openings caused as if by leech bites, and again ceased gradually after the paroxysm was past. By an appropriate treatment, particularly by the employment of quina, which this physician gave in combination with carbonate of magnesia, carbonic acid, and rhubarb, the fever soon disappeared, and along with it the bleeding at the feet. After pediluvia had been employed for some time, menstruation appeared.—*Provincial Returns of Health of the Royal College of Medicine, Pomerania*; and *Dublin Journal*.

NEW GENUS OF NEMATOIDEA.

On examining the stomach of a fine young tiger belonging to the Zoological Gardens, Regent's Park, which died some time ago, two worms were found in it, one of which forms the type of a new genus of *Nematoidea*. Some of the worms measured nearly an inch in length, and a line in thickness; others being more minute. Only a pair of the larger were found, the smaller species existed in countless numbers; they were found in tumors. The mouth of these creatures is surrounded by a tumid circular lip, armed with six or seven circular rows of well-developed spinous processes of a complex structure. The oral orifice itself presents the form of a vertical elliptical fissure, bounded on each side by a jaw-like membranous fold or process, the anterior margin of which is produced in the form of three straight horny points directed upwards, which can be protruded beyond the circular lip by compressing the smooth spineless skin beyond the latter, and the elasticity of the structure causes them to be again retracted on remitting the pressure. The surface of the body appears to the naked eye to be minutely striated transversely; it is variegated by the white genital and amber-coloured digestive tubes appearing through the transparent integument. When examined by a lens of half-inch focus, the anterior two-thirds of the body are seen to be covered with circular series of minute reflected spines, which, viewed with a still higher power, present three distinct points, one large one in the middle, and two small lateral ones.—*Morning paper*.

SIGNS OF DEATH.

M. DONNE has pointed out, as the only certain mode of ascertaining that death has taken place, a short time after the event, the examination of the blood globules. In these, he says, putrefactive changes take place long before any alteration can be discovered in the tissues. He intends soon to present to the Institute a full detail of all the changes to which they are subject, and their distinctions from those occurring during life.

NEW MEDICAL WORKS.

The Surgical Works of John Hunter, with Notes by J. F. Palmer. 3 vols. 8vo. with Plates, 2l. 12s. 6d.

The Nature and Treatment of Diseases of the Ear. By Dr. W. Kramer. Second Edition of the Author's Treatise on Chronic Deafness, much improved and enlarged. Translated from the German, with the latest improvements of the Author, since the last German edition, by J. R. Bennett, M.D. &c. 8vo. 10s. 6d.

A Treatise on Operative Surgery, by W. B. Cocks. Illustrated by 12 plates, representing 64 Surgical Operations, &c. 8vo. 11s.

The Chemical Decompositions of the London Pharmacopœia. By J. Steggall, M.D. 12mo. 3s.

Celsi di Medicina opera elente et enarrata J. Steggall, M.D. 12mo. 7s.

Medico-legal Treatise on Homicide by External Violence, with an Account of the Circumstances which modify the Medico-legal Characters of Injuries, and Exculpatory Phases. By Alexander Watson, Esq.

The Philosophy of the Eye. By John Walker.

Mr. Grainger on the Structure and Functions of the Spinal Cord. 8vo. 7s.

The Philosophy of Health. By Southwood Smith, M.D. Vol. 2, 12mo. 7s.

Encyklopädie der gesamten Medicinischen und Chirurgischen Praxis. Von Dr. G. J. Moser. 8vo. pp. 1138. Zweite stark vermehrte und verbesserte Auflage. Leipzig, 1837. London: A. Schloss.

This is a meritorious performance. The articles are judiciously selected, and contain a vast deal of practical information in small compass. We have perused with satisfaction and profit the article "Stricture," which, although not extending to ten pages, comprehends, nevertheless, every requisite information for the surgeon. We may add, that the "Encyklopädie" serves at the same time as a lexicon for the multifarious technical terms used by German medical writers.

TABLE,

Shewing the amount of Fees at different Schools, for unlimited Attendance on the Lectures required by the College of Surgeons and Society of Apothecaries; also the Fees for those Lectures only which are required by the above Corporate Bodies.

	Antony and Demonstra- tions.	Medicine.	Surgery.	Chemistry.	Materia Me- dica.	Midwifery.	Botany.	Forensic Me- dicine.	Additional percentages on those who have not a no- mination.	Matriculation Fee.	Total.	Total for the Lec- tures require by the College and Hall.
	L. s. 12 12	L. s. 5 5	L. s. 5 5	L. s. 6 6	L. s. 4 4	L. s. 5 5	L. s. 4 4	L. s. 3 3	none.	none.	L. s. 46 4	L. s. 42 0
Aldersgate School	15 15	6 6	5 5	6 6	4 4	6 6	4 4	4 4	none.	none.	52 10	49 7
Webb Street	15 15	6 6	6 6	6 6	6 6	5 5	3 3	3 3	none.	none.	52 10	40 0
Westminster School	16 16	6 6	5 5	6 6	5 5	5 5	4 4	4 4	none.	none.	52 10	45 0
Middlesex Hospital	18 18	6 6	5 5	6 6	4 4	6 6	4 4	4 4	none.	none.	55 13	49 7
St. Thomas's Hospital	16 16	6 6	5 5	8 8	6 6	5 5	4 4	4 4	none.	none.	56 14	51 9
St. George's Hospital	21 0	7 7	5 5	8 8	4 4	7 7	4 4	4 4	none.	none.	63 0	50 0
London Hospital	21 0	7 7	7 7	8 8	6 6	5 5	4 4	4 4	none.	none.	64 2	57 15
St. Bartholomew's Hospital	18 18	7 7	6 6	10 10	7 7	6 6	4 4	4 4	none.	1 1	65 2	57 15*
King's College	21 0	8 8	5 5	8 8	4 4	10 10	3 3	4 4	none.	none.	66 3	56 11
Guy's Hospital	18 0	8 0	6 0	10 0	9 0	7 0	6 0	4 0	4 10	2 0	74 10	67 12
University College												

N.B. The above does not include Lectures delivered at private houses nor schools only, now about to commence. We shall add to our list of next year such of the latter as are then in existence.

* For Students nominated by proprietors, this sum is reduced to £5g 10s.

OF

DRUGS ON SALE IN THE ENGLISH MARKET,

With their Prices and several Duties.

(From the Official Returns up to Tuesday, September 26, 1837.)

	PRICE.		DUTY.		DUTY PAID	
					In 1837 to last week.	Same time last year.
	£ s. d.	£ s. d.		s. d.		
Aloes, Barbadoes, D.P. c	12 0 0	to 30 0 0	} B.P. lb 0 2		80,300	80,533
Hepatic (dry) BD. c	5 0 0	14 0 0				
Cape, BD. c	1 10 0	1 16 0	} F. lb 0 8			
Aniseed, Oil of, German, D.P. lb	0 9 6	0 9 6				
E. I. lb	0 7 0	0 7 6	F. I. lb 1 4	251	1,337	1,516
Assafoetida, B.D. c	0 2 10	0 5 0	c 6 0	23	113	
Balsam, Canada, D.P. lb	0 1 3	0 1 4	lb 0 1	1,350	1,312	
Copaiba, BD. lb	0 4 0	—	c 4 0	183	204	
Peru, BD. lb	0 5 0	—	lb 1 0	507	1,660	
Benzoin (best) BD. c	25 0 0	50 0 0	c 4 0	103	45	
Camphor, unrefined, BD. c	9 0 0	—	c 1 0	292	253	
Cantharides, D.P. lb	0 4 9	0 6 0	lb 1 0	19,863	14,703	
Carraway, Oil of, D.P. lb	0 9 0	—	lb 4 0	1,370	1,359	
Cascarilla or Eleutheria Bark, D.P.C. lb	1 15 0	—	lb 0 1	3,483	3,499	
Cassia, Oil of, BD. lb	0 9 0	—	lb 1 4	2,721	4,120	
Castor Oil, East India, BD. lb	0 0 6	0 0 10	c 1 3	5,118	4,250	
West I. (bottle) D.P. 1½ lb	0 2 3	—				
Castoreum, American lb	1 15 0	—	} lb 0 6			
D.P. Hudson's Bay lb	1 0 0	1 4 0		203	751	
Russian lb	none	—				
Catechu, BD. c	1 2 0	—	} c 1 0	23,175	7,305	
Cinchona Bark, Pale (Crown) lb	0 2 0	0 3 6				
BD. Red lb	0 3 0	0 6 0	} lb 0 1	110,275	95,387	
Yellow lb	0 1 2	0 1 3				
Colocynth, Turkey lb	0 2 6	0 4 0	} lb 0 2	6,427	12,072	
D.P. Mogadore lb	0 3 0	—				
Calumba Root, BD. c	1 4 0	2 5 0	lb 0 2	8,812	9,725	
Cubebs, BD. c	3 0 0	—	lb 0 6	30,207	20,565	
Gamboge, BD. c	5 0 0	15 0 0	c 4 0	69	77	
Gentian, D.P. c	1 4 0	—	c 4 0	405	190	
Guaiaicum, D.P. lb	0 1 0	0 1 8	c 6 0	54	8	
Gum Arabic, Turkey, fine, D.P. c	8 0 0	9 0 0	} c 6 0	2,969	8,442	
Do. seconds, D.P. c	5 0 0	7 0 0				
Barbary, brown, BD. c	4 5 0	4 10 0	} c 6 0			
Do. white, D.P. c	4 15 0	—				
E. I. fine yellow, BD. c	3 0 0	3 10 0	} c 6 0	1,883	2,466	
Do. dark brown, B.D. c	1 15 0	2 5 0				
Senegal garblings, D.P. c	4 15 0	5 0 0	c 6 0	2,706	2,824	
Tragacanth, D.P. c	13 0 0	20 0 0	c 6 0	316	210	
Iceland Moss (Lichen), D.P. lb	0 0 2½	0 0 3	lb 0 1	6,909	20,599	
Ipecacuanha Root, B.D. lb	0 2 6	0 2 9	lb 1 0	8,078	7,776	
Jalap, BD. lb	0 1 8	0 1 0	lb 0 6	4,1190	39,985	
Manna, flaky, BD. lb	0 4 0	—	} lb 0 3	16,066	9,396	
Sicilian, BD. lb	0 1 7	—				
Musk, China, BD. oz	1 0 0	1 8 0	oz 6 0	1,636	1,231	
Myrrh, East India, BD. c	5 0 0	14 0 0	} c 6 0	104	160	
Turkey, BD. c	2 0 0	11 10 0				
Nux Vomica, BD. lb	0 8 0	0 9 0	lb 2 6	1,120	1,789	
Opium, Turkey, BD. lb	0 12 6	0 13 0	lb 1 0	31,361	29,142	
Peppermint, Oil of, F. BD. lb	1 0 0	—	lb 4 0	925	341	
Quicksilver, BD. lb	0 3 6	—	lb 0 1	205,067	212,477	
Rhubarb, East India, BD. lb	0 2 0	0 3 0	lb 1 0	33,938	31,233	
Dutch, trimmed, D.P. lb	0 3 6	0 4 0	} F. lb 1 0	4,487	5,538	
Russian, BD. lb	0 8 3	—				
Saffron, French, BD. lb	0 19 0	1 0 0	} lb 1 0	4,103	3,755	
Spanish lb	1 1 0	—				
Sarsaparilla, Honduras, BD. lb	0 1 0	0 1 9	lb 0 6	78,386	91,288	
Lisbon, BD. lb	0 2 0	—	} lb 2 6	5,949	5,137	
Scammony, Smyrna, D.P. lb	—	—				
Aleppo lb	0 12 0	0 15 0	} E.I. lb 0 6	76,483	71,092	
Senna, East India, BD. lb	0 0 3	0 0 4				
Alexandria, D.P. lb	0 1 6	—	} Other sorts 0 6	43,836	52,077	
Smyrna, D.P. lb	0 1 0	0 1 3				
Tripoli, D.P. lb	0 1 0	0 1 3				

‡‡‡ BD. In Bond. — c. Cwt. — B. P. British Possessions. — F. Foreign. — D. P. Duty paid.

NEW GERMAN WORKS.

Analekten für Frauenkrankheiten von einem Vereine praktischer Aerzte. Bd. I. H. i. 8vo. pp. 160. Leipzig, 1837. Schloss.

Analekten der Chirurgie, unter Redaktion von Dr. E. BLASius und Dr. A. MosER. Bd. I. H. i. pp. 126. Berlin, 1837. Schloss.

THESE are congenerous works. Each professes to contain a collection of the principal monographs, prize-essays, and dissertations of recent date. The first part of the *Analekten* of Diseases of Women comprises memoirs on puerperal convulsions, phlegmasia alba dolens, leucorrhœa, &c. The surgical *Analekten* commence with Lisfranc's lectures on amputation of the cervix uteri, and extirpation of the uterus, together with Stromeyer's new treatment of club-foot.

Die Vergleichende Osteologie des Schläfenbeins. Von EDUARD HALLMAN, Mit. IV. Kupfertafeln, 4to. pp. 130. Hanover, 1837. London, A. Schloss.

THIS is an elaborate disquisition on the comparative osteology of the temporal bone. The author adopts the quadruple division of Cuvier in the order of investigation. 1. The squamous portion with the zygomatic process, which is never a separate piece. 2. The *os tympanicum*. 3. The *os petrosum*. 4. The *os mastoideum*.

Observationes Neurologicæ. Auctore FRIDERICO SCHLEMM. 4to. pp. 22. Berlin, 1834.

PROFESSOR SCHLEMM'S Dissertation treats of the minute anatomy of the medulla spinalis, the description of certain nervous filaments passing to the inferior rectus muscle of the eye, together with the dissection of the vidian and some of the ophthalmic nerves in the *Meleagris gallopavo*.

Encyclopädisches Wörterbuch der Medicinischen Wissenschaften. Herausgegeben, von Drs. Bsch, von GRAEF, HUFELAND, LANK, MÜLLER. Bd. xiv. H. 3 and 4. Berlin, 1836.

THE Berlin Cyclopedia of Practical Medicine continues to sustain its reputation. The articles "Ulcer" and "Poison," in the above volume, are elaborate and excellent.

LITERARY INTELLIGENCE.

In the press, an Inquiry into the Nature and Treatment of Cholera; being Part I, Vol. II., of an "Inquiry into the Principles and Practice of Medicine." By George Calvert Holland, M.D., Physician to the Sheffield General Dispensary.

Dr. Ayre of Hull has in the press a Letter addressed to the Rt. Hon. Lord John Russell, M.P., Her Majesty's Principal Secretary of State for the Home Department, on the Evil Policy of those Measures of Restorative Police and Quarantine, which are employed to arrest the progress of the Asiatic Cholera; with an Inquiry into the Nature of, and the means of obviating, those circumstances in the Physical Condition of the Labouring Poor which especially predispose them to the disease, and make them nearly the only victims of it.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, Sept. 26, 1837.

Age and Debility . . . 17	Inflammation . . . 17
Apoplexy . . . 4	Bowels & Stomach . . 3
Asthma . . . 5	Brain . . . 2
Childbirth . . . 1	Lungs and Pleura . . 4
Consumption . . . 32	Influenza . . . 2
Convulsions . . . 15	Liver, diseased . . . 2
Dentition or Teething . . 3	Measles . . . 13
Dropsy . . . 10	Small-pox . . . 2
Dropsy in the Brain . . 3	Sore Throat and . . .
Epilepsy . . . 1	Quinsey . . . 1
Fever . . . 14	Spasms . . . 2
Fever, Scarlet . . . 1	Thrush . . . 1
Fever, Typhus . . . 9	Unknown Causes . . 17
Heart, diseased . . . 1	
Hooping Cough . . . 7	Casualties . . . 3

Decrease of Burials, as compared with the preceding week . . . } 231

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 39" N. Longitude 0° 3' 51" W. of Greenwich.

Sept.	THERMOMETER.	BAROMETER.
Thursday . . 21	from 29 to 68	29.84 to 29.96
Friday . . . 22	39 67	29.97 29.99
Saturday . . 23	41 61	30.02 30.07
Sunday . . . 24	35 60	30.12 30.19
Monday . . . 25	31 60	30.21 Stat.
Tuesday . . 26	30 60	30.21 30.12
Wednesday 27	29 60	30.06 30.01

Except the 27th, generally clear; a little rain on the 26th.

CHARLES HENRY ADAMS.

NOTICES.

THE Communications of Mr. H. Taylor—Mr. Harbord—Dr. Cape—Dr. Cowan—Mr. Laycock—Dr. Lilburn—and Mr. Lewis, have been received.

WILSON & SON, Printers, 57, Skinner-st., London

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, OCTOBER 7, 1837.

LECTURES
ON THE
PHYSIOLOGY AND DISEASES OF
THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL
AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of
1836 and 1837,

*At the Anatomical School, Kinnerton-Street,
near St George's Hospital,*

BY CHARLES J. B. WILLIAMS, M.D. F.R.S.

LECTURE II.

Internal mechanism of respiration—Form of the air-tubes and cells—Structure of the air-tubes—Cartilages—Mucous membrane—Longitudinal fibres—Circular fibres—Connecting and investing tissues of the lung—Blood-vessels of the lung—Chemistry of respiration—Changes of the air and of the blood—Physical cause of the change of colour of the blood—Adaptation of mechanism to chemical action—Ciliary motions of the bronchial mucous membrane.

BUT now let us proceed to examine the internal parts, to the functions of which the exterior motions of respiration, which we have been considering, are subservient.

Unless the air enter and pervade the lungs freely each time the chest is enlarged, and unless it find a ready egress from them as the chest is contracted, the external machinery, however perfect it may be, will work in vain. Now if we examine the structure of the lungs and their connected tubes, we shall find great cause to admire the beautiful provisions displayed in every part to prevent this want of harmony, and to facilitate and regulate the passage of air through them; and we shall meet with further illustrations of the nature of these

provisions in the modifications which disease can produce in them.

The lungs may be described as essentially constituted by the multiplied divisions of the expansible air tubes; and we shall obtain the best knowledge of their structure by tracing these tubes from the trachea to their terminations.

You must all be sufficiently familiar with the form and appearance of the windpipe and its larger branches. Their form and open calibre are given to them by their cartilages, which, in the trachea and the two first divisions, are in rings, almost surrounding the tube,—in the next order of bronchi, constituting irregular pieces; but at the origin of each branch still nearly annular, to keep open the orifice. These plates are fewer as the bronchi, on subdividing, become more slender, those at the orifices of branches being only half rings; and they cease altogether in the tubes of half a line in diameter, the tubes then being membranous to their terminations.

What are these terminations? Malpighi supposed the lungs to consist of a series of vesicles, freely communicating like those of sponge, and that in these the minuter bronchi terminate. Helvetius conceived that the vesicles of the lungs were only common cellular texture, filled with air, and forming spongy sheaths around the pulmonary blood-vessels, and freely communicating with each other. Haller, judging from the simple vesicular lungs of frogs, &c. came to nearly the same opinion. Even lately Magendie held the notion that the vesicular structure of each lobule was common, having free communication between its cells, besides through the bronchial tubes distributed through it; and a similar view seems to be entertained by Bourgery.

The researches of Reisseissen are the most complete and satisfactory of any that have been made, and I shall avail myself of some of his descriptions to convey to

you an idea of the minute anatomy of the lungs.

By various modes of injecting, he shewed that the bronchial tubes end in *culs de sac*, without mutual communication, except through the bronchi, and without communication with the interstitial cellular texture. Thus by inflating a minute bronchus, the cells of the portion of lung supplied by it became distended as regular roundish vesicles; and, by tying the bronchus, remained so, without any air passing to the adjoining cells. Mercury injected into a single bronchus gave the same result.

If, on the other hand, the common cellular texture be inflated through a minute cut in the pleura, the air is diffused in angular and irregularly-sized bubbles, giving inequality to the pleural surface, and appearing, as in the manner of a common emphysema, more between than in the lobules. Mercury poured into a bronchial tube, and allowed to run to its extremities, is seen in little globules under the pleura, communicating by branches at first very numerous and small, which again run into fewer and larger. If this be pressed between two glasses under the microscope, the mercury then goes to the extremities, and takes the regular cauliflower shape of the extreme canals.

Another mode adopted by Reisseisen to shew the shape of the minute extremities was this:—A portion of lung was kept some days in water, so that only very little air remained; hot water was then poured on it, which distended this air, and displayed the expansions of the terminal tubes like little bunches of buds or berries. [These appearances were exhibited in drawings.] I have repeated and verified these observations, and I think we may admit as correct Reisseisen's description of the form of the terminal bronchi.

Now, then, having considered the form, if we examine more minutely the component structures of the bronchial tree, from its trunk to these bud-like termini, we shall find that each is worthy of notice, and that there are some remarkable differences in the several parts. Thus we have seen that the cartilages—large and almost annular in the larger tubes, forming smaller and irregular segments in those of middle size, and ceasing almost entirely in the inner tubes—answer the important purpose of holding open the tubes in those parts where they are apt to be exposed to pressure. If the tubes were merely membranous, they might collapse together by atmospheric pressure, or by the distension of the pulmonary tissue at the upper parts of the lung, and be prevented from transmitting air to their extremities, whilst the absence of cartilage in the smaller tubes permits their flexibi-

lity and distension. Well, besides this cartilaginous frame-work, which varies according to the parts, we have at least three other structures, appearing in every part of the air-tubes.

1. A mucous membrane, with many muciparous crypts or follicles, conspicuous in the larger tubes, some of which penetrate through the other tunics, even to the cartilaginous coverings. In the smaller tubes this membrane becomes fine, thin, and destitute of distinct follicles, and in the terminal cells is of the greatest tenuity.

2. Longitudinal fibres, which are very elastic, like the elastic coat of the arteries. They are very conspicuous in the large and middle-sized bronchi, but they can be traced to the very ends of the tubes; it is these chiefly which cause the lungs to contract and collapse, when the chest is opened after death; and to a certain extent during life they must therefore assist in the act of expiration. Here you see them in the human bronchi [exhibiting a drawing], and here in those of the calf, in which they are more distinct and numerous than in man.

3. Circular muscular fibres, which are to be found in every part of the bronchial tree. In the larger tubes they are inserted into the ends of the annular cartilages, thus completing the rings. In the smaller they encircle the whole tube, some fibres being attached to the cartilaginous pieces, and to the longitudinal fibres, and some passing over them; and, by the aid of a lens, they can be seen, especially in the lungs of the lower animals, at the very extremities of the canals. The extreme bronchi have been shewn by experiment to contract during life, on the application of a mechanical or chemical irritant; and we shall soon find reason to suppose that their function renders expiration to a certain degree active. When we come to the pathology of asthma, we shall find other proofs of the irritable contractility of these bronchial muscles, and that their share in the process of respiration will serve to explain many facts which were inexplicable before the existence and action of these muscles were demonstrated. We shall recur to this subject when we come to the vital properties of the respiratory organs. The annexed drawing from the human subject exhibits a distinct view of the longitudinal, and of these circular or transverse fibres.

Thus, then, we have found in the respiratory apparatus a chest capable of enlargement and diminution; and within it not a mere bag, or assemblage of bags, but a series of elastic, expansible, and contractile tubes, subdividing into innumerable branches, each terminating in blind enlargements, and constituting the chief part of the parenchyma of the lungs. But this

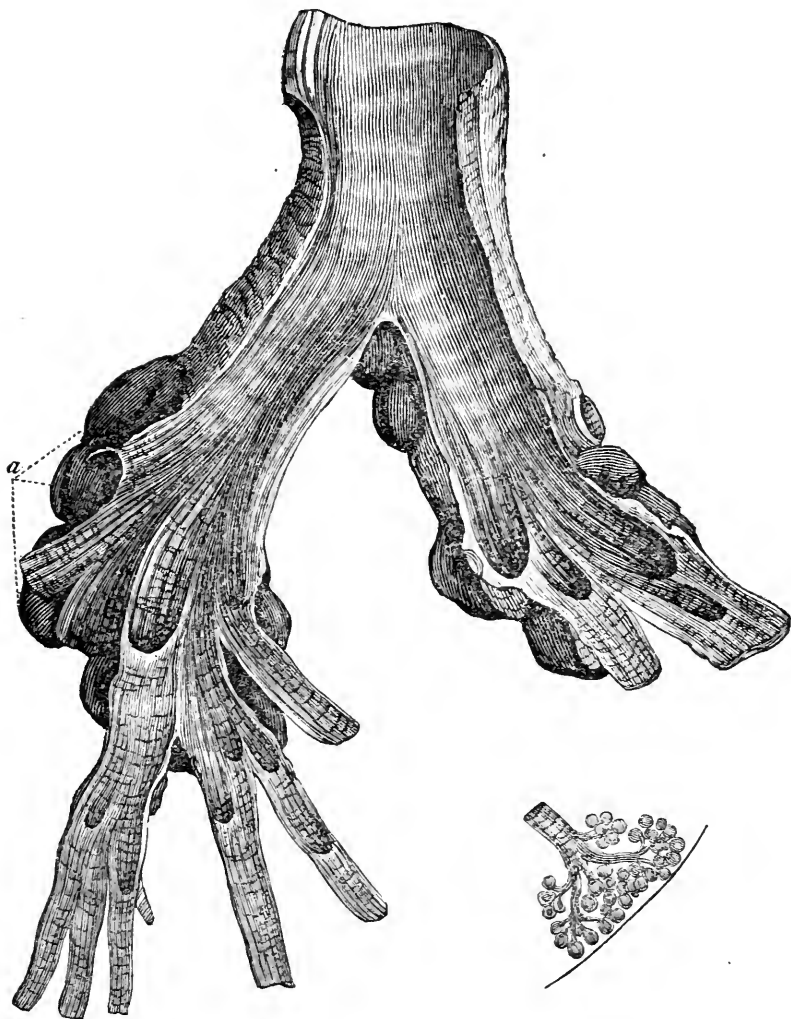


FIG. 1.

FIG. 1.—Division of the human Trachea and Large Bronchi, shewing the longitudinal and circular fibres. (a.) Branchial Glands.

FIG. 2.

FIG. 2.—Ultimate division of the Bronchi into clusters of air-cells. Magnified three diameters.

congeries of tubes and terminal cells is compacted together by other structures. There are the blood-vessels distributed over them, for the important purpose which we have afterwards to consider; there is an interstitial cellular texture, uniting the bunches of tubes and their vesicles into lobules, and in a more condensed form uniting these lobules into lobes; and there is an elastic serous membrane, a part of the bag which lines the whole chest, investing this congeries of

structures. This packs them into an organ with a simplicity of outward form which adapts it well to fill the changing capacity of the chest, while the mobility and elasticity of the connecting and investing structures still leave unimpaired the permeability and expansibility of each individual tube. Thus, when one part of the chest expands more than another, the air presses most directly through the tubes leading to that part, and an equality of pressure is thus kept up between the chest

and lungs, which is highly favourable to their harmonious working.

Although the elasticity of the bronchi gives the lungs a natural tendency in favour of contraction, so that, when not prevented by the pressure of air within, they collapse, yet the whole lungs will always *admit* of more distension than that which is required of them in respiration; so that if parts become obstructed, the air may enter more freely into other parts, for a short time without inconvenience; but, as we shall see hereafter, the continuance or excess of this partial distension is capable of producing permanent disease of a remarkable character.

Having thus sketched the route and the mechanism by which air passes to and fro in the lungs, the question, why does it so pass? brings us to another anatomical element to be considered—the blood-vessels, which are carefully and minutely distributed around the *air-tree*, especially its finer terminal parts. It is quite unnecessary for me to go here into any common details respecting the general circulation: I conclude *that* to be known; and I would reserve the particulars of the structure of the heart and great vessels, which deserve especial notice, to that part of the course in which their diseases will be considered. Blood of a dark hue is conveyed from the right ventricle of the heart through the pulmonary artery to the lungs, and it returns by the pulmonary veins into the left ventricle, of a florid red colour. Now by what ways does it pass as it undergoes this change? Minute anatomy has supplied us with an answer. It had long been known that fine injections would pass from the pulmonary artery to the pulmonary veins, or the converse, there being no valves in the veins of the lungs; but the actual communication of one with the other, through a set of capillaries on the sides of the minute air-tubes and cells, was first clearly demonstrated by Reisseissen. But it has since been seen in lungs of living frogs and other animals. In these drawings, which are enlarged from those of Reisseissen, you see this communication. In this, again, you see another set of vessels, the bronchial arteries, which are much smaller; they supply the mucous membrane and interlobular tissues, and may be seen here anastomosing with the pulmonary vessels. Branches from both bronchial and pulmonary arteries go to the pleura. The bronchial veins appear chiefly to empty themselves in the pulmonary veins.

The great object, then, of all the apparatus which has been occupying our attention, is that the blood and the air shall be continually moving in close proximity

to each other. We have said that the effect of this on the blood is to render it florid; and we may now add, that the chief effect on the air is to abstract a portion of its oxygen, and to substitute about an equal portion of carbonic acid gas. These are the fundamental facts of the *chemistry of respiration*. If we were to enter minutely into the details and explanations of these, we might be led into an interesting field of animal chemistry, which well might occupy half a dozen lectures; but we have so large a quantity of other matter more imperatively requiring attention, that we must dismiss the subject with a few cursory observations.

There are two views which have been taken of the use of the air which passes in and out of the lungs in respiration: one is, that its oxygen combines with, and carries off merely, a certain quantity of carbon, which the lungs are, by a vital secreting power, continually separating from the blood. A superfluity of this carbon is supposed to distinguish venous from arterial blood, it being acquired in the greater circulation through the system, as arterial is converted into venous blood, and thrown off by the pulmonary circulation, when venous blood is again made arterial. This supposes the air to be little more than a broom sweeping, or a current washing, away a carbonaceous excrement thrown out on the bronchial surface; and in confirmation, the black matter sometimes expectorated, and that almost always found in the bronchial glands and other corners of the lungs, have been adduced as instances where the air had not swept or washed clean. But there are many, and some I think conclusive, objections against this view. It is opposed by the chemical fact, that simple carbon, were it so excreted, could not combine with oxygen at the temperature of the lungs. It is also opposed by the fact, that the simple contact of air with the blood, without any structure to separate the carbon from the latter, is sufficient to effect the same change in the blood and in the air, as that which takes place in the lungs. Moreover, it is found that not only is no secreting structure necessary, but the change takes place in spite of an intervening membrane, such as a bladder, gold-beaters' skin, or even a thin lamina of Indian rubber. These and many other facts are quite inconsistent with the supposition that carbon is first separated from the blood, and then united with the oxygen of the air.

Of the black pulmonary matter we may find a sufficient source in the blacks and soot from our candles and fires, of the abundant presence of which in the air which we breathe, our linen, our ceilings, and every thing white that is exposed to

it, afford sufficient evidence. The lungs act as a filter for these impurities contained in the air, which, being insoluble, are not absorbed away, but accumulate in the tissue where they have become entangled.

The other view of the chemistry of respiration is, that the oxygen which is missing in respired air is absorbed into the blood, which throws out at the same time about an equal quantity of carbonic acid gas. The blood is thus arterialized, and, by the oxygen which it contains, rendered fit to excite and nourish the tissues, of which it is the proper pabulum; but in this office, under the control of an unknown vital influence, the oxygen exerts its chemical affinity, and abstracting from the blood a certain quantity of carbon, becomes carbonic acid; and the blood is thus again changed from arterial to venous. The chief characteristic, therefore, of arterial blood is, that it has *oxygen*,—of venous blood, that it has *carbonic acid*, combined with it; and the substitution of one principle for the other will effect the conversion of one kind into the other. This view is supported by a great many remarkable facts, and I know of none which really opposes it.

The change of blood in the lungs is essentially a chemical process, to promote which the mechanism of respiration is eminently adapted; but we find the same change to take place out of the body, and to be influenced as chemical processes generally are. The change of the blood in the greater circulation, from arterial to venous, is also partly chemical; and the affinity which we have adverted to, of oxygen for carbon, is a chemical force that is no doubt active, but this is probably greatly under the control of the vital powers; and I hold it to be a beautiful proof of the useful subserviency of inorganic to vital chemistry, that this affinity, which we know to be merely chemical, must tend to promote the formation of urea, uric acid, ammonia, gelatine, and other matters containing less carbon than the principles of the blood from which they are formed, which are more properly the products of vital organs, and the necessity of whose generation in the healthy state is obvious from the constancy and regularity with which it is carried on. We have no time to trace further the interesting relations that thus declare themselves in the chemical and vital laboratory of the body; but I would point out, as one pretty obvious corollary from them, that the function of the lungs stands in a relation peculiarly close with those of the great secreting organs of the body—the kidneys and the liver.

We cannot enter on the subject of the

composition of the blood; but I would call your attention to the remarkable physical difference which we have already noticed between venous and arterial blood—that of colour: that of venous is the most intense dark red—so dark that to reflected light it is almost black; but by transmitted light, it is of the finest crimson. Arterial blood, on the other hand, is of a bright crimson by reflected light; but it is of a dingier colour and more opaque than venous blood by transmitted light. This fact led me to the suspicion that the apparent brightness of arterial blood might be owing to the production of a white opacity in it, which would have the effect of reflecting white light through a thin film of the deep crimson. On closely examining the change of colour, whether produced by air or by the addition of saline matter, I have found that a certain white opacity always accompanies the brightening of blood: thus, if you closely watch this coagulum of dark blood when I drop it into this saline solution, you will see that it becomes first white and opaque at the edges; and this whiteness extending into the mass, shines through the deep crimson colouring matter, and gives it the florid hue. A thin drop of dark blood on a glass, viewed with a lens, appears transparent at first, but gradually as the air begins to act on it, or instantaneously on the addition of a grain of salt, a cloud of opacity is seen in it, which, when seen by reflected light, gives the florid colour of arterial blood. Seen through a good microscope, this opacity is observed to be caused by a myriad of distinct globules, smaller than those of the blood, apparently of the size of the central globule, and of those seen in serum. I have arranged this microscope that you may see this after lecture. The oxygen of the air, or the salt, appears to have the power of giving density or distinctness to the white organic particles contained in the blood, or even to increase the number of them; and these particles, acting as so many little mirrors, which reflect light through the colouring matter, give it the brightness so characteristic of arterial blood. Thus the change of colour appears in a simpler light, and one less calculated to mislead, because we are thus reminded of other changes in condition which accompany it. I should not have introduced this matter, but that it serves to show the utility of bringing all our senses to bear on the investigation of a subject—a leading principle in these lectures.

To return to the office of the lungs:—we have no evidence that there is any apparatus organized peculiarly for the absorption of oxygen into the blood, or the exhalation of carbonic acid from it. We

have noticed that the displacement of carbonic acid by oxygen takes place in blood out of the body; that the intervention of a membrane does not impede it; and it becomes a matter of obvious inference, that the distribution of the blood in millions of capillaries around the air-cells and tubes, with the thinnest possible membranes only separating it from the air, is only an admirable mechanical contrivance for rendering the chemical action between the air and the blood as complete as possible. The fact that air can pervade a membrane was long doubted; and this doubt was considered, by a great many writers, a sufficient reason for rejecting the view which we are considering; but this doubt was not well founded; the facts described by Priestley and John Hunter being enough to remove it. These found that venous blood became florid, although contained in a bladder or goldbeater's skin; and Goodwyn rendered the blood in the jugular vein of a living rabbit florid by blowing on it. I had the satisfaction of setting this question still further at rest, by showing, many years ago, that not only was the blood changed in colour, but that the air received carbonic acid from it, and consequently, that the air must have exerted an action on the blood through the membrane. Since I made this experiment, the researches of Dutrochet and Mitchell have developed the fact of the passage of gases or liquids through membranes into a general principle: and one which I believe may be referred to a still simpler law, that of the intermixture of different gases, as developed by Dalton and Graham, and which depends on the comparative elasticities of different kinds of matter. All these steps in science, which we cannot dwell on longer, have dispelled many difficulties which used to beset the theory of respiration: and the further results obtained in the experiments of Edwards, that azote, as well as oxygen, is sometimes *absorbed*, especially during the winter, and sometimes *exhaled*, leave no doubt that gases, as well as liquids and solids, may combine with, and be separated from, the components of the body.

A few other points of adaptation of the minute structure of the lungs to promote the chemical action between the air and the blood, are worthy of notice. The air-canal is lined throughout with a mucous membrane. This, in the trachea and larger bronchi, is not only of a considerable thickness, but is furnished with numerous distinct follicles, which throw out abundance of the peculiar slimy albuminous secretion called mucus. In the smallest bronchi these follicles cannot be detected, and it is uncertain whether they exist; but the membrane is every where moist

and slippery, as if covered by a thinner mucus of the same kind. The utility of this secretion is obvious, in protecting the membranes and tissues of the interior from divers irritations of extraneous matter, from excessive dryness, cold, or heat of the inhaled air; and hence the need of its being thicker and more abundant at the commencement of the canal, where these irritations would exert their fullest effect. But I believe this mucus, in the finer tubes and their extremities, to be useful in another way, in promoting, by its chemical quality, the transfer of oxygen from the air to the blood. Fourcroy long ago shewed that the bronchial mucus had, in a signal degree, the property of absorbing oxygen and of yielding it again to other substances, such as metals; hence it is the most effectual medium for killing mercury. I think, therefore, that it is reasonable to suppose the mucus with which the bronchi are bedewed, to aid in facilitating the oxygenation of the blood. A remarkable phenomenon has been lately observed with regard to the mucous membrane of the bronchial tubes of animals recently killed. When it is examined under water, through a microscope, the water is seen to be thrown into distinct and very rapid vortices or currents, which continue with great vivacity for a considerable time, even after the membrane is quite cold. These currents were ascribed by their discoverers, M. Purkinjie and Valentin, to the movements of very fine hairs, or *cilia*; and as they are seen in the gills of some aquatic animals, as the common muscle, in which they obviously serve the purpose of respiration, by keeping up a fresh supply of water to the branchial vessels, they suspected them to exert some similar effect on the air in the bronchial surface of the mammalia. But Dr. Alison has well remarked, that as the same currents are to be seen also in the mucous membranes of the genital organs of female animals, it is more reasonable to suppose, that the object of the movements that produce them is rather the equal diffusion of the mucus, the uniformity of which may be essential to the effectual performance of the functions of these organs. The same distinguished physiologist considers the opinion of Purkinjie and Valentin, that these currents are produced by cilia in motion, as too hastily assumed, their existence in the air tubes having never been proved; and he is inclined rather to ascribe them to a vital attraction and repulsion—a principle the existence of which he has brought many ingenious arguments to prove, and one which, if it do obtain, must have a large share in the motions and functions of the body.

ON THE HUMAN VOICE.

By PROFESSOR MÜLLER.

WE never remember to have read a more happy application of the laws of mechanical science to the structure and physiology of living bodies, than that which Professor Müller has effected in the account which he has given in the Third Part of his "*Physiologie des Menschen*" of the larynx and voice. He has detailed them there at too great length to admit of our translating the whole section of the work, though it would fully deserve it, could we spare room for it; but we shall endeavour, in as brief an abstract as possible, to give the most important results which he has arrived at, cleared of all the technicalities of music and mechanics, which can be spared, without rendering the description unintelligible.

The following are the proofs which he adduces, independently of those drawn from his experiments, of the glottis being that part of the vocal tube by which the voice is produced:—1st. That when an opening is made in the trachea below the glottis, the voice ceases and returns when that opening is closed—while an opening made above the glottis does not necessarily destroy the voice. 2nd. Magendie's experiments, showing that the voice continues when the epiglottis, superior vocal (or false) ligaments, and the top of the arytenoid cartilages have been removed. 3rd. The visible vibration of the inferior vocal ligaments, when the voice is heard, in animals whose glottis is exposed, and in men who have attempted suicide. 4th. The injury of the laryngeal nerves, which supply the small muscles acting on and modifying the aperture of the glottis. 5th. The possibility of producing sounds similar to those of the human voice, by blowing through the larynx when removed from the body, when the vocal ligaments have a certain degree of tension, and the glottis is open to a certain width, and this, when every other part, except the inferior vocal ligaments and their attachments, is cut away. Hence the glottis and its immediate limits, the inferior vocal ligaments, may be regarded as the essential vocal apparatus—the trachea and bronchi being analogous to the conducting pipe, or, as organ-builders call it, the boot of the pipe—and all the apparatus above the inferior vocal ligaments to the body or bell of wind instrument or organ pipes, by which the sound produced in the glottis, or by the vibrating tongue, is modified. In birds

there is a different arrangement of similar structures; the organ producing the voice is the inferior larynx, which is at the division of the trachea, and to which the bronchi serve as the conducting pipe, while the trachea, and the upper larynx which has no ligaments, perform the office of the pipe which modifies the sound produced at the lower larynx.

The vocal ligaments are rendered capable of regular vibrations, like those of membranes stretched at both ends, by being composed of the same peculiarly elastic tissue as has been pointed out by Lauth, Schwann, and Eulenbergh, as existing in the middle coats of arteries—the ligamentum nuchæ—lig. flava of the vertebral laminae—stylo-hyoid ligament—longitudinal elastic fibres of the trachea and bronchi, and many other parts of the body of man and animals. It is characterized by its yellowness, dryness, slight vascularity, and by its frequently giving off from the larger bundles, of which its fibres are composed, very fine branches, which, anastomosing with those from adjacent bundles, often form a kind of network. In chemical composition it is principally distinguished from the other fibrous, tendon-like or ligamentous tissues, by the difficulty with which gelatine can be obtained from it—and in physical characters, it is most marked by its extreme elasticity, which is permanent in it under almost all circumstances, even after it has been preserved for many years in spirit. But the vocal ligaments are not the only parts of the larynx in which it exists; the thyrohyoid and middle crico-thyroid consists of it, and it is extensively distributed through the whole interior of the organ, arising principally from the inferior half of the angle of the thyroid cartilage, and thence radiating in fibres backwards, downwards, and a little upwards, forming a continuous membrane attached to the whole of the upper edge of the cricoid cartilage (except where the arytenoid cartilages play), and to the anterior angle and edge of the bases of the arytenoid cartilages. A somewhat increased thickness of this membrane at three parts forms the so-called middle crico-thyroid, and the inferior thyro-arytenoid ligaments; it forms the upper vocal ligaments, which are also connected to the lower by a thin layer of elastic tissue lining the venticulum Morgagni. The same tissue is found also in the lateral hyo-thyroid, thyro-epiglottic, and glosso-epiglottic ligaments; and if to these be added the longitudinal fibres of it in the membranous part of the trachea and bronchi, an idea may be formed of the great extent of walls fitted for vibrations

and resonance in the neighbourhood of the vocal organ.

The vocal ligaments are made to possess various degrees of tension, by the contraction of the several muscles acting on the cartilages to which they are affixed. Thus the crico-thyroid, drawing the thyroid nearer to the cricoid cartilage, increase their tension, while the arytenoid cartilages are fixed; and the posterior crico-arytenoid muscles effect the same change if the thyroid cartilage be fixed, and the arytenoid cartilages be coincidentally approximated by the proper arytenoid muscles. According to the degree of tension produced by these muscles, the glottis becomes longer or shorter. It is made narrower by the approximation of the arytenoid cartilages by the proper arytenoid muscles, and wider by the separation of these cartilages by the posterior crico-arytenoid muscles.

The elasticity of the ligaments connecting these cartilages will maintain the corresponding edges of the thyroid and cricoid cartilages in approximation, without the aid of muscular effort; so that while the arytenoid cartilages are fixed, there will always be a certain degree of tension of the vocal ligaments; and when the posterior crico-arytenoid muscles act, to draw the arytenoid cartilages backwards, they must have this elastic middle crico-thyroid ligament to act against.

The glottis is capable of receiving the following forms:—When at rest it is lanceolate, growing wider on inspiration, and narrower on expiration. Its sides are formed behind by the inner surfaces and anterior processes of the bases of the arytenoid cartilages; before, and to the greatest extent by the vocal ligaments, attached to those processes, and the angle of the thyroid cartilage. When at its greatest width, it has the form of a lozenge, with its posterior angle cut off, and the distance between the lateral angles, at which are the anterior processes of the arytenoid cartilages, may be $5\frac{1}{2}$ lines. In its narrowed condition the glottis may have a triangular form; it may be open in its whole length, the arytenoid cartilages being merely approximated; or by their coming in contact at their anterior angles, a double aperture may be produced; or by their coming completely in contact, the posterior part of the glottis may be entirely closed, and there will then be no opening except that between the vocal ligaments. This last condition is produced by the united action of the lateral crico-arytenoid and the proper arytenoid muscles; the aperture will be pointed both before and behind, and will vary in width according

to the coincident degree of tension of the vocal ligaments.

The loosening and shortening of the vocal ligaments is effected by the thyro-arytenoid muscles, which at the same time narrow the space above and below the lower vocal ligaments.

The exact form of the glottis during the emission of sound is not precisely known, but it is certainly very much narrowed, as is shown by Mayo's observations on those who had attempted suicide, Rudolphi's in a case of loss of the nose and palate, and Magendie's on living animals. Müller thinks, in accordance with the opinions of the last-mentioned physiologist, that the posterior part of the glottis, between the arytenoid cartilages, is generally closed during the production of voice, but as in the dead larynx he does not find this a necessary condition, he does not so decide it.

The following is the mode employed by Müller in experimenting on the production of voice with the larynx after it is removed from the body. The principal objects to be attained are, a fixed position of certain of the cartilages, and at the same time a regular and measurable power of moving others, so as to give the vocal ligaments the desired degrees of tension and varied forms. These he attains by laying the larynx with its posterior wall on a board, and there fixing it firmly by the cricoid cartilage; the arytenoid cartilages are then fixed by an awl or metal pin, run transversely through them both, near their bases. This is the most delicate part of the process, requiring great care, in order that when the extending force is afterwards applied, both vocal ligaments may be made equally tense; and so that when these cartilages are pressed towards each other on the pin, their anterior or vocal processes may touch. The transfixing pin may then be fixed with cords to the board. When this has been accomplished, and the cricoid and arytenoid cartilages are both fixed to the board, any degree of tension may be given to the vocal ligaments by drawing the angle of the thyroid cartilage forward. This is best done, and will admit of being measured, if a fine string be affixed to it just before the insertion of the ligament, then passed over a pulley, and then have hung on at its opposite end one of a pair of delicate scales, which may be loaded with different weights, so as to draw away the anterior part of the thyroid from the arytenoid and cricoid cartilages, as much as the vocal ligaments will allow. In proportion as the weight is increased, the vocal ligaments will now be stretched. All the parts of the larynx above the lower vocal ligaments may now be cut away, as they are unnecessary for the

production of the required sound, and the actions of the ligaments may be better observed when they are removed. A wooden tube to blow through is put into the end of trachea.

The following facts have been observed in the frequent performance of experiments on larynges, thus prepared.

On blowing through the trachea when the glottis is narrow, the vocal ligaments give out full and clear notes, which come very near to those of the human voice, and much resemble those produced by blowing over moist elastic arterial coat, stretched over the end of a tube, as in the best kind of artificial larynx. They differ from those obtained when the ventricle, upper ligament, &c. are affixed, only in being less loud. The vocal ligaments sound best and most easily when the posterior part of the glottis, between the arytenoid cartilages, is closed, though this form is not absolutely necessary; and if the tension of the ligaments be the same in both cases, the note produced is of the same height, whether the posterior part of the glottis be open or not, which is at once an evident proof that it is the vocal ligaments whose vibrations produce the sound, and that the air is not the primary vibrator, for in that case the note would be deeper when the glottis was open to its whole length than when it was open only for the length of the vocal ligaments. And if the anterior angles of the arytenoid cartilages touch, so that there is an aperture both behind and before them, no second note is produced by the second posterior aperture, though the air sometimes rustles, in passing between the cartilages and their uniting membrane.

If the ligaments be equally tense in all cases, the width of the aperture of the glottis does not make any difference in the height of the note produced,—it is only less clear, in consequence of the noise of the air rushing through, when there is a wide opening; and in this, as in many other respects, a larynx exactly resembles the artificial ones made with bands of caoutchouc and instruments with membranous and metallic tongues, in which a wider opening only alters the expression, and not the height, of the note.

If the vocal ligaments be unequally tense, there are rarely two notes produced; but when the tension is equal, there is occasionally produced a much higher note than the proper one, from the ligaments coming in contact in some part of their length. Notes can as well be produced when the ligaments touch each other every where, as when they have a narrow opening between them; and if the tension be the same, the notes produced in these

two conditions are not different as regards their height. In these cases the notes are produced most easily when the ligaments which are in contact are very lax, and they are caused by the interrupted passage of air through them, and may be well produced when the glottis is extremely short.

Low notes may be produced in a very short as well as in a very long glottis, and high notes in a very long as well as in a very short one: if only in the short one for low notes the ligaments are very lax, and touch each other; and if in the long glottis for high notes, the ligaments are at the same time very tense. This shortening and lengthening of the glottis, without alteration of its tension, may best be effected by compressing the lips of the glottis with forceps; while the degrees of tension may be altered by pressing the angle of the thyroid cartilage backwards to the cricoid.

When the whole vocal ligaments, from the angle of the thyroid cartilage to the vocal processes of the arytenoid cartilages (in contact with each other), are in vibration without touching, the notes produced by increasing degrees of tension of the ligaments do not increase in height in precisely the same degrees as those produced by strings and membranes stretched at both ends; there are always some half or whole notes below those given by strings and membranes. For example: with the same length of string, the notes or numbers of vibrations increase directly as the square roots of the powers producing the tension; i. e. if a string, extended by a weight of 4, give the note *c*, it will give the octave or *c'* when extended by a weight of 16, and the second octave or *c''* with a weight of 64. But by adding weights into the scale attached to the thyroid cartilage, as above described, according to this rate of increase, octaves are not obtained from the vocal ligaments, but notes, $\frac{1}{2}$ a note, or $1\frac{1}{2}$, 2, or 3 whole notes below the octaves. However, the analogy is near enough to show that the notes of the human vocal organ, so far as they are produced by the glottis and its limits, are analogous to those of strings and membranous tongues. [The experiments proving this are of very great difficulty, and require considerable practice to perform them correctly, but we cannot undertake to translate the whole of Müller's long directions.]

The following are the results of some of his successful attempts. The degrees of tension were always increased from 4 to 16, and from 16 to 64; and the weights thus employed were *l. oz.*, each about equal to a half ounce in this country.

Weights in Loths.			
Tension .. 4		16	64
1st Experiment.—Notes	<i>c'</i>	<i>a'</i>	<i>g''</i> sharp.
2d do. do.	<i>c'</i> sharp	<i>b'</i>	<i>a''</i> sharp— <i>a''</i>
3d do. do.	<i>g'</i> sharp	<i>c''</i> sharp	<i>c'''</i>
4th do. do.	<i>a'</i>	<i>d''</i>	<i>c'''</i>
5th do. do.	<i>a</i> sharp	<i>f'</i> sharp	<i>g''</i>
6th do. do.	<i>a</i> sharp	<i>g'</i> sharp	<i>g''</i>
7th do. do.	<i>d'</i>	<i>c''</i>	<i>a''</i>
8th do. do.	<i>d'</i> sharp	<i>b'</i>	<i>a''</i>
9th do. do.	<i>g</i>	<i>g'</i>	<i>g''</i>

The two octaves impure.

The notes may be in every case determined by a well-tuned piano, struck by a second person.

The vocal ligaments, separated from the larynx and stretched, act nearly like strings. Thus one gave, with a weight of 16, *a* sharp. When the weight was reduced to 4, it gave *d*; and when it was increased to 16, it gave *a* sharp again.

By altering the tension in the same direction, the notes of the larynx may pass through a compass of two octaves; with greater tension, disagreeable, higher, whistling or shrieking notes are produced. If the experiment shewing this cannot be well performed by increasing the weight, and drawing over the pulley in the direction of the ligaments, it may be done by making a lever of the thyroid cartilage, and drawing it downwards, by a weight attached to a cord, towards the cricoid, in imitation of the action of the crico-thyroid muscles, which act thus in the production of high notes during life, by diminishing the breadth of the crico-thyroid ligament. The following results were obtained by this plan, in two succeeding experiments on the same larynx:—

Experiment I.

Weights.	Notes.
$\frac{1}{2}$ loth	<i>a</i> sharp.
1 —	<i>b</i>
$1\frac{1}{2}$ —	<i>c'</i>
2 —	<i>c'</i> sharp.
$2\frac{1}{2}$ —	<i>d'</i>
$2\frac{8}{10}$ —	<i>d'</i> sharp.
3 —	<i>e'</i>
$3\frac{1}{2}$ —	<i>f'</i>
4 —	<i>f'</i> sharp.
$4\frac{1}{2}$ —	<i>g'</i>
5 —	<i>g'</i> sharp.
$5\frac{1}{2}$ —	<i>a'</i>
6 —	<i>a'</i> sharp.
$6\frac{1}{2}$ —	<i>b'</i>
7 —	<i>b'</i> — <i>c''</i>
$7\frac{1}{2}$ —	<i>c''</i>
8 —	<i>c''</i> sharp.
$8\frac{1}{2}$ —	<i>d''</i>

Weights.	Notes.
$9\frac{7}{10}$ loths	<i>d''</i> sharp.
$10\frac{7}{10}$ —	<i>e''</i>
$11\frac{7}{10}$ —	<i>f''</i>
13 —	<i>f''</i> sharp.
15 —	<i>g''</i>
17 —	<i>g''</i> sharp.
19 —	<i>a''</i>
22 —	<i>a''</i> sharp.
25 —	<i>b''</i>
28 —	<i>c'''</i>
31 —	<i>c'''</i> sharp.
35 —	<i>d'''</i>
37 —	<i>d'''</i> sharp.

No higher note.

Experiment II.

Weights.	Notes.
$\frac{1}{2}$ loth	<i>b</i>
1 —	<i>c'</i>
$1\frac{1}{2}$ —	<i>c'</i> sharp.
2 —	<i>d'</i>
$2\frac{1}{2}$ —	<i>d'</i> sharp.
3 —	<i>e'</i>
$3\frac{1}{2}$ —	<i>f'</i>
4 —	<i>f'</i> sharp.
$4\frac{1}{2}$ —	<i>g'</i> +
5 —	<i>g'</i> sharp.
$5\frac{1}{2}$ —	<i>a'</i>
6 —	<i>a'</i> sharp.
$6\frac{1}{2}$ —	<i>b'</i>
$7\frac{1}{2}$ —	<i>c''</i>
$8\frac{3}{10}$ —	<i>c''</i> sharp.
9 —	<i>d''</i>
10 —	<i>d''</i> sharp.
11 —	<i>e''</i>
12 —	<i>f''</i>
13 —	<i>f''</i> sharp.
15 —	<i>g''</i>
$17\frac{1}{2}$ —	<i>g''</i> sharp.
18 —	<i>a''</i>
20 —	<i>a''</i> sharp.
22 —	<i>b''</i>
26 —	<i>c'''</i>
29 —	<i>c'''</i> sharp.
32 —	<i>d'''</i>
37 —	<i>d'''</i> sharp.

No higher note.

After the first experiment, the vocal ligaments had only altered so much as to give *b* instead of *a* sharp with a weight of a quarter of an ounce. From these experiments it follows, that about a pound of muscular power may produce the notes within the compass of two octaves.

But still lower notes may be produced if the vocal ligaments be freed from every degree of extension exercised by the middle crico-thyroid ligament, and allowed to have the greatest laxity possible, by drawing the thyroid cartilage towards the arytenoid by a cord running over a pulley behind the larynx, with weights attached, as before, to it. In this method, and by blowing upwards through a larynx, Müller obtained, by gradually increasing the weight, almost a complete scale downwards, from *d'* sharp to *B*. In leaving the ligaments thus free from the action of the crico-thyroid ligament, and other parts, while the posterior part of the glottis is closed, the action of the thyro-arytenoid muscle is imitated, in the shortening and relaxation of the vocal ligaments.

[To be continued.]

OBSERVATIONS
ON THE
REACTION OF THE SALIVA UPON
RED AND BLUE LITMUS, AND
TURMERIC PAPER.

To the Editor of the Medical Gazette.

SIR,

A PAPER on some physiological and pathological properties of the saliva, by M. Donné, appeared in the *Archives Générales* of May and June, 1835. M. Donné showed, that by comparing the morbid with the healthy state of this fluid, we may distinguish accurately between inflammatory and non-inflammatory affections of the stomach. He asserts, that on no occasion has he found the saliva acid when the appetite and digestion were good; and states, that acidity of the saliva is a diagnostic symptom of gastritic irritation.

I have misplaced a reference to a French writer who makes a similar assertion respecting the sputa in chronic bronchitis, and states, that by this means it may be distinguished from phthisis, in which the sputa are alkaline.

I have occasionally tested the truth of these observations, and have found the sputa neutral in phthisis. In a fatal case of chronic bronchitis (ascertained to be this disease on dissection,) I found the sputa, at various periods, to be alkaline, acid, and neutral.

From a few desultory observations, I was led to doubt the accuracy of M. Donné's conclusions; finding, however, that that gentleman has the reputation of being a talented and industrious observer, I determined upon a closer line of investigation. The practice of the York Hospital presents but cases of unquestionable gastritis, and I therefore turned my attention to the state of the saliva generally. The observations I made I have subjoined in the form of tables. Their unexpected result has induced me to publish them, hoping the conclusion to which they may lead may attract the notice of M. Donné, and of other inquirers into the pathological states of the fluids of the body. Pathological chemistry will soon rival pathological anatomy in importance.

I made these observations indiscriminately upon forty-eight in-patients of this hospital: of this number there were not three cases alike: it comprised diseased joints in every stage, ulcers, accidents, and the various other diseases usually found in a general hospital. Thirty-six, or three-fourths of the whole number, asserted that they had a good appetite and digestion. This number included the most varied cases: ulcers on the legs, with and without impaired health; diseased hip joint with sinuses, emaciation and diarrhoea; senile gangrene; schirrous uterus, &c. Eight had appetite and digestion more or less impaired; of these, two were under the influence of mercury: four had loss of appetite, or frequent vomiting.

The diet of these individuals was various: a few were on fever and full diet, but the majority on house diet, consisting of beef or mutton, and baked flour or rice puddings, on alternate days, for dinner; and tea, milk, or broth, for breakfast and supper. The latter was taken at half-past five; dinner at half-past twelve; and breakfast at half-past eight. With a few exceptions, no individual took medicine after his meal until I had made the observations.—Slips of paper, an inch and a quarter

in length and a third of an inch in breadth, were used. In the first nine series, litmus and turmeric paper were used, on Sept. 13th (evening.) Litmus paper, reddened with dilute acetic acid, was substituted for the turmeric: in the remainder it was reddened with very dilute sulphuric acid; thirty minims of the official dilute acid being mixed with four ounces of distilled water for this purpose.

The papers were sometimes put into the mouth at the same moment, one being inserted at each end; but generally in succession, with an interval of from one to three or four minutes. The reaction was noted down, as was also the state of the tongue, at the moment of withdrawal. Care was taken that the papers were well moistened.

The whole of the observations, with the exception of those in the two last series, were made at from half-past eight

to nine o'clock in the evening, and from ten to half-past ten in the morning.

Every shade of tint was observed, as will appear from the tables. *Strongly acid* means, that the litmus was immediately and deeply reddened: *acid*, that the reaction was immediate and distinct, but in a less degree; and so on with the others. *Very slightly acid*, and *very slightly alkaline*, express changes not easily perceptible, and which I noted, solely that the observations might be more exact.

The tongue was noted as being *white* when it was of a greyish, milk-like colour, but not coated, and was sometimes moist and sometimes rather dry. The tongue was said to be pale when of a whitish red colour, *smooth* and moist; a state observed most frequently in relaxed habits. The other states mentioned, include various degrees of each, under the same name.

A TABLE of 567 Observations on the Action of the Saliva upon Red and Blue Litmus and Turmeric Paper, and on the corresponding Appearances of the Tongue.

	Strongly Acid.	Acid.	Slightly Acid.	Very slightly Acid.	Neutral.	Very slightly Alkaline.	Slightly Alkaline.	Alkaline.	Strongly Alkaline.	Total.
Sept. 6th—Evening. Tongue not examined....	1	7	8	1	5	0	0	0	0	22
7th—Morning.										
Tongue natural	0	4	10	0	5	0	0	1	0	20
Pale	2	1	6	0	2	0	0	0	0	5
White	1	2	2	0	1	0	0	0	0	6
Coated or furred	1	3	1	0	2	0	0	0	0	7
Having a white fur, dotted with red points ...}	0	0	0	0	1	0	0	0	0	1
	1	10	13	0	11	0	0	1	0	35
7th—Evening.										
Tongue natural	2	7	5	0	5	0	0	0	0	19
Pale	0	2	1	0	1	0	0	0	0	7
White	0	0	2	0	0	0	0	0	0	2
Coated or furred	1	1	1	0	1	0	0	0	0	4
Having a white fur, dotted with red points ...}	0	0	2	0	1	0	0	0	0	2
	3	10	11	0	11	0	0	0	0	35

	Strongly Acid.	Acid.	Slightly Acid.	Very slightly Acid.	Neutral.	Very slightly Alkaline.	Slightly Alkaline.	Alkaline.	Strongly Alkaline.	Total.	
8th—Morning.											
Tongue natural	0	3	2	0	7	0	0	0	0	12	} Morning.
Pale	1	3	2	0	4	0	0	0	0	10	
White	0	0	0	0	1	0	0	0	0	1	
Coated or furred	0	1	3	0	9	0	0	0	0	13	
Having a white fur, dotted with red points ...}	0	0	0	0	2	0	0	0	0	2	
	1	7	7	0	23	0	0	0	0	38	
9th—Morning.											
Tongue natural	0	3	9	0	9	0	0	0	0	21	} Morning.
Pale	0	0	1	0	5	0	0	0	0	6	
White	0	0	1	0	3	0	0	0	0	4	
Coated or furred	0	2	3	0	5	0	0	0	0	10	
Having a white fur, dotted with red points ...}	0	0	0	0	1	0	0	0	0	1	
	0	5	14	0	23	0	0	0	0	42	
11th—Evening.											
Tongue natural	0	1	7	0	2	0	0	0	0	10	} Evening.
Pale	0	1	0	0	2	0	0	0	0	3	
White	0	1	1	0	1	0	0	0	0	3	
Coated or furred	0	3	6	0	0	0	0	0	0	9	
Having a white fur, dotted with red points ...}	0	0	2	0	0	0	0	0	0	2	
	0	6	16	0	5	0	0	0	0	27	
12th—Morning.											
Tongue natural	0	2	1	4	5	0	1	2	0	15	} Morning.
Pale	0	1	1	0	1	0	1	1	0	5	
White	0	1	0	2	2	0	1	0	0	6	
Coated or furred	0	2	0	2	5	0	0	0	0	9	
Having a white fur, dotted with red points ...}	0	0	0	2	0	0	0	0	0	2	
	0	6	2	10	13	0	3	3	0	37	
12th—Evening.											
Tongue natural	2	4	1	8	2	0	0	0	0	17	} Evening.
Pale	0	0	0	1	1	0	0	0	0	2	
White	1	2	0	2	0	0	0	0	0	5	
Coated or furred	3	0	1	1	0	0	0	0	0	5	
Having a white fur, dotted with red points ...}	0	0	1	1	1	0	0	0	0	3	
	6	6	3	13	4	0	0	0	0	32	

	Strongly Acid.	Acid.	Slightly Acid.	Very slightly Acid.	Neutral.	Very slightly Alkaline.	Slightly Alkaline.	Alkaline.	Strongly Alkaline.	Total.	
13th—Morning.											
Tongue natural	0	0	2	7	6	0	0	0	0	15	} Morning.
Pale	0	0	0	0	7	0	0	0	0	7	
White	1	0	0	2	3	0	0	1	0	7	
Coated or furred	0	0	2	2	2	0	0	0	0	6	
Having a white fur, dotted with red points ..	0	0	0	1	1	0	0	0	0	2	
	1	0	4	12	19	0	0	1	0	37	
13th—Evening.											
Tongue natural	4	1	3	6	2	0	0	0	0	16	} Evening.
Pale	0	0	1	2	1	0	0	0	0	4	
White	1	3	2	1	0	0	0	1	0	9	
Coated or furred	1	1	0	0	2	0	0	0	0	4	
Having a white fur, dotted with red points ..	1	0	0	0	1	0	0	0	0	2	
	7	5	6	9	7	0	0	1	0	34	
14th—Morning.											
Tongue natural	1	2	2	4	6	0	0	3	0	18	} Morning.
Pale	0	0	1	1	1	0	0	0	0	3	
White	0	0	0	1	5	0	0	2	0	8	
Coated or furred	1	1	0	0	4	0	0	0	0	6	
Having a white fur, dotted with red points ..	0	0	0	1	1	0	0	0	0	2	
	2	3	3	7	17	0	0	5	0	37	
14th—Evening.											
Tongue natural	3	2	2	2	4	0	0	0	0	13	} Evening.
Pale	0	2	0	0	1	0	0	0	0	3	
White	2	4	0	2	1	0	1	0	0	10	
Coated or furred	2	0	0	2	0	0	0	0	0	4	
Having a white fur, dotted with red points ..	0	1	0	1	0	0	0	0	0	2	
	7	9	2	7	6	0	1	0	0	32	
15th—Morning.											
Tongue natural	1	0	0	2	9	0	1	3	0	16	} Morning.
Pale	0	0	0	0	1	0	3	0	0	4	
White	0	1	1	2	3	5	0	0	0	12	
Coated or furred	1	0	1	1	3	0	1	0	0	7	
Having a white fur, dotted with red points ..	0	0	0	0	1	0	0	0	0	1	
	2	1	2	5	17	5	5	3	0	40	

	Strongly Acid.	Acid.	Slightly Acid.	Very slightly Acid.	Neutral.	Very slightly Alkaline.	Slightly Alkaline.	Alkaline.	Strongly Alkaline.	Total.
15th—Evening.										
Tongue natural	2	2	2	1	1	1	1	3	0	13
Pale	0	0	0	0	0	1	1	1	0	3
White	2	3	2	1	2	0	0	0	0	10
Coated or furred	2	1	1	1	0	0	0	0	0	5
Having a white fur, dotted with red points ..	0	0	0	1	1	0	0	0	0	2
	6	6	5	4	4	2	2	4	0	33
16th—Immediately before breakfast.										
Tongue natural	0	0	0	0	0	0	2	2	1	5
Pale	0	0	0	0	2	0	0	2	0	4
White	0	0	0	1	4	0	4	4	6	19
Coated or furred	0	0	0	0	5	0	2	1	1	9
Having a white fur, dotted with red points ..	0	0	1	0	1	0	0	0	1	3
	0	0	1	1	12	0	8	9	9	40
16th—Immediately before dinner.										
Tongue natural	0	0	0	1	5	1	2	0	5	14
Pale	0	0	0	1	1	0	1	0	0	3
White	0	0	0	1	3	0	1	3	3	11
Coated or furred	0	0	0	1	3	0	0	1	3	8
Having a white fur, dotted with red points ..	0	0	0	1	1	0	0	0	1	3
	0	0	0	5	13	1	4	4	12	39

The next Tables will place the observations taken in the morning in more striking contrast with those taken in the evening.

	Strongly Acid.	Acid.	Slightly Acid.	Very slightly Acid.	Neutral.	Very slightly Alkaline.	Slightly Alkaline.	Alkaline.	Strongly Alkaline.	Total.
Tongue natural	Morning ..									
	2	14	26	28	52	1	6	11	6	136
Tongue pale....	Evening ..									
	13	17	20	17	16	1	1	3	0	88
Tongue white ..	Morning ..									
	3	5	5	2	24	0	5	3	0	47
Tongue white ..	Evening ..									
	0	5	2	3	9	1	1	1	0	22
Tongue white ..	Morning ..									
	2	4	4	9	25	5	7	11	9	76
Tongue white ..	Evening ..									
	6	13	8	6	5	0	1	0	0	39

		Strongly acid.	Acid.	Slightly Acid.	Very slightly Acid.	Neutral.	Very slightly Alkaline.	Slightly Alkaline.	Alkaline.	Strongly Alkaline.	Total.
Tongue coated or furred	Morning ..	3	9	10	6	38	0	3	2	4	75
	Evening ..	9	6	9	4	3	0	0	0	0	31
Tongue having a white fur, dotted with red points	Morning ..	0	0	1	5	9	0	0	0	2	17
	Evening ..	1	1	5	3	4	0	0	0	0	14
Total of evening observations, including Sept. 6th		30	49	52	34	42	2	3	4	0	216
Total of morning observations ..		10	32	46	40	148	6	21	27	21	351
Total of both		40	81	98	74	190	8	24	31	21	567

I would remark, that the three central degrees of reaction are very nearly allied. The same remark is applicable to the observations in the three columns

to the right and left. It will be instructive to throw these together under the heads of acid, neutral, and alkaline, as follows:—

	Acid.	Neutral.	Alkaline.	Total.
Morning observations	88	194	69	351
Evening observations.....	131	78	7	216

What conclusions, then, must be drawn from the preceding tables? The following appear to me correct:—

1. The saliva may be acid without any apparent disease of the stomach, and when the individual is in good health.

2. It is alkaline during different degrees of gastric derangement, as indicated by the tongue.

3. It may be alkaline, acid, and neutral, when the gastric phenomena are the same; and, consequently, acidity of the saliva is not a diagnostic mark of gastritic derangement.

4. In general it is alkaline in the morning, and acid in the evening.

The fourth conclusion will require some explanation, since it is obviously contrary to the earlier series, although in accordance with the general results. It must, however, be remembered, that the alkalinity of the saliva was tested, in the first nine series, by turmeric pa-

per, which is not a sufficiently delicate re-agent for the purpose. The consequence would be to place those in which the saliva was alkaline, under the head of neutral; leaving the results obtained by means of the litmus unaffected. Therefore we find, that on the five first mornings (on which the turmeric paper was used) of 196 observations, the saliva was neutral in 99, and alkaline in *one* only; whereas on the succeeding mornings (red litmus being used), of 156 observations, it was alkaline in *seventy-three*, and neutral in *fifty-nine* only.

It may be also remarked, as opposed to this conclusion, that in the first five morning observations, of the gross number 196, the saliva exhibited various degrees of acidity in *ninety-six*; while, in the succeeding morning observations, this was observed in *thirty-two* only of 156.

I cannot explain these discrepancies,

except by supposing that during the first experiments, the novelty of the procedure and the evident expectation of tasting something, on the introduction of the test-papers into the mouth, caused the saliva to be temporarily acid;—a supposition in accordance with our knowledge of the influence of mental emotions on the secretions in general.

The uniformity in the evening observations is at least curious, and appears to me highly corroborative of the truth of the fourth conclusion.

Assuming as a general fact, then, that the saliva is acid in the evening and alkaline in the morning, it remains to inquire under what circumstances it varies from these states.

From the few observations I have made, it is not improbable that the acidity of the morning saliva is a symptom of disease, but not exclusively of the gastritic form. I subjoin a few cases selected from the mass:—

1. Reuben Jones, aged 30, admitted suffering from retention of urine. He has a stricture of the urethra, which admits only the smallest bougie. General health good. Tongue natural.

2. William Walker, aged 60, complained of difficulty of breathing, sleeplessness, frequent micturition, and pain about the rectum and perineum. He has a large calculus in the bladder*. Appetite unimpaired; tongue white. Has gruel diet, and takes an opiate at night. Pulse regular.

3. John Bowes, aged 21, suffers from disease of the hip-joint; sinuses run into it, and discharges profusely. He is emaciated, and suffers occasionally from diarrhœa, which is kept in check by Dover's powder combined with mercury and chalk, at bed-time. Appetite good; tongue pale; pulse quick.

4. Fanny Tate, aged 30, has serofulous abscesses in the neck, and severe neuralgic pains of the occiput and temples. Countenance sallow and anxious; movements languid; appetite somewhat impaired; tongue pale; pulse regular.

5. Thomas Ruddock, aged —, has abscess in the left knee-joint, following an injury. Is sleepless and feverish; pulse excited; appetite somewhat impaired; tongue furred.

6. John Porteus, aged 63, had his leg amputated below the knee ten days ago, (August 27th.) for ulceration of the cartilages of the ankle-joint. He is emaciated, his constitution is impaired, and he has occasional diarrhœa. Pulse rapid; tongue with a white coat and a few prominent papillæ. The stump healed by the first intention.

7. George Shepherd, aged 63, has languid ulcers on the legs; is pale and weak; and vomits almost every thing he takes. No tenderness of epigastrium; tongue thickly coated.

I subjoin a table of the observations made upon the saliva of these patients:—

		Acid.	Alkaline.	Neutral.
Jones	{ Morning	4	0	0
	{ Evening	3	0	0
Walker ..	{ Morning	7	2	0
	{ Evening	7	0	0
Bowes ..	{ Morning	6	2	0
	{ Evening	3	0	0
Tate	{ Morning	4	2	3
	{ Evening	4	0	1

* Mr. Champney performed the operation of lithotomy upon this patient on the 20th Sept. and removed a large stone, shaped like an equila-

teral triangle, having uric acid nuclei and a phosphatic crust. The patient is doing well.

		Acid.	Alkaline.	Neutral.
Ruddock.	{ Morning	3	2	4
	{ Evening	5	0	0
Portens ..	{ Morning	6	0	3
	{ Evening	4	0	1
Shepherd	{ Morning	7	2	0
	{ Evening	7	0	0

The state of the atmosphere seemed to influence the re-action of the saliva.

Should any of your readers be inclined to prosecute these researches, I would just remark, by way of caution, that the changes in the colour of the papers should be noted immediately, as an evident tendency to acidity is very soon observed. I took particular care to notice this, and found that in 308 of my observations, at the end of six hours the colour of the paper was unchanged after the first action of the saliva; in 250 there was an evident tendency to acidity; alkaline becoming neutral—neutral, acid—and acid, strongly acid; while in nine only was there a reverse tendency.

These observations plainly lead to conclusions opposed to those of M. Donné. If his inquiries have been made principally with the morning saliva (which is very probable), the conflicting statements may, in some degree, be harmonized.—I am, sir,

Your obedient servant,

T. LAVCOCK,
House Surgeon, &c.

York County Hospital,
Sept. 25, 1837.

CASE OF CHRONIC LARYNGEAL DISEASE, COMPLICATED WITH PHTHISIS.

To the Editor of the Medical Gazette.

SIR,

THE accompanying case of chronic laryngeal disease, complicated with phthisis, and in which it was thought

necessary to perform tracheotomy, appears to me to offer many points of practical instruction. I therefore forward it, with some remarks, for insertion in the *MEDICAL GAZETTE*.

Your obedient servant,

GEORGE BURROWS, M.D.,
Assistant Physician to St. Bartholomew's
Hospital.

45, Queen Ann Street, Cavendish Square,
Sept. 30, 1837.

Thomas Lockwood, æt. 37, a carter, was admitted into St. Bartholomew's Hospital under my care, September 14th, 1837, in the following condition:—Pale, and somewhat emaciated; countenance rather anxious; skin warm and dry; voice gone, so that he can only speak in a hoarse whisper. He complains of uneasiness in the throat, which he refers to the top of the thyroid cartilage, where he feels as if the passage were "blocked up" during inspiration: there is actual pain felt when the thyroid cartilage is compressed between the thumb and finger. Difficulty of swallowing, especially liquids, which excite cough. Nothing morbid can be seen in the fauces. He has a constant cough, and expectorates a muco-purulent fluid, which is rather viscid. Difficulty of breathing, requiring the head and shoulders to be elevated, much increased by any exertion; no pain in the chest at present. Pulse 108; very thirsty; tongue moist, and whitish in the centre; appetite good; bowels open twice in the last twenty-four hours; evacuations loose.

Auscultation.—Air appears to pass freely through the lungs before and behind; no unnatural sounds are detected, but the loud tracheal sounds greatly interfere with the respiratory murmur in the lungs.

History.—He stated that he had been much exposed to the weather, and had been intemperate. Two years ago, in the winter, he had had pain in the chest, with cough. Last winter he again had pain in the chest, with cough. He has been unable to follow his employment for six weeks. A month ago his throat began to inconvenience him as at present, and has remained, during that period, much in its present condition. It appears that he has already had leeches and a blister applied to the throat.

The case was regarded as one of chronic laryngitis, consequent upon attacks of active inflammation of the mucous membrane of the trachea and bronchi. He was ordered eight leeches to the larynx; calomel, grs. ij., with opium, gr. $\frac{1}{4}$, night and morning, and acetate of morphia, gr. $\frac{1}{8}$ th, three times in the day.

The objects attempted by this plan of treatment were to subdue any local congestion of the larynx, to bring the system gradually under the influence of mercury, and allay the irritability of the glottis by the sedative.

15th.—The throat was rather easier after the application of the leeches, and deglutition performed with rather less difficulty. Six more leeches to the larynx.

16th.—He was found, this morning, at 5 A.M. by the nurse, insensible, breathing with extreme difficulty, and the face livid. Upon elevating his head, and giving him some wine and water, he partially rallied; but at ten o'clock A.M. he was found by the clinical clerk still insensible, covered with a profuse perspiration: his countenance pale, lips livid, the pupils contracted, the breathing laboured, the pulse 140. At 2 o'clock P.M. the breathing had gradually returned to nearly the same condition as when he was admitted: his pulse, however, was 140, and feeble; the finger nails were livid, and the respiration was carried on with considerable effort, and he had expectorated a large quantity of muco-purulent fluid.

Regarding the case as one of confirmed chronic laryngitis, and anticipating the recurrence of similar attacks of obstructed respiration, one of which would most probably terminate his life, I requested the opinions of my colleagues on the propriety of performing tracheotomy. I considered this opera-

tion not only justifiable, but absolutely essential to the man's safety, and by thus putting the larynx at rest, a chance would be afforded of subduing the serious disorganization going forward. In consultation on the case, much difference of opinion existed upon the propriety of an operation; and as there was at the moment no actual threatening of death from asphyxia, it was agreed to postpone the tracheotomy until more urgent necessity should arise.

He was ordered to take the Calomel and Opium pills every six hours; to continue the Morphia; and 5j. of strong Mercurial ointment to be rubbed in at the axilla.

The house-surgeons were also requested to lose no time in performing tracheotomy, should the paroxysm of dyspnoea return in the ensuing night.

17th, 10 o'clock, A.M.—He has had a quiet night, with sleep since 3, A.M. There has been no return of the extreme difficulty of breathing. His countenance is expressive of anxiety; the forehead covered with cold perspiration; the finger nails livid; pulse 140, and feeble. He says the breathing is easier, but it is evidently performed with considerable effort; tongue moist, furred in the centre, red at the tip and edges; bowels not open. He is evidently drowsy from the effects of morphia.

Auscultation.—The respiratory murmur is less audible over the mammary region of the right side than elsewhere; there is a suspicion of large crepitation about the scapulae. Both sides are resonant to percussion in the upper part of the chest, in front.—Discontinue the morphia, and in other respects to pursue the same plan of treatment.

About 4 o'clock, P.M. he was seen by my colleague, Dr. Roupell. It appeared to him that the closure of the glottis was becoming more complete, the difficulty of breathing increasing, and the risk of the man's life imminent, so that, although Dr. Roupell was opposed to the propriety of the operation at the previous consultation, he now made up his mind that it must be performed without delay. Mr. Stanley was sent for, and upon his arrival, he immediately agreed to the necessity of tracheotomy. After some slight hesitation the patient consented, and conducted himself throughout the operation with absolute stoicism. No-

thing untoward occurred in the operation, excepting the escape of a small quantity of blood from a vein, which was secured by a ligature.

A portion of the third and fourth cartilaginous rings of the trachea were removed, but its lining was too irritable to bear the introduction of a tube. The relief afforded to the patient was obvious, and which he manifested also by signs with his hand. He was left for the night with a nurse sitting by his bed, to assist him to food, and to watch him carefully. All remedies were suspended. Upon giving him some warm milk after the operation, it was observed to flow in part from the opening in the trachea. I should add that, upon subsequent occasions, when I saw him take fluids, none escaped from the tracheal opening.

The escape of liquids from the wound might give rise to the suspicion that the œsophagus was punctured by the knife of the operator. The dexterous and cautious manner in which the operation was performed assured us that such was not the cause of the phenomenon: it has been observed in other cases of disease and injury to the larynx, and no doubt arose from imperfect action of the arytenoid muscles which should close the glottis during deglutition.

18th.—He passed a quiet night, with sleep. His appearance is much more favourable; the skin is warm, and has lost its livid tint; pulse 120, with more power; breathing easier; the air passes principally through the opening in the trachea, although a considerable quantity passes through the nostrils. Whenever he swallows, cough is excited, and purulent mucus expelled both from the tracheal opening, and from the mouth. Pressure on the alæ of the thyroid cartilage still causes pain; tongue rather dry, coated with whitish fur; no mercurial fetor; one natural stool.

Eight leeches were applied to the larynx. Calomel, gr. iij., with Op. gr. j, to be taken night and morning. His diet to consist of warm milk and broth.

19th.—He has passed a restless night, without sleep; this morning (11, A.M.), the pulse is very frequent, and almost imperceptible; the nails are blue; he is unable to swallow, and is rapidly sinking. He died shortly afterwards, without any further change in the symptoms.

Examination of the body six hours after death.—*Larynx*.—The mucous membrane covering the arytenoid cartilages very much thickened and œdematous; a small ragged excavated ulcer existed on the superior edge of the right arytenoid cartilage; other small deep ulcers in the ventricles of the larynx; the whole mucous membrane of the larynx œdematous.

The trachea presented several patches of ulceration of considerable extent; in two or three situations the mucous membrane was entirely destroyed, and on the œsophageal aspect of the trachea, nearly opposite the opening, there was a small abscess in the submucous cellular tissue. An ulcer was situated close to the artificial opening in the trachea, and another a little below had completely laid bare the cartilages of the trachea. The lining of the trachea and bronchi intensely red. No ulceration or perforation of the œsophagus.

On opening the chest the lungs did not collapse; the two surfaces of pleura over the upper parts of the left lung were adherent by loose cellular tissue; the pleuræ at the upper part of the right lung were firmly adherent by condensed cellular tissue; in the lower part of the right pleura there was considerable effusion of straw-coloured serum, with shreds of lymph; the pleuræ in this part was enormously thickened by adventitious layers of coagulated lymph. In the apex of the right lung was a vomica which would have contained a good-sized orange. Throughout the right lung were numerous crude tubercles, advancing to the stage of softening; the substance of the lower lobes was considerably condensed. The left lung contained numerous granular tubercles in their nascent stage, but was otherwise healthy.

The heart healthy; the right cavities and coronary vessels distended with dark blood. The liver granulated; the kidneys healthy.

REMARKS.—The perusal of the foregoing case will naturally give rise to numerous reflections. Some will immediately say, how could such an error in the diagnosis of such extensive disease of the lungs be made by any one accustomed to employ auscultation? Others may think that the operation of tracheotomy ought not to have been performed excepting to rescue the pa-

tient from impending suffocation. To those who are accustomed to employ auscultation only occasionally, and are acquainted merely with the signs of disease made known to us through the sense of hearing in simple lesions of the lungs, it may appear culpable ignorance in the auscultator, that the cavity and pleuritic effusion in the right side of the chest were not detected. It would take greater space than is here allowable to defend the physician from such a severe condemnation. "The most experienced in the use of the stethoscope," writes the late Mr. John Wood, in his excellent paper on Inflammation of the Larynx and Bronchotomy (Medical and Chirurgical Transactions, vol. xvii.), "have regretted the little advantage derivable from its application in these cases, in consequence of the diminished supply of air to the lungs, and the noise and confusion produced by each violent effort of inspiration." In the Dublin Hospital Reports (vol. v. p. 80), Dr. Graves and Dr. Stokes have remarked, that "the difficulties which disease of the glottis causes, in deciding upon the exact state of the lungs, have not been sufficiently dwelt upon by authors. In point of fact, whenever the disease in the larynx is sufficient to cause sibilous breathing, all the stethoscopic phenomena depending on the characters of the respiration are singularly modified. . . . A knowledge of this fact is of great practical importance in investigating the state of the lungs in persons labouring under chronic laryngitis; an inquiry which ought never to be omitted, on account of the frequent occurrence of tubercles in persons afflicted with this disease. Thus we have seen *several cases of phthisis*, in which extensive excavations and numerous tubercles were found in the lungs on dissection, and yet during life no unequivocal evidence of this state of the lungs had been derived from the use of the stethoscope. The extreme weakness and altered tone of the voice in this disease (chronic laryngitis), deprives us of the assistance which the discovery of bronchophony, or pectoriloquism, would give in forming our diagnosis; and the protracted and gradual inspiration either prevents or renders very indistinct the valuable phenomena of crepitation, gargouillement, cavernous, bronchial, and puerile respiration. Under these circumstances, the general symptoms and percussion

are our only guides, and we have already seen how uncertain both these are."

Such opinions, emanating from these able physicians, are fully supported by the details of the foregoing case, wherein persons accustomed to an extensive employment of the stethoscope, from its first introduction into this country, were unable to detect the structural changes which had taken place in this man's lungs. But those who imagine that the stethoscope will never lead the careful observer into error, are recommended to peruse the admirable lectures of Dr. Latham, "On Subjects connected with Clinical Medicine." In Lecture 14, he treats "of the possible fallacies of auscultation—how it may lead to an erroneous diagnosis in pneumonia—in dilatation of the bronchi—in emphysema of the lungs;" and I might add (as has already been observed by Drs. Graves and Stokes), "in laryngeal obstructions."

But why were not the cavity and the pleuritic effusion discovered in the foregoing case? It is well known that pectoriloquy, gargouillement, and cavernous breathing, are the pathognomic signs of a cavity in the lungs. Our patient in this case was nearly in a state of aphonia; he could only "speak in a hoarse whisper." It was therefore hardly possible that pectoriloquy should be heard when the vocal sounds were so faint. The effect of coughing produced so much pain and distress in this case, that we not only refrained from asking the man to cough, while the ear was in apposition to the chest, but we were actually compelled to remove the ear from the chest as soon as the coughing commenced. This, probably, is the reason why no gurgling sounds were heard beneath the right clavicle, the seat of an extensive vomica. Perhaps cavernous breathing ought to have been detected in this same situation; but let it be borne in mind, that in these cases the loud tracheal sounds pass along the bronchi to every part of the lungs, and so embarrass the auscultator, that it is very difficult to distinguish cavernous, bronchial, and tracheal sounds, from each other.

Perhaps it may be said that the history and constitutional symptoms should have informed the careful physician that this was a case of phthisical laryngeal disease. The history, such as

could be obtained from a man who spoke with difficulty and pain, was not that of phthisis. He acknowledged no previous attacks of hæmoptysis nor diarrhoea, and, moreover, gave us accounts of distinct attacks of acute inflammation during the two preceding winters; besides, what hospital physician is not aware how little he can gain from the histories of urgent cases, as detailed by hospital patients? The constitutional symptoms arising from chronic bronchitis and phthisis are so similar, that it is almost impossible to make a diagnosis between the two without the aid of the stethoscope. But why was not the pleuritic effusion discovered by auscultation and percussion? In the hospital case-book it stands recorded, after as careful an auscultation, by Dr. Roupell and myself, as the man's strength would permit, "the respiratory murmur is less audible over the mammary region of the right side than elsewhere." Thus far the auscultation was correct, for that was the situation of the effusion; but our inferences were wrong. We immediately concluded that the right lung was not so good as the left; but all our suspicions being directed to the existence of tubercular cavities in the upper lobes, their usual situation, and the man never having spoken of pain in the right side, the pleuritic effusion was not detected.

Such appear to have been the various causes which completely masked those signs which would have enabled us to have formed a more correct diagnosis of the extent of disease in the lungs, beyond the laryngeal disease, which was ascertained on the admission of the patient.

The grounds on which the propriety of the operation was supported, have already been adverted to in the recital of the case; they are similar to those advocated by the late Mr. Wood. (Medical and Chirurgical Transactions, vol. xvii.) That much-lamented and talented young surgeon, after pointing out the importance of minute diagnostic research in these cases, and how the co-existence of pulmonary lesions will interfere with the prospect of a favourable issue, remarks, "We are not, however, to be deterred from operating, either from the supposition or assurance that the lungs are simultaneously disordered with the larynx. If the cure of the disease were therefore

out of the question (of which we cannot be positive), the operation, by lessening the frequent necessity of coughing to get rid of the increased mucous secretion, and the consequent fatigue to the lungs, at a time when their condition requires all possible repose, might yet obtain for the patient alleviation of distress and prolongation of life.

"The advantages of performing bronchotomy *very early* in cynanche laryngea, and the dangers attending its delay, have been exposed by Mr. Lawrence (Med. and Chir. Trans. vol. vi.) The Germans, however, claim the merit of having advised and practised the operation in cases of chronic inflammation of the larynx, where no *immediate assistance* was required to preserve life."

Upon these principles Mr. Wood advocated a much earlier resort to the operation than is usually adopted. He says, "when the symptoms of laryngitis are developed, and the inefficacy of ordinary means is evident from the daily increasing distress and weakness of the patient, from the failing voice, difficult respiration, and exhausting cough, tracheotomy should be at once performed; for if delayed to a later period, the chance of recovery will be materially lessened, and often entirely lost. The surgeon, too, often waits for symptoms which threaten immediate suffocation. Yielding to the fears and objections of the sick person and his friends, or similar considerations, he defers the use of the knife until extreme distress induces the dying man to submit to any mode of obtaining relief; and thus a measure most salutary, when adopted at the proper period, is brought into disrepute, because its employment is postponed till a favourable result is no longer possible."

Considerations analogous to the foregoing induced me to recommend the operation immediately after the first attack of obstructed respiration, in the case above recited.

Much additional relevant quotation might be adduced from a learned and elaborate paper by Mr. Lawrence, in vol. vi. of the Medical and Chirurgical Transactions; and to this paper is appended an interesting case, with observations, by Dr. Latham, who remarks, that "to vindicate the propriety of this operation, it is enough to know the situation of the disease; neither the his-

tory nor symptoms will fix its precise nature. The distinction of the cause will be lost in the magnitude of the irritation; so that whether there be inflammation of the membrane, ulceration of the cartilages of the larynx, or abscess in the cellular substance, or accidental lodgment of an extraneous body, the symptoms will probably be the same."

Thus much, then, for the difficulties and obscurity which attends laryngeal disease. It is hoped that, if the foregoing case does not reflect credit on the physician who treated it, that it may prove useful to his pupils and professional brethren.

ACUPUNCTURE IN HYDROCELE AND IN ASCITES.

To the Editor of the Medical Gazette.

SIR,

HAVING in a former letter hinted that the puncturation for the radical cure of hydrocele differed in some respects from that employed in the palliative treatment, although I have seen radical cures take place from the simple operation of a single puncture, yet, as acupuncture is not a radical cure in *all cases*, I beg to submit to my professional brethren the result of my own experience.

My method is to make a single puncture twice a week, for the first few weeks, so as to keep the tunica vaginalis in a collapsed state, and to bring on a healthy instead of a morbid secretion: by so doing, I find that the disposition to secrete water diminishes, and, therefore, if on examination I find only a drachm of fluid, I puncture. By pursuing this method, and employing at the same time the under-mentioned ointment* twice a-day, a radical cure is effected very speedily in recent hydrocele; but when the hydrocele has been of ancient date, the tunica vaginalis is so thickened that, although the fluid is absorbed, the parts do not appear to diminish, the thickening of the part preventing any evident contraction. Our object ought to be to simplify as much as possible the operations of surgery, and if we are desirous of emptying

the fluid at once, it can be done by a grooved needle without a canula; thus avoiding all unnecessary employment of instruments.

M. Velpeau, it seems, according to the accounts in the medical journals, tried puncturation in three cases of hydrocele without effect. Now if the puncture is made properly (and this can only be ascertained by a drop of water following the withdrawing of the needle), the absorption of the fluid will inevitably take place; although, where there is great thickening of parts, the volume of the scrotum is not so visibly lessened as in other hydroceles.

A valuable life ought not to be endangered by doubtful or hazardous experiments, although they may in some cases effect a speedy cure. Injections are strongly recommended, but they have frequently been productive of very serious, indeed fatal, results: it matters not what substance is employed, whether simply cold water, or wine and water, or the alcoholic solution of iodine so much lauded by M. Velpeau; the beneficial effects of which may, perhaps, depend more upon the alcohol itself than upon the iodine with which it is combined.

In a former letter I stated that Dr. Davis saw three cases of empyema get well after puncturation. I have now the additional satisfaction of recording the experience of Mr. King, who has had the courage to step out of the beaten track of routine practice, and, from analogical reasoning, to employ a new and, as it seems, a successful mode of treatment in *ascites*. Mr. King writes as follows:—

"6, Maddox-street, Sept. 25.

Dear Sir,—Having, in conjunction with a confrère in the profession (Mr. L'Estrange), applied acupuncture with some advantage to the treatment of ascites, I deem it right to make you acquainted with the circumstances of the case.

The subject of the operation is a tall man, of a constitution originally strong, but now a good deal worn, and I think between 50 and 60 years of age. The disease showed itself in July, and the patient remained a fortnight without medical aid. The man sent for Mr. L'Estrange, who, by the skilful administration of diuretics, obtained some

* R Hydriodatis Potassæ, ʒss.; Ung. Cetacci, ʒiss. M. f. unguentum.

amelioration. We first met in consultation on the 16th of August, and, after a few days, added mercury to the other means employed. As this was without benefit, and the respiration seemed much impeded at night, we thought of tapping; but the patient's strength was at so low an ebb that we did not feel certain that he could bear it. Upon deliberation, we were of opinion that some benefit might accrue to him from puncturing the abdomen with a long needle (of the size used for darning), so as to give the effused serum an opportunity of oozing from the peritoneal sac into the subcutaneous cellular tissue. The first puncture was made September 2d, on the right side, midway between the umbilicus and spine of the ilium, and was followed, as I withdrew the needle, by the issuing of three or four drops, one by one, of yellow serum. September 3d, another was made, anteriorly to the first. September 4th, there was a marked diminution in the tension. The abdomen was quite altered to the touch; the cellular tissue, which, in this region is two inches thick, presenting an œdematous state, in lieu of the tense unyielding condition it was in prior to the operations. A third puncture was made between the other two. September 7th, I made two more punctures.

9th.—Amelioration still marked, and the patient's general health is improving.

As every operation was followed by some amendment, I made three punctures on the 11th, three on the 15th, three on the 18th, five on the 20th, six on the 21st, six on the 22d, and seven this day. The result may be stated to be a diminution of four inches in the circumference of the abdomen, and an amendment of the patient's general health, such that he is able to walk out.

My present opinion is favourable to the employment of puncturation as a palliative. In some cases of ascites and empyema, when there is no organic disease, might not a cure be obtained by it?

If you think it may be useful to make this case known to the profession, you are quite at liberty to publish this letter.

Yours very truly,

T. KING.

To D. Lewis, Esq.

As the hydriodate of potash is an excellent adjuvant to puncturation in the cure of hydrocele, perhaps it may be

found to be equally efficient in other dropsies. The field is open for further experiments.—I remain, sir,

Yours truly,

D. LEWIS.

28, Artillery Place, West.
Bunhill Row, Sept. 26, 1837.

REMARKS ON THE USE
OF
TARTAR EMETIC AND OPIUM IN
SPASMODIC AFFECTIONS.

To the Editor of the Medical Gazette.

SIR,

AMONGST the various affections which engage the attention of the general practitioner, few are of more frequent occurrence than those denominated spasmodic. The suddenness of these attacks, with the fearful appearance of the symptoms, generally throw the friends of the patient into a state of the utmost alarm; it is therefore during the paroxysms that medical aid is most frequently sought. These circumstances, taken in combination with the anxiety which every medical practitioner has to relieve the extreme suffering in his patient, render the knowledge of such remedies, or such mode of treatment, as will certainly and speedily arrest the affection, exceedingly desirable. Every practitioner will, without doubt, endeavour to allay the natural feelings of alarm that arise in the minds of those by whom the patient is surrounded, by assuring them that the disease is not, generally speaking, of a dangerous nature. But this is not sufficient: the affection may progress from hour to hour, from day to day, notwithstanding all his remedies: one paroxysm may succeed another, until the strength of the patient is worn out, and exhausted nature at length finds repose in sleep. Such, I believe, is the general routine of the disorder. The remedies usually set forth in systematic works have been found, in many cases, totally inadequate to the desired end; so far, at least, as the paroxysm or fit is concerned. It is to this latter stage of the affection only, that the following remarks are intended to apply: the after-treatment will, of course, depend on the causes which have given rise to it.

In hysteria, for example, which, according to Cullen and other nosologists,

is placed in the class Neuroses, and order Spasmi (for the paroxysm is, undoubtedly, of a spasmodic nature) we have tonic contractions of various parts of the muscular system, occurring as well in the involuntary as in the voluntary muscles. These are dependent, I conceive, on a highly excited state of the brain and nervous system; and the tonic contractions or spasms continue more or less during the paroxysm. It may be argued that this state of the brain and nervous system is not primary, but only symptomatic of some local affection of a distant organ, and that we should therefore direct our attention to the "origo mali," if we hope to be successful in our treatment. But how frequently does it happen, in the treatment of a case, that we are obliged to give up for a time the remedies which are calculated to strike at the root of a disease, in order to combat some particular symptom which may have assumed a threatening aspect, and thus, as it were, for a time lose sight of those principles which form the basis of our treatment. In like manner, in a case of hysteria, I contend that we must leave the particular cause to after-treatment, while we direct our attention to the more formidable symptoms which manifest themselves during the paroxysm.

Now the whole class of antispasmodics, as they are termed, (and these are the remedies chiefly resorted to,) are, with scarcely a single exception, stimulants, and stimulants of a powerful nature. Such medicines, in this state of the system, I should say are not only inapplicable, but prejudicial. For what is the effect of a stimulant applied to a nervous trunk, but to increase its energy, and, consequently, the contractile power of the muscle it supplies? Or, if applied to the nervous extremities, as the surface of the stomach, an increase of action is the consequence. A stimulant, in fact, I need scarcely say, is "that which rouses the action or energy of a part," and, as before observed, all the medicines set down as antispasmodics are stimulants. What result, therefore, are we to anticipate from their employment in hysteria, spasm of the stomach, of the diaphragm, &c. but that they should increase the affection they were intended to subdue? By reasoning from cases somewhat analogous we shall arrive at similar conclusions. Who, for instance, would think of giving ammonia or assaetida to relieve a spasmodic stric-

ture? Who would prescribe a dose of æther or musk to allay the tonic contraction of the muscles about a dislocated joint, in order to facilitate its reduction? In a case of strangulated hernia, would any benefit be expected to accrue from the administration of camphor, valerian, castor, &c.? Such medicines would undoubtedly increase the spasm.

These views have been confirmed by incidental results in my own practice; and I appeal to the experience of other practitioners, whether their most strenuous endeavours to subdue an attack by the medicines above specified have not frequently proved abortive, even when assisted by the powerful aid of the lancet? Such must have been the result, I have no hesitation in saying, in very many instances. I could cite several cases of this description which I have attended, both alone and in conjunction with other practitioners, in which the disease has been prolonged during two and even three days under such a mode of treatment. I say prolonged, for I have now reason to be convinced, that by the employment of other means the symptoms might have been allayed in as many hours.

By what I have previously stated we are led to the conclusion, either that the affection is of a most obstinate nature, or that the general practice is founded on fallacious principles. The latter, I believe, is the case. It is necessary, therefore, that we should look for some other remedial agent possessing the power of relieving the excited state of the brain and nervous system, and thus allaying the tonic muscular contractions which characterize this class of diseases.

The remedy which I propose, and it is one which I have used with the greatest success for the last three years, is the tartar emetic, in combination with opium in some of its forms. The sedative effects of the opium are powerfully increased by uniting with it the antimonial salt, while its narcotic properties are diminished. The manner in which I prescribe it is the following:—Three or four grains of tartar emetic, with two drams of laudanum or one of Battley's sedative liquor of opium, and two ounces of water. A tea-spoonful of this mixture to be given every fifteen minutes, until relief is obtained, and afterwards every hour, until all symptoms of the affection have disappeared. Its first effect is generally to produce nausea, or even vomiting. The latter I encourage,

where, as is frequently the case, I have every reason to suspect that the spasm proceeds from improper or undigested food in the stomach, acting as a source of irritation. After this, the medicine is generally retained; *tolerance*, as Rasori describes it, being established, and the spasms speedily subside. It may be given in spasm of the stomach, diaphragm, spasmodic asthma, and during the paroxysm of hysteria, with the most beneficial results. I have administered it in such cases in the advanced stage of pregnancy; nor do I consider an irritable state of the stomach, with vomiting, any objection to the use of it, the sickness generally subsiding after the second or third dose. Should the symptoms be very urgent, if the case will in other respects allow of it, I do not hesitate to bleed, as a most powerful auxiliary; but phlebotomy may in general be dispensed with. Enemata are also at times beneficial, particularly where the attack is accompanied by constipation, though the tartar emetic sometimes produces evacuations from the bowels, and renders their administration unnecessary.

I am of opinion that this salt possesses a peculiar controlling or sedative power over the brain and nervous system. The beneficial result of Dr. Graves's treatment of the latter stage of typhus, where there was great cerebral excitement, with tartar emetic and opium, as recently published, bear me out in my conclusions. Rasori describes the operation of tartar emetic in large doses as *counter-stimulant*. And Dr. W. Philip speaks in terms of high commendation of the soothing effect of a combination of this salt with henbane, in cases of extreme nervous irritation; and even in maniacal cases, he says, "composure may generally be obtained."

While on this subject, I may also state that I can bear testimony to the good effects of the combination to which I have named, in the treatment of the more sthenic forms of delirium tremens, as recommended by M. Lepelletier, in his recent work on Tartar Emetic. He, however, appears to have given it alone, after large doses of opium had failed in procuring sleep. This is another strong proof of its sedative power.

In illustration of the above views, I could record a number of cases of which I have notes; but since the treatment is so very simple, I shall merely recom-

mend a trial of it to your numerous readers, being myself fully convinced of its efficacy.

There are, however, some other affections in which I have reason to believe that beneficial results might be obtained by a vigorous administration of these remedies; but as I have not yet had opportunities of putting them to the test, I shall merely make a few observations on the subject, which future investigation and experience may either confirm or set aside.

The diseases to which I refer are tetanus and hydrophobia. The former is undoubtedly a spasmodic affection, and very much allied in its pathological characters to those before mentioned. Dr. O'Beirne, in speaking of tetanus, says*, "he believes it to be seated in the anterior spinal cord, and probably in some parts of the brain. The change in these parts is only functional, and not connected with inflammation or organic lesion. All the symptoms are therefore the product of muscular action, dependent upon an accumulation or increased intensity of the *motific principle* residing in the anterior spinal column." In conformity with the above views, the remedial measure on which he places the most reliance is the use of tobacco enemata thrown into the colon. Now considering that these views are correct, I would suggest the employment of tartar emetic and opium in preference to the tobacco enemata: its action is on the same principle, and its use not attended with the same dangerous results which have frequently followed the employment of this powerful narcotic. The desperate character of the disease is no argument that we should resort to desperate remedies, provided safer ones will answer the purpose.

The latter affection (hydrophobia) is one upon which we are yet in great darkness. The number of well-authenticated cases we have on record, in which this affection could not be placed to the bite of a rabid animal, together with other peculiarities connected with it, lead me to the conclusion, that there are two varieties, as in tetanus, viz. the idiopathic and traumatic. The progress, too, of the disease in those who have been bitten by a rabid animal, is so opposed to all known laws of the effects of a morbid poison introduced

* Transactions of the British Association, Bristol 1836.

into the system, that I cannot but doubt their connexion. Mr. Travers says*, "that a morbid poison received into the circulation will sooner or later shew itself in an external form; but independently of that, it will give notice of its existence by a visible decline in the functions of health." This is not the case in hydrophobia; we have here, as in tetanus, no appreciable organic lesion, no external marks making their appearance during the progress of the affection. Ought we not, then, to dismiss the idea of such a poison lying dormant in the system for months, and even years, and then producing such a formidable train of symptoms? And ought we not to direct our treatment more to subdue the peculiar spasmodic symptoms which characterize the paroxysm? The tonquin remedy, which was formerly held in high repute as a specific in this disease, was composed of antimony, opium, and musk. This remedy, I imagine, would not have been so highly extolled, had it not been found beneficial in arresting at least, if not in curing, some cases of this dreadful affection. And when we consider that this was a secret remedy, the *modus operandi* of which was not understood, I think we may reasonably expect more satisfactory results from the judicious employment of tartar emetic and opium, which form the active ingredients in the old remedy. Your obedient servant,

R. YATES ACKERLEY, M.R.C.S.

1, Gt. Homer Street, Liverpool,
14th Sept. 1837.

ARTIFICIAL RESPIRATION,

DURING CONVULSIVE FITS IN CHILDREN.

To the Editor of the Medical Gazette.

SIR,

IN the number of the Edinburgh Medical and Surgical Journal for April 1818, is a paper by my respected friend Mr. Terry, now surgeon to the Northampton General Infirmary, detailing the successful employment of artificial respiration in the asphyxia of still-born children, and pointing out very forcibly the necessity of long continued perseverance even in apparently hopeless cases. In one instance related by him,

resuscitation took place after two hours and a half; and in the other, after one hour and three-quarters: in the latter instance the child being completely restored. I have met with several instances where I have been equally successful, when the action of the heart was not in the least perceptible, and where, for a very considerable time, all appearance of vitality was absent, and yet, after artificial respiration had been kept up for a time in which perseverance seemed hopeless, the action of the heart began to be distinguished, the livid aspect gave place to the colours of health, and the lungs at length were called into action.

My object now, however, is to call the attention of the profession to the application of the same means of artificial respiration in the asphyxia that is frequently met with during a convulsive fit in young infants. I believe that many a child has been lost—often and often the long-suspended act of respiration has ceased entirely—from the attendants limiting their measures to hot baths, frictions, cold water splashing to the face, stimulating applications to the nostrils, &c. &c., when the artificial inflation of the lungs, with the alternate expulsion of the air by pressing the chest and abdomen, would have restored the lost function, and recovered the child. I have met with several successful cases of this character, but the following is so striking that I forward it for publication in your valuable journal.

On the 20th March, 1837, an infant in Nottingham Place, five days old, had been in perfect health up to the moment that he was suddenly seized with a most severe attack of convulsions, the cause of which appeared to be the overloading the stomach the preceding evening. The fits began by the muscles of the eyes and face being thrown into violent clonic contractions, producing the most frightful contortions of the features; the pupils were dilated, the whole face and head turgid, as if the veins were going to burst, and and of a deep blue, or rather purple, colour. The respiratory muscles next took on the convulsive action, and caused the greatest oppression in the breathing; indeed the respiration was *completely stopped in most of the fits*, and recovered, after a frightful interval, by convulsive gasps and sobs, followed by deep sighs. The other muscles of

* On Constitutional Irritation.

the body and limbs then followed, and the whole muscular system became involved in the spasm. The following is an account of the fits and their duration:—

Interval.	Time of Attack.	Duration.
Three-quarters of an hour ..	Half-past 8, A.M.	First seizure, quarter of an hour.
Three-quarters of an hour ..	Half-past 9	Second, half an hour.
Half an hour	Quarter to 11 ..	Third, three-quarters of an hour.
Half an hour	Noon	Fourth and fifth, two short convulsions, quickly succeeding each other.
Quarter of an hour	1, P.M.	Sixth, quarter of an hour.
Three quarters of an hour ..	Quarter to 2....	Seventh, ten minutes.
Thirty-three minutes	20 minutes to 3.	Eighth, twelve minutes.
One hour, 25 minutes.....	25 min. past 3..	Ninth, eight minutes.
Half an hour	5 minutes to 5.	Tenth, twenty eight minutes.
Twenty minutes	6	Eleventh, half an hour.
One hour and a quarter	10 minutes to 7.	Twelfth, three-quarters of an hour.
	10 minutes to 9.	Thirteenth, twenty-three minutes.

The main indication in the treatment was to get the bowels to act, and to relieve the pressure on the brain. The child had been put into a hot bath by the nurse, which was repeated during the second attack, without apparent benefit. A leech was applied on the frontal bone, near the fontanelle; half a grain of calomel and a grain and a half of jalap, with a tea-spoonful of castor-oil, were given every four hours, and the lower part of the bowels repeatedly evacuated by injections of soap and water and castor-oil. The strength was kept up by injections of jelly, with a few drops of sal volatile or brandy in them. After the twelfth fit, the bowels acted for the first time from the higher intestines copiously; after which there was only one mild though long seizure, when they entirely ceased; the child went to sleep, and was perfectly well the next day, and has remained so ever since, without the slightest symptom of any return.

The whole case shows to what extent functional mischief may affect the brain and spinal column, excited by sympathetic irritation, without any inflammation or change of structure, for the fits ceased almost immediately after the operation of the medicine administered by the mouth; as, though the enema emptied the large intestines, this did not have any effect upon the symptoms, the obstruction seeming to be in the upper portion of the canal.

The pressure produced upon the brain

was such that respiration was entirely suspended during the greater part of the fits, and even the action of the heart could not be felt for more than ten minutes in the third and twelfth fits, and the child lay to all appearance dead. It was at such times that I proceeded to restore the suspended functions by artificially inflating the lungs, in the manner described by Mr. Terry—namely, by breathing into the mouth of the infant from my own, closing the nostrils, and compressing the thorax after each inflation; observing the natural periods of frequency as much as possible. I am quite convinced the child would have been lost, had it not been for the artificial aid thus afforded to nature in the severe struggle, till the offending matter was expelled. By means of the artificial respiration, the colour (especially of the face and lips) turned from purple to red, but still there was no breathing, till a convulsive gasp announced the termination of the fit.

I am aware that some have recommended a tube to be passed into the larynx, in preference to breathing into the mouth; and also many have objected to the employment of one's own breath, as the expired air is loaded with carbonic acid, and deprived of a portion (though a small one) of its oxygen. The success of the plan I employed, however, is quite sufficient to justify its use, and it is much more easily effected than by means of the tube, which has

sometimes seriously injured the soft parts, and is too readily displaced. A purer air would no doubt be more proper, but unless its temperature could also be kept up to that of the human body, we should lose one of its restorative properties, or perhaps as much consequence as the other. In the cases where artificial respiration was kept up in animals under the influence of narcotic poisons, by Sir B. Brodie, as detailed in his Croonian Lectures, the temperature of the air employed was thus artificially elevated; but in the hurry and confusion of a still-born child or a convulsive fit, there is no time to make the necessary preparations. What I would most wish to insist upon, is the necessity of perseverance in such instances; and perhaps the history of the case I have given may induce others to persevere in the same manner to the very last (*lateat scintilla forsitan*), and thus perhaps, as in this instance, restore an only child to its anxious parents.

Your obedient servant,
LAWSON CAPE, M.D.

4, Princes Street,
Hanover Square, Sept. 26, 1837.

EFFECTS OF A BLOW ON THE STOMACH.

REMARKS ON DR. HOLLAND'S PAPER.

To the Editor of the Medical Gazette.

SIR,

IF you should deem the following remarks worthy of insertion in your widely disseminated and duly appreciated journal, by so doing you will oblige,

Your obedient servant,
H. G. HARBORD.

South Dispensary, Liverpool,
Sept. 25, 1837.

The eyes of the scientific world, or at least of the British portion of it, have been, and still are, directed to the proceedings at the meetings of the British Association recently held at Liverpool. It is, therefore, but just that the medical part of the community should turn their attention to some of the disputed points mooted at the meetings of the medical section. I was much struck,

among others, with a paper by Dr. Holland (of Sheffield), entitled, "An investigation into the cause of death from a blow on the stomach, with remarks on the means best calculated to restore animation."

After mentioning the commonly received notions, such as a nervous shock from injury to the semilunar ganglion producing paralysis of the heart, and adducing evidence that the heart did not cease to contract immediately on the receipt of such injury, and noticing the opinions of some with regard to the eighth pair of nerves, he proceeded to state his own views, which were nearly as follows:—

Death from such a blow generally occurred in pugilistic encounters, when the circulating system was much excited, and such a blow was generally directed from below upwards. Now the effect of this would be, in his opinion, to compress the aorta, among other important parts, between the fist and bodies of the vertebrae, which curve forward at this point. The consequence would be, that the current of arterial blood would be, at the time, stopped; and not only so, but, from the direction of the blow, driven backwards upon the heart, and so cause a retrograde movement, which would drive a mass of blood into the left ventricle, and by thus overpowering it, produce death. He stated he was convinced this could take place, for he had succeeded in distending the left ventricle by *gently* injecting a fluid through the aorta. The Doctor also mentioned, that a blow at the angle of the jaw, it was well known by pugilists, would produce insensibility; but this he referred to a different cause, viz. the sudden impulsion of the blood through the carotids upon the brain.

In fine, three points were assumed, and considered by the learned Doctor to be proved, viz. that—1st, the aorta was so situated as likely to be influenced in the manner stated; 2dly, that a blow given with great violence would cause a retrograde motion of blood, and its entrance into the left ventricle; and 3dly, that the latter would cause death.

Dr. Williams noticed the remark about the semilunar valves allowing the injected fluid to pass them, when *gently* thrown into the aorta, but clearly

proved that blood, or any fluid, could be driven into the ventricle *only* by being *gently* propelled, and thus allowing it to trickle into the heart, without insinuating itself between the valves and aorta: this fact is so well known to all anatomical preparation makers, that it requires no comment. Sir James Murray objected to Dr. Holland's views, that although the blood might be driven back by the blow, it would be generally diffused throughout the enlarged portion of the aorta, innominata, subclavians, and other arteries arising from it; or, even supposing it should rupture or pass the valves, he did not see how such a small quantity in excess could so materially affect the heart's action. Dr. James Johnson thought that the effects might be referrible, in some measure, to paralysis of the diaphragm from the blow. Dr. Copland suggested, that the heart itself might be injured through the diaphragm.

Had I been one who had already earned fame by research and patient investigation, I would have risen in the "great assembly," and there made known my opinions; but I was awed into silence by the presence of such a galaxy of genius.

It is the lot of few, and master minds only, to strike out new and original lines of thought—to embellish the annals of science with new and important discoveries—to establish systems vast and comprehensive. On these men we look with an eye of respect and reverence; and as we follow them "*haud passibus aequis*," along the paths they have opened before us, we stay a while to gather what they may have passed in their haste, or neglected in their approach to the grand object of their pursuits; and we think ourselves fortunate if, in this way, we obtain any fact, however unimportant—any truth, however simple; and we rejoice thus to add our mite to the common cause of science. It is with such feelings as these that I have entered upon the investigation of this subject; and I shall now venture to lay before your readers my deductions from the evidence and opinions before us, and my views of the matter.

Let us trace the probable effects of a blow directed backwards and upwards in the epigastric region, during an excited state of the circulation. In the

first place, the aorta would be compressed against the spine, and thus the current of blood momentarily stopped; and not only so, but driven backwards towards the heart. Again, independently of the effects of the concussion on the vena cava or semilunar ganglion, the diaphragm would be driven upwards, diminishing the capacity of the chest, the calibre of which would be still further diminished by the ribs being forcibly drawn downwards and inwards. Besides this, the blow (as suggested by Dr. Copland) might have some immediate effect upon the heart itself, especially if at all directed to the left side. Having now, in imagination pictured the primary effects, let us follow them still further.

The retrograde or impeded current of blood meeting with that portion just ejected from the heart, must expend itself somewhere. Five large passages present themselves; the largest, however, viz. that which leads to the left ventricle, is immediately closed by means of the semilunar valves, and the carotids being at the summit of the arch of the aorta, the force of the two portions of blood, namely, that from the heart, and that driven backwards upon it, would be principally expended upon them, although the subclavians would, undoubtedly, have more than their share, though not to such an excess as the carotids. The natural consequence of this would be, either to congest the brain to an extent incompatible with the due performance of the vital functions, or to cause the rupture of a vessel within or upon it; the tendency to congestion being materially increased by the mechanical obstructions to respiration, from the diminished capacity of the chest preventing the free return of venous blood to the right side of the heart; and all these various causes acting and reacting one on the other, would tend to extinguish the vital spark, already enfeebled by the first shock.

Having now extended my remarks to a length almost unwarrantable, I must apologize for such transgression, and conclude by hoping that these views, if erroneous, may be deemed worthy of refutation, as the subject is an important one.

SOLID NITRATE OF SILVER IN
GONORRHŒA IN FEMALES.

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To the Editor of the Medical Gazette.

SIR,

IN your number for the 9th ult. I find a letter from Dr. McCune Smith, in which he speaks with disapprobation of the application of solid nitrate of silver in gonorrhœa.

It is easy to start objections to any point, but not quite so easy to prove them to be such. Since Dr. Hannay made known to the public, through the medium of your valuable journal, the mode of applying the solid nitrate of silver in gonorrhœa, I have had opportunities of trying it, and seeing it tried, in between thirty and forty cases, the result of which enables me to state, that it is not only a most efficacious remedy, but that it is unattended with *any bad symptoms*, and excites scarcely any uneasiness. I beg you to understand that I am now speaking of simple gonorrhœa. When ulcerations accompany the vaginal discharge, there is some pain experienced on the introduction of the nitrate of silver, but it is not so severe as Dr. McCune Smith would have us believe.

As regards the success of this plan of treatment, I have invariably found that, in four-and-twenty hours after the application of the solid nitrate of silver, the nature of the discharge has undergone a considerable change; what was thick and purulent became thin and watery, and this, in the course of a few days, ceased altogether.

It is stated that the patients frequently experience relapses. I will not say that such may not be the case, especially in the hands of such feeling practitioners as Dr. McCune Smith, though it certainly never occurred in those to whom I applied it.

Query.—Has Dr. Smith ever used the solid nitrate of silver, or seen it used, in cases of gonorrhœa? He either has, or has not. If he has, though in a few instances, I suspect he must have written in direct opposition to the palpable evidence of his senses. And if he has not, it would have been wise in him to have acted a silent part. Be this as it may, I cannot help thinking that the following phrase peculiarly applies to those who draw practical conclusions

without practical premises—“*Damnant quod non intelligunt.*”

Your obedient servant,

HENRY SOMERSET TAYLOR.

Hereford,
26th Sept. 1837.

FŒTUS—SIGNS OF ITS VITALITY.

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To the Editor of the Medical Gazette.

SIR,

I WAS some time since called by a very intelligent surgeon, residing at Maida Hill, to bring on premature labour in a patient of his, who was supposed to be carrying a dead child, and who had, on a previous occasion, suffered materially from not having been delivered artificially after the child had been putrid a considerable time. On applying the stethoscope over the gravid uterus, the “bruit placentaire” and sound of the fetal heart were wanting; but as the os uteri was not so low down as at the seventh month of utero-gestation, and as her general health was good, and Mr. Wing constantly at hand in case of emergency, I thought it better to allow some days to elapse before premature labour should be induced, and arranged that we should meet again in a few days. At the appointed time we accordingly did meet; and just as I was about to examine her she gave a slight start, and directed my attention to her abdomen, where I distinctly felt the angular projection of a limb of the fœtus, which gradually receded, as is pretty generally the case when the child is alive. Mr. Wing observed the same; and the patient remarked that this often occurred. We congratulated ourselves that we had not brought on the labour before the seventh month, as we had here one of the generally-received indications that the fœtus was alive. On making an examination per vaginam, the os uteri was found considerably shortened; and when she was in the erect posture, that peculiar sensation, called “ballotement” by the French obstetric authors, was absent. In ten days after our last visit labour came on naturally, and she was delivered of a child, which had evidently been dead some weeks. This case is peculiarly interesting, as shewing the certainty with which we may form our

diagnosis as to the existence *in utero* of a living child by means of the stethoscope, when, in all probability, without its assistance we should be in error.

Your very obedient servant,
GEORGE S. LILBURN, M.D.

24, Harley Street,
Sept. 27th, 1837.

CASE OF
AMPUTATION OF A PART OF THE
LOWER JAW.

By J. ADAIR LAWRIE, M.D.

Professor of Surgery, Andersonian University,
Glasgow; Senior Surgeon, Glasgow Royal Infirmary.

HELEN JOHNSTON, *æt.* 67, Saltmarket, was admitted into the Glasgow Royal Infirmary, June 9, 1834. The following description and reports are abridged from the hospital journals:—

The gum covering the front half of lower jaw presents a foul, irregular, jagged ulcer, discharging a fetid serum, and is the seat of stinging pain. Alveolar processes seem destroyed, and the teeth, as far back as the bicuspidati, on either side, are loose, and project from the ulcerated surface. The bone is felt bare in front, and laterally, on either side, thickened; on the right, nearly as far back as the angle. A tumor, having many of the characters described, and about the size of a hen's egg, is situated under forepart of tongue, and between it and posterior surface of jaw bone, and appears intimately connected with the ulcer on the gum. The tongue is raised and thrown back by this swelling, but, as far as can be ascertained, is not implicated in the disease. The chin is rendered prominent by the swelling of the gums, and, together with the under lip, is unnaturally livid; but there is no trace of disease on any part of the mucous membrane of the cheeks or lips. Articulation and deglutition impeded. She is much annoyed with the fetid discharge, and her condition is consequently very uncomfortable; pulse natural; general health rather impaired; countenance good. A tumor of considerable size was cut from the same situation as the present a year ago in this house. It reappeared five months since.

June 14th. — At a consultation, the

majority were of opinion that excision of the greater portion of the inferior maxilla was advisable, recommending, at the same time, the extraction of the loose teeth, and the delay of a few days thereafter to ascertain more accurately the state of the tongue.

Some teeth have accordingly been removed; and on further examination, the tongue seems sound.

25th. — The nature of the disease and the extent of the proposed operation having been explained to the patient, she requested to be allowed to leave the hospital to consult her friends. She returned two days ago, and having declared her willingness to submit to whatever might be deemed proper, the following operation was performed this afternoon:—

The patient being seated on a chair, her head turned back and supported on the chest of one of the surgeons, an incision was made from the left angle of the mouth, through the entire substance of the lip, nearly perpendicularly downwards as far as the inferior horizontal edge of the lower jaw; the bone thereby laid bare externally, but no incision or separation of the bone from the soft part made inside the mouth. The needle of Dr. Jeffray's chain-saw was passed round the bone (inside the mouth) and the saw drawn through. The saw was worked without a single check by the operator, standing on a stool behind the patient. A similar procedure was adopted on the right side, but from the more exterior thickening of the periosteum, the incision was carried further back, and the bone sawn close to the masseter muscle. The under lip and integuments were now dissected from the front of the tumor and bone; the knife was passed between the tumor and the tongue, and the whole of the diseased parts, together with the portion of bone sawn, removed. The time occupied was seven minutes. The bleeding vessels were tied; the flap formed by the under lip was replaced and retained by sticking plaster and bandage, and the patient walked, unassisted, to her ward. Pressure was so successfully applied by my assistants to the bleeding vessels, that not more than five ounces of blood were lost. So soon as the attachments of the muscles of the tongue to the bone were cut, the tongue was pulled back into the throat; and had not this been foreseen and steadily prevented, the

patient must have been suffocated. It was easily obviated by one of the assistants carrying his finger towards the root of the tongue, and pressing the organ downwards and forwards. To assist this, and afford the means of readily laying hold of the tongue, should the tendency return after the patient should be in bed, a ligature was passed through the tongue, near its point, and attached to the dressings. About an hour after the operation, hæmorrhage came on to such an extent as to compel the removal of the dressings and stitches. Two or three vessels were tied under the tongue. The instant the stitches were cut the tongue again receded, and was even more troublesome than during the operation. The dressings having been reapplied, the tongue retained its proper position, and the respiration continued ever afterwards perfectly easy.

July 7th.—Adhesion has succeeded on left side, but failed on right. The edges of the wound on the right side being quite clear, and there being no appearance of a return of the disease, the granulations and a slight paring of skin were removed by the scalpel, and the edges brought accurately into contact by pins and a stitch, without an external dressing. The adhesion was quite successful, and on the 12th of August she left the hospital in perfect health.

About two months after she left the hospital, the disease seemed to have returned, in the form of enlarged glands, on the left side of the neck, towards the angle of the jaw, and immediately above the left side of the sternum,—the latter of considerable size. Those towards the angle of the jaw proved to be the result of irritation, and not of extension of malignant disease, because at the end of a few weeks they suppurated, discharged their contents, and disappeared. That at the top of the sternum diminished considerably; and although it has not disappeared, it gives so little annoyance that the old woman is not even aware of its existence.

I saw widow Johnston a few days ago. She continues in good health: the mouth and neck are quite free from disease; the swelling above the sternum stationary, and free from pain. She expresses herself satisfied with the result of the operation. The disfigurement of her face is very slight,

and consists only of a shortening of the under jaw; the remains of the jaw on either side have approximated, and lie within half an inch of each other, apparently loosely connected by ligament. She articulates very distinctly, swallows liquids as well as she ever did, and can masticate bread and soft food. The prehensive power of the under lip is impaired, requiring her to push solid food farther into her mouth than before the operation. She cannot protrude her tongue beyond her under lip. She says she did not give a true account of her age at the time of the operation, lest I should be deterred from undertaking it; she was then upwards of 70, and must now be upwards of 73.

The surface of the tumor after the operation was granular, and of a greyish colour. Internally it was fibrous, firm, paler than externally, and not at any point softened. It was firmly adherent to the gums, which had very much the same external appearance as the tumor. A considerable portion of the upper surface of the bone amputated, was removed by ulcerative absorption. Towards the right side, the alveolar processes and cancelli of the bone were softened.

REMARKS.—The successful result of this operation is my reason for publishing it; for although apparently formidable, and the second of the kind performed in this large city, it is by no means difficult to execute. Its sole value (and the same may be said of all operations performed for the removal of supposed malignant disease), consists in its success. The grand aim of the surgeon in such cases, is to ascertain, from external characters and history, what tumors are likely to return after operation, and what not; a diagnosis so difficult to form, that our best and most experienced surgeons are often at fault. The internal structure of tumors—I say it with due deference to our pathologists—is of very minor importance; because, as that can only be ascertained after removal, a prognosis founded on it comes too late. It is of importance to connect the internal structure with the history and external characters; but, I repeat, the latter must ever be the subject for the diligent study of the practical surgeon. The details of the present case, extracted from the public journals of an hospital, are not so minute in this respect as I could wish;

the following, however, are worthy of remark. The *history*: arising without any known cause, slow in its progress, returning in the same spot after removal, and not implicating (except by slow immediate extensions) the parts in the neighbourhood—in close contact with delicate parts (the tongue, and mucous membrane of the cheeks and lips), and leaving them sound—the age of the patient (70), her general health, colour, and expression of countenance good. *External characters*: irregular and granular on its surface; colour light; not hard to the touch; the gums ulcerated, but not fungous; the discharges fetid, not hæmorrhagic; the pain stinging, not severe. Its situation, the cellular tissue under the fore-part of the tongue.

The history after the operation is interesting, inasmuch as what appeared to be a return of the disease ended in suppuration, and healed without difficulty.

I cannot help thinking that a careful record of the history of similar cases before operation, and a statement of the result, whether successful or unsuccessful, three years afterwards, together with minute details of their *external* characters, might, at no very distant period, lead to a more safe and scientific treatment than even our best surgeons at present pretend to.

It is unnecessary to say much regarding the mode of operating adopted in this case. It appeared to me to combine facility and rapidity of performance, with the very important advantage of giving a clear view of the diseased parts, and those among which they lay. It had the disadvantage of being rather difficult to heal.

Some distinguished surgeons, among whom I may mention Velpeau and Richerand, object strongly to the removal of the chin, on the grounds that the attachment of the genio-hyo-glossus muscle to the chin being destroyed, the patient incurs the risk of dying of suffocation during the operation, in consequence of the retrocession of the tongue, or ultimately of inanition, from inability to swallow. The first of these objections is so far well founded, as to require the sedulous attention of the operator. M. Lallemand, in a similar case, nearly lost his patient on the operating table, and only saved him by opening the larynx. He appears not to have been aware that the tongue would recede.

His assistants were pupils, and were so much alarmed by the frightful appearance of the patient, as to be of little service. He at first attempted to pull the tongue forward; but the bleeding was so profuse, that he was forced to let go the tongue, and leave his patient in a state of asphyxia, until he freely cauterized the bleeding surface. He then opened the crico-thyroid membrane, inserted a canula, and dressed the wound. A knowledge of this case prepared me for a similar difficulty in Johnston's, and I found that the moment the genio-hyo-glossus muscle was cut, the tongue receded, and the patient would have been suffocated. Had I not been properly assisted, I might, perhaps, have been compelled to follow Lallemand's example; but I was saved all uneasiness by Dr. Pagan putting his finger on the root of the tongue, and pressing it downwards and forwards.

One circumstance struck me as remarkable: as soon as the dressings were applied, and support given to the under and fore part of the tongue, its tendency to recede immediately ceased, and the poor woman breathed with ease. When the dressings were removed, on account of the secondary hæmorrhage, the tongue was instantly and spasmodically drawn into the pharynx, and was with more difficulty kept forward than during the operation. No sooner, however, were the dressings re-applied, than it regained and retained its natural position. This fact is at once physiologically curious and practically important.

The second objection, that the patient must die of inanition, is supported by a case in which the unfortunate occurrence took place in a patient under the care of Richerand, in the Hospital St. Louis, in 1822. Probably that case presented some unusual peculiarity. The tongue is reported to have been quite immovable. Johnston never experienced much difficulty in swallowing, and Lallemand's patient drank with facility immediately after the operation. A successful case, in many respects resembling Johnston's, is related by Genroul: he removed a portion of the lip and integuments, although sound; a procedure which Johnston's case proves to be quite unnecessary.

Glasgow, Sept. 1837.

TUBERCULAR LEPROSY.

To the Editor of the Medical Gazette.

SIR,

CONCEIVING that your numerous readers will be interested in the perusal of the following brief analysis of a most important, because practical, paper, by Dr. Hancock, lately read at the Medical Section of the British Association, at Liverpool, I transmit the same to you for insertion in your excellent GAZETTE. It relates to one of the most dreadful chronic disorders incident to humanity—viz. Tubercular Leprosy; the *Lepra Arabum*, or *Cocobae* of the Africans, which Dr. H. has succeeded in identifying with the *Ara-apatta* of the Caribs, and the *Radesyge* of the northern nations of Europe. This disease is unfortunately very prevalent in the West Indian colonies and British Guiana; but it would seem that the native Indians, amongst whom Dr. Hancock sojourned during many years, possess much useful and practical knowledge on this and other important subjects; at least they employ very effective remedies and modes of treatment in divers inveterate maladies, which, in the civilized countries of Europe, are justly regarded as the very opprobria of the healing art.—I am, sir,

Your obedient servant,

C. MACKENZIE.

London, Sept. 27, 1837.

The first part of Dr. H.'s paper went to show that the disease in question has been usually confounded with elephantiasis, or what is termed the Barbadoes *leg* and Siam disease; in which the skin and soft parts about the leg and ankle become so enlarged as to bear a close resemblance to the leg and foot of an elephant—a local disease confined to the affected limb, and perfectly distinct from the true *Lepra Arabum*, in which the whole body, or the entire mass of fluids and solids, appears to be implicated.

In treating of the causes and symptoms of the disease, Dr. Hancock thought that it obviously consisted in a vitiated condition of the blood and serous fluids; but this appeared to be connected, either as cause or effect, with obstructions of the absorbing and secreting vessels; this, together with a peculiar

predisposing diathesis, determines the form of the disease. The obstruction and a faulty secretion produce serous deposits under the skin, forming the tubercles, knobs, or indurations, which soon characterize the disease; in proportion as they increase in number and magnitude, the circulation, with the absorbing, secreting, and depurative processes, become daily more obstructed. The process being slow and gradual, the humour solidifies almost as fast as it transcends. The tubercles are of a copper or leaden colour, and more numerous on the legs and thighs. There is stiffness of the eyelids and thickening of the palpebra: the voice becomes hoarse and nasal, and the respiration more or less affected. Varicose veins and knotty pustules are observed about the root of the tongue; and in the advanced stage the whole countenance becomes distorted, full of knobs and tubercles, and of a hideous aspect. In the last or ulcerative stage, the hairs fall off from the eyelids, eyebrows, chin, and other parts; the toes are swelled, and crack with dry fissures, and the skin becomes quite callous and insensible. A corruption of the whole mass of humours, and general disorganization of the solids, ensue; hectic fever supervenes, and terminates the existence of the miserable sufferer. In some cases a few solitary symptoms will occur, whilst in others many will appear almost simultaneously.

Dr. Hancock adduced a variety of arguments to prove the perfect identity of *Lepra Arabum* with the *Radesyge* of Norway, Sweden, and other northern countries. He had been led to investigate this part of the subject from having seen, with surprise, in the 18th volume of the Edinburgh Medical and Surgical Journal, a summary of symptoms drawn up from Holst, Stowe, and other writers, with the view to prove their *non-identity*. The objections contained therein were shewn to arise from a misapprehension of the subject, or from an over anxiety to establish certain preconceived opinions. Not only did the whole train of symptoms agree in every particular, but also the general progress of the two diseases, and the morbid changes which take place. Dissections of those who had died of leprosy had not afforded to the writer evidence of such changes in the genital organs, as have been adverted to by certain

writers. It was worthy of remark, that the appearances noticed by different authors, in the Radesyge of northern Europe, are all occasionally observable in the Cocobae of the West Indian colonies, from which it is clear that varieties have been multiplied without reason, or for any practical purpose, but tending only to complicate and create confusion. Dr. H. had, in no instance, known the Cocobae (*Lepra Arabum*) to be communicated by the husband to the wife, nor *vice versa*; so that, notwithstanding the prevalent opinion to the contrary, he had long ago regarded it as void of contagion; yet he was almost induced to think that, under predisposition and other concurring causes, the disease may, in the *ulcerative stage*, be communicated. It seems that *white* persons in the colonies avoid touching a person infected with leprosy; and they generally segregate the lepers on a distant part of the respective plantations.

Having briefly stated the results of some post-mortem examinations in the disease, Dr. H. proceeded to detail the methods he had found most effective in its cure. When attended to early, the symptoms were easily arrested by the use of saline laxatives, with antimonials, anodynes, diaphoretics, vapour baths, and frictions; bleeding, spare diet, and the several means for promoting *lymphatic absorption*, and all thesecretions, especially by perspiration. The advantage of such means, duly persisted in, was evinced in the relief obtained for the patient, and rendered obvious to others by the foul and fœtid miasms evolved, as well as by the exuvia thus thrown off from the surface of the body. Cases which had occurred during Dr. H.'s practice in Guiana were adduced as illustrative of the advantages obtained over the disease by calling in the united aid of the various remedies.

As auxiliaries, opium and the *Coonuparu* (the leaves of a plant of the Euphorbiaceæ family), were found to afford essential aids, along with the alternate use of warm vapour baths and cold affusions,—means which the author has found to be equally beneficial in divers chronic diseases. Moderate bleeding was often found to augment the strength, and greatly to facilitate the cure; along with the moderate use of tonics, diaphoretics, and iodine. The sensation of lassitude and debility often depends on

hypertrophy or congestion. The morbid growths, or fungoid tubercles, in this disease, are nourished by excess of blood; they resemble those of cancer, and by many they have been considered equally incurable.

The Indians of Guiana resort to fomentations, baths, and to a drink of the bark of a tree called *Mouca*, with the root of a vine, *Paramaroora*—a species of *Cissus*—and the bark of *Waiacono* (guaiacum), the infusion of which is fermented with honey. They use also the bark of the tree "*Tamootu*," a nondescript. These simple methods of the American natives are likewise of vast efficiency in arresting the most pernicious fevers, dysentery, and a multitude of ailments both acute and chronic.

MEDICAL ATTENDANCE ON THE POOR.

To the Editor of the Medical Gazette.

SIR,

IF you think your readers are not quite tired of every thing appertaining to the poor-laws, more especially that part of them connected with medical attendance, perhaps you may find room for the following observations.

It appears very clearly that the medical profession is to look for no support from the government, or any of its officers; I think, therefore, they must look to their own interests, and by a combination of action produce that beneficial result which they can never obtain by individual exertion alone. I believe every one who is capable of judging—that is, every one who knows the extent of attendance, and the quantity of medicine required for the poor—considers that the medical attendant, as at present paid, is very badly remunerated. This, I believe, is allowed by all; but it is stated in answer, that the grievance cannot be remedied; that it arises out of the absolute necessity of lessening the poor-rates; that the attendance is not compulsory; and that if the resident practitioners do not like the terms offered, the Assistant-Commissioner will send a person to take the office. This is not only threatened, but it has been acted on in several instances; and the medical practitioner who, after a regular and necessarily expensive education, has by a great outlay of money,

and by assiduous and active exertion, established a connexion just sufficient to obtain the necessaries of life for his wife and family, has found his future prospects blighted by the introduction of a young man (perhaps who has just finished his education), under the influential interest of the Commissioner and his officers.

There appears to me but one remedy for this grievance, which is the following:—Let every gentleman who is eligible, and desirous of serving the office of medical attendant to a Union, send in his name to any one of his brethren who may be fixed on; and when the names are collected, let them be drawn from a box in the presence of the whole, and then let the gentleman whose name is first drawn be considered the person who is to have the appointment for a year.

Now, as to the terms of remuneration, I propose that in a wide agricultural district the practitioner be paid sixpence a head; in towns and manufacturing districts, fourpence.

I should be willing that a gentleman thus appointed should be under the directions of the Board of Guardians, for I know no other authority to which he could be responsible; and should the Board find him incompetent or unwilling to perform his legitimate duties, they might remove him, and let the names be drawn again to supply his place. Should the Commissioners absolutely refuse to appoint a gentleman thus presenting himself for their approval, and send a stranger to fill the office, I think the best method of counteracting this would be the following:—I should have it publicly known that every person who allowed himself, under these circumstances, to be appointed to a Union, should be sent to Coventry by his brethren, and have his name advertised in every medical periodical. I fear there are some few in our profession, as there are in all professions, who prefer their own personal advantage, however obtained, to upholding the character of that branch of science to which they unfortunately belong; but I trust they are not sufficiently numerous to interfere with the plan I have proposed.

Here I naturally expect to be asked this question—Will the Commissioners permit this? Most certainly not, if they can prevent it, for it will take away

their power of authority, which authority they have exercised in several instances in a most ungentlemanly and shameful manner. I am speaking from facts: they have treated many gentlemen of equal, if not superior education to themselves, as if they belonged to the lowest class of tradesmen, who were waiting on their high mightinesses with a tender for shoes or bread—refusing to hear any observations they may have wished to make—telling them, “totidem verbis,” they may either take the contract or leave it—that if no resident practitioner will take it, they can procure one immediately who will be glad of it. No doubt there are many young men who know not where to settle, who would jump at an introduction by a Commissioner for the smallest stipend—almost for nothing—as there are many briefless barristers, of equal talent, who would congratulate themselves at the appointment to the poor-law commission at one-fourth of the emolument the present possessors enjoy; and as economy is held forth as the attribute of the present ministry, I wonder they have not had recourse to this expedient. But no: this would save money to the country, it is true; but it would lessen their patronage, by diminishing the value of one of their numerous commissions.

Should a Commissioner read this, I have no doubt he will laugh at the presumption of a country doctor in giving advice on laws which “work so well.” Truly they do work well for the Commissioners and the rate-payers, but how do they work for the poor? The poor alone can tell; they, miserable creatures, know that they must become beggars, absolutely naked and starved, before they can *claim*, or at least *obtain* any relief, except by the sad alternative of giving up their all, I believe their very clothes, and then their only resource is the workhouse; and when they have once entered that abode, they have no prospect of a return to their cottage, having previously disposed of every article of clothing and furniture.

I should think much might be done for the poor by a tax on the funded property, in the form, for instance, of one-eighth per cent. on every transfer, which might be held for circumstances of peculiar distress in different parts of the country. By this tax many would contribute their quota to the support of

the poor, who now do not pay a farthing. There are thousands who live in hotels, chambers, and lodgings, whose door is never knocked at by the tax-gatherer, nor timidly tapped at by the poor—men with no fixed habitation, but with plenty of money to feed their imagination in the boarding of it, or to squander in personal gratifications.

I know not why the man who possesses one thousand a year in land should pay fifty pounds to the poor, when the fundholder pays nothing. The property of both are equally protected by the laws and military force of the country, the latter of which is drawn almost exclusively from the labouring poor. Moreover, the landowner affords relief to his neighbouring poor in many acts of charity, more especially in time of sickness, which they who live as I have mentioned have not even the opportunity of doing.

I beg pardon for this extra-professional lucubration, and remain, sir,

Yours obediently,

WILLIAM HILL.

Wotton-under-Edge,
Sept. 19, 1837.

MEDICAL GAZETTE.

Saturday, October 7, 1837.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

A FEW WORDS TO STUDENTS.

WITH the exception of underselling schemes in the medical market, the present session seemed to offer so little of novelty, that we did not think it necessary to devote an article especially to it. But since we last wrote, a *sirocco* (or rather, from its general direction, a *tramontana*;) of puffing has blown with unusual violence, and we must give a little advice to those students who have not yet made their arrangements.

Whenever, then, a peculiar advantage is offered, do not let it distract all your attention, but rather search for the ten

disadvantages it is intended to cover. These will not lie deep beneath it. In the majority of instances the pretended benefits are by no means *peculiar*, but are common to all similar institutions. Dr. A., for example, is not the *only* hospital physician who keeps a case-book, nor Mr. B. the *only* surgeon who delivers clinical lectures; nor is clinical instruction the *only*, though when properly conducted it be a very important part, of medical education. It is a system now generally pursued, in degrees of excellence varying with the capacities of the teachers, and often abused, so as to be comparatively useless. The rest of these peculiar advantages are, from the manuscript Baconian communications to the midnight anatomy by gas-light, mere glittering bits of tinsel, as unworthy the attention of the student, as of the character of those who promise them. They are placed in the foreground only to attract the eye from disadvantages for which they cannot compensate.

With this caution, then, to "beware of peculiar advantages," we pass to consider the course which the medical student may best adopt in the years of study he is now commencing. "*Cuique in sua arte credendum*," is more true of the matter than of the value of the subject. If introductory lectures were to be believed, the student would have to try to study each subject to the total exclusion of the others. His head confused by the contemplation of all the magnified glories of each course, placed by its professor in its brightest light, the student resembles Molière's Monsieur Jourdain, in the midst of his masters of music, dancing, fencing, and philosophy, each urging his own especial claims, and his universal utility. And like him, he knows neither how to find time, nor which to choose. We would give him one rule—study nothing except in its immediate practical appli-

cations. The difficulty of studying diseases profitably in private practice, and the impossibility of pursuing some branches of the science, except now, are sufficient to prove the absolute necessity that the student should regard the present as a period during which that knowledge must be obtained which may form the best foundation of private practice, and which future opportunities will be scarcely more than sufficient to retain.

The subject which is above all of the highest importance, is that which has unfortunately been usually regarded as of the least—the clinical study of disease. To it, every thing should be secondary and subservient, and it should be an active and a personal study. It is not sufficient to follow any teacher, however excellent: clinical lectures, if they lead the student to be merely a passive listener, by satisfying him that when he has heard the case read from the book and the accompanying remarks, he knows all that can be learned of the disease, are useless, if not positively mischievous. They offer not the slightest advantage over books to the student who does not himself examine the cases. We do not doubt that these, which many praise as the greatest advantages, are among the worst faults of the French schools: relying on his clinical lecturer, the pupil there seldom touches or speaks to a patient; he takes notes of the examination and the remarks made by the physician, or surgeon, and thinks that he has then sufficiently learned the disease, which by himself he could neither recognize nor treat. This, however, is almost a necessary result of the system of admitting pupils to the wards only during the visits of the medical officers; a system which is imitated in some of our own institutions, and is attended with inconveniences for which no

amount of clinical instruction can compensate. Let students especially look to this: if the wards of their hospitals be not open for them to study disease, quietly and for themselves, they will find it nearly useless to enter them at all. The information to be derived from the lecturer should never be neglected, but alone it is quite insufficient to teach medicine.

We have often observed, that one of the most prevalent errors among students is, that their attendance on London hospitals is rendered peculiarly valuable by the number of operations and rare cases they may see—an error which an abuse of clinical instruction, and the establishment of clinical wards, has not a little tended to confirm. But the reputation and the usefulness of a practitioner will depend chiefly on his ability to cure or alleviate quickly the most common maladies; and it will be of little importance to have seen one or two cases, the *selecta majora* of a clinical ward, which he may never again meet with. Let operations and rarities be the condiments rather than the aliment of his clinical dietetics.

The value of reading and lectures is more than doubled when conjoined with the personal study of disease: the patients may (*magnis componere parva*) be compared to figures or maps, and books and lectures to their letters of reference—the former would require far more labour to comprehend them, without the assistance of the latter, which by themselves are perfectly useless: taken together, it is comparatively easy to learn the nature of the subject examined. In the choice of books, we would strongly recommend the use of monographs on each of the most important subjects, rather than of any one system of medicine or surgery. Even if there had yet been published one which was valuable except for occa-

sional reference, we would still prefer separate treatises, especially after the first year of attendance on practice. They need not be numerous; and even if they were, they would be found much less tedious, as they are also more instructive than single systems, of which the supposed necessity is lessened by the lectures, which, when not marred by the ubiquity of some favourite theory colouring every subject, should completely supply their place. As we have recommended the avoidance of "peculiar advantages," so we do that of *peculiar ideas*; both are most probably alike unsound. In reference to reading, students will do well to regard libraries and reading-rooms attached to their schools as of much value.

The general tendency of our advice on the study of anatomy must be the same. It should be regarded as next in importance to clinical study, not only because it has a most near relation to, and important applications in, both medical and surgical practice, but because the present affords the only opportunity which the majority of students can have of pursuing it. It, too, should be strictly practical: one attentive dissection will teach more, and more lastingly, than many lectures or demonstrations, or than any amount of reading. The latter by itself is perfectly useless, and, besides, so unpleasant a mode of losing time, that it is surprising it should be so much resorted to. In their first year, students should use, while dissecting, some condensed manual, and, as a general rule, that written by their teacher ("as which of them has not?") will be most advantageous. Afterwards, monographs, and the special descriptions of various organs, and the different more minute arrangements of parts and structure, should be studied; but all with equal reference to their practical importance. It need

scarcely be stated, that surgical skill is impossible of attainment without a due anatomical knowledge; much of medical practice is importantly assisted by it, and without it pathology and physiology would cease to exist. Its importance, therefore, makes it highly desirable that it should be studied attentively during the whole period of hospital education; not a day should be allowed to elapse without adding something to the stock of which all memories have so slight a tenure, and which, when lost, it will be so difficult to replace. The opportunities afforded for dissecting and for medical and surgical practice, should be the principal, if not the sole, guides to the student in making his choice among the schools.

We would now proceed to the collateral branches of education; but we must confess, that when we have already recommended daily and attentive practical study in the wards and in the dissecting-rooms, we have given very nearly enough to occupy the whole of the time usually allotted to work; for these would require three hours of lectures, and at least two for anatomy, and two more for the wards, each day; yet there are midwifery, chemistry, botany, materia medica, medical jurisprudence, all to be learned. *Quid agendum?* In the conversations which we have had with numerous students now commencing their studies, it has been lamentable to see how nearly universal the feeling is, that the only thing to be worked for is the passing of their examinations, and especially of that at Apothecaries' Hall. Yet this is neither surprising nor deserving of censure. We need not repeat the reasons why, as we have often shewn that the time allotted is quite insufficient for attaining a proper knowledge of all the subjects required; but as the license is essential, it is striven for at any sacrifice.

We are sorry to recommend any thing like a superficial study of important sciences; but we are compelled to advise students to disregard, during the first years, all that has not an immediate bearing on matters of primary importance, and to make the best use of their time to obtain a sound practical knowledge of their profession. Let them, at least, not waste the winter, when they may be studying anatomy, and watching disease; and let them work up the details of other matters a few months before their examination. We recommend this with the less hesitation, from the conviction of the inutility of a great part of these subjects in practice, and from knowing that, if learned now, they will be forgotten before the examination arrives, in the constant occupation of the mind by other more important studies.

In conclusion, we would strongly urge students not to think that the sooner they can pass their examinations and get into practice the better, but let them devote every day that they can spare, even after they have obtained their diplomas, to the improvement of the opportunities now afforded them. When deprived of them, they will discover, perhaps too late, their value; and the majority will find, to their sorrow, that their day of greatest medical knowledge was that on which they ceased to be students, and became practitioners.

HINTS FOR EXAMINERS.

EXAMINERS would be not a little surprised at the intimate acquaintance which is generally possessed of all their systems, and at the ingenious means which are resorted to to evade their vigilance, or slip through the loop-holes in their regulations. We question whether they themselves know nearly so much about their own laws as those whose interest it is to avoid them. For their information, we beg to present some announcements which this season has produced.

Here is a *morceau* for the Col-

lege:—"Gentlemen prepared, by private instruction, for passing the College of Surgeons, three guineas; during which time, those who have previously attended other schools, are permitted to attend Mr. D.'s public lectures" (on anatomy, and surgery, and demonstrations, all recognized,) "and dissections, and receive certificates of the same gratuitously." Now what does this mean, but that those who have, for any cause, been refused their certificates at another school, may buy them here for three guineas?

And here is a *bonne bouche* for the Hall:—

"Mingaye Syder, licentiate of, and senior private examiner for, Apothecaries' Hall, has continued a collection of notes, from the year 1821 to September 1837, embodying every question of the *least importance* ever mooted at the Hall during that period. To this advantage his method of conveying information, and the facility with which his system accustoms the student to express himself with perspicuity and becoming firmness, Mingaye Syder," &c.

Oh! "Mingaye Syder"—Galen, what a name! And then the "senior private examiner." Hail! thou Nestor of grinders; and the "becoming firmness" and the "perspicuity"—is rhetoric required? is a "*sermo lenis minimeque pertinax*," or "*cum sale et facetiis*," most worthy to be studied? But of all advantages, surely this is the greatest,—all the questions from 1821 to last Saturday night!! But, by the by, this is not peculiar to Syder; several complete copies, we are told, are extant; and a remarkable unity of character is said to reign throughout each. Surely it would be well to commence a new series in October 1837, even at the risk of ruining such valuable members of the profession as the above.

GUY'S HOSPITAL.

HYDROPHOBIA.

To the Editor of the Medical Gazette.

SIR,
PERHAPS the following case may not prove uninteresting to some of your readers; I think it shows the inefficacy of opium as an antidote in hydrophobia. The history of the case is perfectly correct, and was taken by myself while sitting by the

patient's bedside, which I did from the time of his admission till the period of his death.—I am, sir,

Your obedient servant,
FREDERICK CHARLES JONES.

Sept. 30, 1827.

Edward Lynche, aged 26, a strong and muscular man of moderate size, a native of Cork, has been in England for twelve months, and with the exception of periodical attacks of ague, has enjoyed a good state of health. About five weeks ago, while harvesting in Kent, he drove a dog from the barn in which he was living. Afterwards, on going into the fields, he accidentally left the door of the barn open, and two dogs entered. On his return to fetch a knife and fork, the dog which he had previously driven out flew at him, and bit him in the cheek and upper lip: he seized the dog between his knees, and strangled him with a bit of hayband, which he took from his pocket.

The lip, which was much torn, was immediately excised at the wounded part, but, according to his wife's account, nothing was done to the wound of the cheek. A subscription was entered into to facilitate his departure with his wife, who being in the family-way, it was thought best not to inform her of the possible consequences of the accident.

Lynche stated that his habits are temperate, but that on Saturday, the 23d of September, in the evening, he drank two or three pots of beer, after which he was seized with a fit of vomiting, and loathed the sight of meat; he slept well, however, on that night.

On Sunday, at nine in the morning, his bowels were open, but he was very unwell all that day, and in the afternoon fell into a deep sleep, from which there was some difficulty in awakening him. He had no spasms, but passed a restless night.

On Monday morning spasms took place, and he became worse; bowels not open; passed a quiet night; but on Tuesday the 26th, he was taken into the hospital at nine o'clock in the morning, and a bed was assigned him in Billet's ward. At a quarter before ten a turpentine enema was administered, the greater part of which remained in the intestines.

I saw him at a quarter-past 10, one hour after his admission; his pulse was soft, slow, and irregular; he complained of no pain, but a feeling of great lassitude; could drink no water, but took a table spoonful of milk; felt a sense of constriction of the throat: his voice feeble, and when speaking resembles a loud whisper; the skin moderately warm; extremities cold; no perceptible perspiration; pupils

dilated; tongue moist and clean; countenance anxious: on the approach of any one a slight spasm takes place, and he appears as if being choked. Dr. Babington was immediately sent for, and on his arrival at about noon, found the previous symptoms much aggravated. Pulse 60; heat normal; tongue slightly coated, but moist; skin of natural warmth; no pain; chest sonorous upon percussion; respiratory sound natural; breath of an acid odour. Every two or three minutes he was seized with spasm, characterized by violent and gasping efforts at respiration. A high degree of nervous irritability was apparent, and was increased by the presence of visitors, and even by light, so that he kept his head turned from the windows, and his eyes closed.

Dr. Babington, in consultation with Dr. Baek, ordered ʒj. of the carbonate of iron to be given in the form of bolus, every half hour; his spine to be well rubbed with ceratum cantharidis, and a belladonna plaster to be then applied along its whole course. While these means were in preparation the spasms became more frequent, and were excited by the most trivial causes, such as opening a door, the movement of a person about the room, &c., owing, I suppose, to the vibration of the air upon the face. Even flies seemed to cause him much annoyance.

About half-past 2 he sunk into a refreshing sleep, which lasted for three quarters of an hour; the ward door was ordered to be closed, to prevent the patient from being disturbed.

5 o'clock.—He complained of great prostration of strength; the spasms were greatly increased in intensity; the tongue moist; pulse 54, irregular and intermittent; skin rather warm: he felt no pain in the lip, but had the sensation of a ball rising in his throat; when he spoke it was with great exertion, and like a man out of breath; quite sensible. He has passed his urine three or four times, rather high coloured.

7 o'clock.—Getting rather more restless; pulse 84, sharp; tongue dry, mouth parched, skin hot, but no perspiration. The boluses were divided into three, and after each he took about two ounces of milk, which was all the nutriment he had since his admission to the hospital. He had taken five of the boluses, and when taking the sixth he experienced great difficulty in swallowing, in consequence of the flatus meeting the substance passing into the œsophagus. Pulse, while I was writing the above, sunk to 64; he appeared very much convulsed, and could not bear any one to stand before him, but did not mind my standing at his back.

10 o'clock in the evening.—He was

more convulsed; and Dr. Babington, finding that no diminution of spasm had resulted from the use of carbonate of iron, ordered him three grains of muriate of morphia every half hour till a mitigation of the symptoms should ensue; and that, in addition to the plaster down the spine, his breast and the region of the diaphragm should be rubbed with ceratum cantharidis, and a belladonna plaster applied over the stimulated part. Pulse 54, but rather full. He was more restless; could not bear to have the candle brought into the room; his voice, however, was more natural, and there was no rigidity of the muscles. His cries were loud and frequent, and he did not like any one but his wife to come near him.

At half-past 10 he took three grains of muriate of morphia in the form of pill, and was getting more restless.

At 11 he took three grains more; the former dose appeared to have given him some relief, and he was becoming more composed. Slept till 3 A.M. on Wednesday. At half-past 3 he was rather restless, and the dose was repeated.

5 o'clock.—Passed a quiet night upon the whole, crying out, however, at intervals. Pulse 68; described himself as better, but rather sick. He took a cupful of milk, which he asked for himself, but swallowed with great difficulty. Bowels not open since his admission.

7 o'clock in the morning.—Getting more restless; pulse 80; tongue dry and furred; made water freely; skin hot, but no perspiration; said he felt no pain, but felt a weakness at his heart.

8 o'clock.—Asked for some water; took two ounces, but immediately jumped up in bed, and appeared choking. The pupils were contracted; spasms more violent. Repeated the dose of morphia.

10 o'clock.—Was very violent; and when Dr. Babington entered the room, jumped out of bed in a frantic manner, yet apparently without any definite object. No persuasion could induce him to return, till at length he promised to do so if left alone. The experiment was tried, but no sooner had all parties retired than he bolted the door, made a rush at the high wooden railing which separated him from the windows, and, before an entrance through another door could be gained, thrust through two squares of glass over the railing which he had forcibly torn away. His wife (who had not long quitted him) now arrived, and succeeded in inducing him to return to bed. To avoid future accidents to himself or others, since he was evidently growing more unmanageable, it was thought necessary to secure his arms and legs, which were fastened to the bed by means of sheets. The confine-

ment excited him much at first, but he gradually became reconciled to it. He was particularly anxious, however, that only one person should enter his room at a time. Dr. Babington ordered him an additional grain of muriate of morphia every half hour.

11 o'clock.—Tongue parched; skin moist; pulse 86. Had had no stool; bellowed out very loudly, in a deep hoarse voice; after each paroxysm he became very low; his countenance was expressive of great anxiety, and his manner extremely irritable. He was decidedly much worse.

Half past 12.—Still keeps crying out, and asks for milk, but cannot bear the sight of the cup, which he directs may be kept under his bed. The fluid makes him sick, or rather is forcibly ejected from his gullet. He has had two doses of 4 grs. of muriate of morphia, and is now taking 8 grs. more; which Dr. Babington has ordered to be repeated every half hour, until a decided control over the paroxysms is established. Skin moist. He is to have a tobacco enema exhibited as soon as possible. The voice is becoming more thick, the breathing more laborious, and the feeling of oppression much increased. The pulse 140 after the paroxysms, but he expresses no more horror of fluids than of any thing else he is requested to swallow. An apple was offered him, and he bit a piece from the side, but was unable to masticate it.

A quarter before 2.—The angles of his mouth continually elevated and depressed; eructation, accompanied by a noise having a slight resemblance to the howl of a dog; very much convulsed; countenance very livid; eyes having an up-turned expression; breathing about eight times in a minute; pulse 180, small; contraction of the occipito-frontalis muscle; chest heaving; breathing very laborious; changing rapidly for the worse. Tobacco enema cannot be administered, on account of his violent spasmodic movements. His lower extremities were confined in the same manner as his arms. Pulse hardly perceptible; appears perfectly unconscious of what is taking place, but seems to have some ideas of a pleasing nature, for now and then the countenance assumes a serene smile, evidently not owing to convulsive contraction. Pupils strongly contracted, and not obedient to light; conjunctiva much injected.

2 o'clock.—Dr. Babington sent for. Pulse 195; twitching at the bed-clothes, and pulling his neck about very much. All the bad symptoms considerably augmented. From this time till the period of his death, which took place at twenty minutes past 3 in the afternoon, his pulse was continually varying. He died coma-

tose, and immediately after death his pupils were observed to be very much dilated. On attempting to obtain some blood from the arm after death I failed. Some serum flowed about half an hour afterwards, and in the course of three or four hours the sheet was found saturated with blood.

ST. BARTHOLOMEW'S HOSPITAL.

Morbid alterations in the Articular Tissues from Chronic Inflammation.

Aug. 17, 1837.—The knee-joints removed by amputation from two patients on the 15th, were examined, after having been injected with size and vermilion, and presented some points of interest in relation to the changes induced by long-continued inflammation in the several articular tissues.

The symptoms during life had clearly indicated chronic inflammation of the synovial membrane as the primary disease, upon which ulceration of the cartilages, elongation of the ligaments, and suppuration and ulceration around the joints, had supervened. In one case (that of the child) the disease had lasted for nearly two years; in the other (a man of 32) for fourteen months. The morbid appearances in both were almost precisely similar.

The ligaments were softened and lengthened; they had lost all that density and that peculiar wiriness of feeling which they present in a healthy state, and they could be pressed out between the fingers, or scraped into a fine membrane. The external lateral ligament was merely lengthened as it extended over the swelling, but preserved its rounded form; while the internal, besides being equally lengthened, was expanded like a membrane over the thickened subjacent tissues. The crucial ligaments were torn across with but little effort, in separating the heads of the tibia and femur.

The synovial membrane was in some parts at least half an inch in thickness, and presented a soft pulpy reddish surface, lining dense fibrous and gelatinous layers. This change had affected it in its whole extent, and where opposite surfaces similarly altered had met, adhesion had taken place, and the cavity of the joint had there been obliterated. In many parts, similar pulpy matter was effused upon or beneath the synovial membrane covering the cartilages on the heads of the bones, presenting the same reddish vascular surface, and here and there united to the adjoining similarly altered parts. The vessels of that part of this deposition

which lay upon the cartilages, were evidently derived from the synovial membrane around their edges, and did not pass from the bone through the cartilage, which beneath many of these spots appeared quite healthy.

In addition to the changes above described, as found on portions of the articular surface of the cartilages, these were in several parts completely removed by the absorption which highly vascular granulations, arising from the subjacent bone, had effected. In some small spots, these granulations protruded through holes which they had made in the cartilage, and presented bright red, fungous-looking papillæ, projecting into the joint. Where they had grown to a less extent, the cartilage was as it were undermined by them, and could, for some distance, be stripped off with the greatest ease. In one of the joints the cartilage had been completely removed from the inferior and posterior part of the condyles of the femur, at the part where, in the bent position of the limb, it had been in contact with the upper surface of the femur, and here a firm, very hard, polished surface of bone presented itself.

In the limb of the adult the bones had retained their natural structure, except in the part immediately adjacent to the cartilages; but in the boy's leg they were highly vascular, and their walls were considerably thinned: this was rather singular, because almost the only difference in the symptoms had been, the far less degree of pain which the boy had suffered for several months than the man had.

A few days afterwards a knee-joint was obtained after death, from a young woman who had died of phthisis. It had evidently been affected some years since with morbid processes, in some respects similar in character to the above, but had been long since cured, for, except in partial stiffness, and a slightly flexed state of the limb, no outward appearance of disease existed. The ligaments were healthy, and of their proper length and firm texture, and the synovial membrane was but slightly thickened. Many large patches of cartilage were completely removed. In the preceding cases it had appeared as if the pressure on the opposed surfaces on the heads of the bones had induced an early absorption of the cartilage at those parts; here, however, it was perfectly sound in these situations. In the parts where it was removed entirely, a thin filamentous cellular tissue was found on the surfaces of the bones, where they were in contact with synovial membrane; while in the other case, where the bones had come in contact with each other, they had assumed a smooth surface and an ivory hardness. In

some situations the same cellular tissue extended on to the edge of the cartilage, and firmly adhered to it, but it could not be seen if vessels passed from one to the other. The slighter degree of alteration in the synovial membrane over the cartilages, shewed very clearly the important modification which is induced in the effects of inflammation in the same tissue, by differences in those subjacent to it, and from or through which it receives its vascular supply; while the difference observed in the surfaces of the bones as they were in contact with each other, or with the synovial membrane, presented an analogous instance of the effects which external circumstances have on the products of an exposed and inflamed surface.

Singular Production of Urticaria.

In the stomach of a horse, which had been brought in the morning from the knackers for a preparation, there were found a number of large lumbrici. As two gentlemen were endeavouring to inject the intestinal tubes of some of these with mercury, one of them was seized, about five minutes after he had commenced handling the worms, with a violent fit of sneezing, accompanied by a profuse flow of mucus in the nose, swelling of the Schneiderian membrane and integuments, so as to be evidently visible externally. These symptoms were soon followed by an eruption of nettle-rash over all the face and neck, with the most intense itching and heat, which increased as the irritation about the mucous membrane of the nose became less. After about ten minutes the eyelids swelled and became œdematous, the conjunctiva was reddened, and a considerable secretion of tears took place. Then the eruption appeared on the hands, and soon after, in a slighter degree, over the arms and chest. The annoyance continued extreme for nearly an hour, and the itching and redness from the urticaria did not disappear till five or six hours after it first commenced.

The other gentleman was affected in precisely the same manner, about a quarter of an hour after he began to attempt the injection; but in him the symptoms were less severe, perhaps from his being in the habit of taking snuff: none of them, however, were absent.

Nothing but the effluvia from these lumbrici, which were somewhat evident by a strong rank smell, could have produced these effects; no putrid matter of any kind was lying near, and the two persons simultaneously affected had not been exposed to any other common cause that could have led to it; indeed, they had not been together for ten minutes before the affection commenced.

WESTMINSTER HOSPITAL.

Case of Impervious Stricture—Newly-modelled Operation.

R. SHEPPARD, about 50 years old, was admitted, under Mr. Guthrie, on the 16th June ult., with stricture, of long standing, in the urethra: he had frequently been attacked with retention of urine, which had always been relieved by the warm bath. The stream of urine had been scanty for many years.

On the 25th, a few days after his admission, he was attacked with a paroxysm of retention. He was placed in the warm bath for half an hour, and a strong dose of castor oil and a drachm of Battley's opiate solution were given to him. Whilst he was in the bath, an attempt was made to pass a catheter for him, but without effect. But being easier, though not relieved of his urine, he was put to bed: he slept quietly till six the next morning. He then became uneasy, and was again placed in a warm bath, the pain being relieved, but no urine could escape.

At 1 P.M., 26th, the patient had great anxiety of countenance; the bladder reached to the umbilicus, and the perineum was tumefied and painful. There was much fever. Mr. Guthrie endeavoured to pass a catheter, but did not succeed. He then had applied twenty leeches to the perineum, and fomentations to the abdomen, and a clyster of laudanum and gruel was thrown up into the rectum. These measures produced temporary relief.

At 6 P.M. all the symptoms were aggravated, and Mr. Guthrie in consequence determined to operate. The patient was placed on the table in the position proper for lithotomy. The surgeon introduced a catheter (No. 8) as far as the stricture, and an assistant firmly held it in that position. Mr. Guthrie now placed his left index finger in the rectum, and ascertained the exact seat of the stricture. Retaining his finger in the stricture, he made a transverse incision in the perineum, an inch and a half above the anus, and cut through the several fasciæ to the seat of the stricture, which he divided; he then passed on the catheter into the bladder, and drew off three pints of urine, of which the last portions were highly ammoniacal. The patient was relieved, and the wound being dressed, he was put in bed, and an opiate was given to him.

27th.—The patient had a good night. He is quite easy, and the urine flows through the perineum as well as along the urethra.

28th.—He had a good night without opium; this morning pain in the penis

came on, and all the urine came through the perineum. In the course of the day the symptoms were aggravated. In the evening the penis was swollen and painful, and he had thirst, anxiety, and heat of skin. His bowels opened with castor oil. Fomentations were used, and an opiate subsequently administered.

29th.—The opium gave him a good night, and he is quite easy. His countenance is tranquil; his pulse 98, and soft; he passes his water partly through the urethra and partly through the wound. Twenty leeches are applied to the perineum.

30th.—The scrotum being tumefied and painful to the touch, and the patient having severe pain in the testicles and perineum, Mr. Guthrie made two small incisions in the scrotum, and let out some pus. No urine flowed from the wounds.

July 1st.—The pain of scrotum is less, but the penis is more swollen. The penis is therefore scarified all over, and relief is afforded.

2d.—He had a good night's rest. This morning the penis is more swollen, and a black slough has made its appearance in the integuments of the penis, on the under surface of the organ, about two inches behind the perineum: the slough enlarged, and nearly surrounded the penis like a broad belt. Mr. Dasent made two incisions through the integuments lengthwise, along each side of the penis; about an ounce of blood flowed. Fomentations applied to the inflamed surface.

4th.—The patient is much better. The slough (as large as a crown) has separated.

Sept. 2d.—The cicatrix formed by the loss of the slough has contracted, and the penis is consequently distended during priapism. A gum elastic catheter (diameter, No. 9,) is introduced in order to provoke the absorption of the stricture.

6th.—The catheter is withdrawn to day, and the patient is able to make water in a stream of moderate size. There is still some slight irritability of bladder, the patient being obliged to get up three or four times in the night for the purpose of micturition. His health is good.

Mr. Guthrie observes upon this operation—"The improvements which have been made in surgery of late years, render this operation less necessary than formerly; cutting out and cutting into portions of the urethra, are, like cutting out testes, comparatively obsolete operations; but still an opening into the urethra may occasionally be required, and when the disease is situated at the termination of the bulbous portion of the urethra, or further back, I recommend the operation to be done in the following manner:—The pa-

tient being placed and secured, as in the operation for stone, a sound is to be passed down to the stricture and held against it; the rectum having been previously cleared by an enema, the index of the left hand is to be introduced into it, and the membranous part of the urethra and the prostate are to be examined, as well as the bladder. If the membranous portion be dilated with urine, so much the better; but the object of introducing the forefinger is to ascertain the relative situation of the upper part of the rectum and the urethra, which latter organ only touches the rectum at the termination of its membranous portion and the commencement of its prostatic portion. There is a certain distance, which is greater or less in different individuals, between the last inch of the rectum and the urethra placed above it. The two parts form two sides of a triangle; the apex of which is the prostate, the base the external skin. It is within the two lines of this triangle that the operation is to be done. The surgeon taking the catheter in his right hand, whilst the forefinger of the left is applied to the upper surface of the rectum, moves the point upwards and downwards, so as to communicate with the forefinger of the left hand, and to convey to it a knowledge of the situation of the extremity of the instrument, and particularly of the distance between them; which the motions given to the catheter by the right hand will clearly indicate. The next step of the operation is to divide the skin, cellular membrane, fascia, and muscular and tendinous fibres, which intervene between the rectum and urethra. There are two ways of proceeding: the first, when the obstacle is behind the bulb and the exterior parts are not diseased, may be commenced by a straight incision in a perpendicular direction. If the surgeon be not well acquainted with the anatomy of these parts, or if they are very much hardened, a transverse curved incision should be made across the perineum, about a quarter of an inch above the verge of the anus. This gives room. If the transverse incision be not adopted, the point of the straight bistoury is to be placed on the skin, a little above the verge of the anus, the cutting edge being above and the back towards the rectum, the handle a little depressed, and the apex directed upwards a little. The knife is to be carried for the distance of an inch in this direction, and clear of the rectum; it must then be carried upwards, and brought out in the median line, making a division of the external parts to the extent of an inch and a half. The part being sponged, the surgeon again introduces the bistoury in the median line, and deepens the cut, being guided by the touch of the

forefinger in the rectum. He then feels for the urethra with the right index-finger, an assistant keeping the catheter steady against the stricture, the end of which will now be readily felt. If the forefinger of the right hand does not go beyond it and touch the sound part of the urethra, the knife is to be resumed, and the urethra to be exposed and opened, which may be done at the apex of the prostate. Whether the stricture be now divided or not, the cure may be completed under either predicament."

Supposed case of Poisoning.

Hannah Mayes, a beautiful creature, about twenty-three years old, known as a respectable servant girl, was admitted the 16th September ult. When brought in, she was in a state of insensibility, which had existed sixteen hours. By the friends' account, this had been preceded by a fit of epilepsy, which she had been subject to for years. She presently came to herself, but, in about ten minutes afterwards, she had another fit of these hysterical epileptic or anomalous paroxysms.

As long as she lived, these fits occurred at short intervals of about ten minutes. The force and frequency, however, of the attacks diminished in proportion as the patient became exhausted, and she ultimately sunk in the course of a few hours, apparently worn out by the abnormal muscular efforts. During the intervals of intermission the countenance of the patient was placid, the face flushed, the palpebræ closed, pupils contracted, respiration short and frequent. A pyramidal tumor occupied the lower regions of the abdomen, inferred to be the pregnant uterus. In this part auscultation indicated the presence of foetal pulsation. The pulse of the patient was about 90, full and soft.

When the fits came on, the respiration was accelerated to 50 per minute. The facial muscles were first affected, producing by their unnatural contraction most painful mal-expressions. The voluntary muscles of the trunk and limbs were then excited, especially all those parallel to the spine. The spasms induced in these last muscles were occasionally of so tonic a character, as to produce an appearance of opisthotonos. The veins of the face and head became distended to the utmost of their capacity, and the pulse first became accelerated, and then irregular and inappreciable.

Somebody fancying that the pyramidal tumor was the bladder distended with urine, a catheter was introduced, and three drops of urine drawn off. A blister was applied to the chest; aloetic and colochin-tine purges were administered; the cro-

ton tiglium was placed on the tongue; but harassed nature answered not to these experiments, and gradually yielded to the grasp of death.

Some months previously she had listened to "the voice of the charmer," and become pregnant. To conceal the fact she left her situation, and took an obscure lodging in a low neighbourhood. She thus escaped the observation of her friends. She is said to have exhibited great depression of spirits as her pregnancy advanced, avoiding society, and indulging in hermit-like moodiness. Her mind was obviously tormented by a sense of the disgrace she has incurred; her appetite in consequence fell off, and she ate but very little for weeks. At last the symptoms described occurred; and the question to be decided in a medico legal point of view is, whether the morbid phenomena have been produced by drugs, administered with a view to her own destruction or that of her infant; or whether a naturally sensitive mind, deeply affected with a continuous grief, and acting upon a debilitated body, may not have engendered the suicidal symptoms which have been related. Surely the last supposition does not indicate a principle unprecedented in the moral history of man.

The body was examined twenty-eight hours after her decease. There was turgescence of all the blood-vessels of the brain; the cortical substance evinced a deep pink colour. No effusion into the ventricles had taken place. No tumor, or other abnormal growth, existed in any part of the encephalon. The lungs were congested to such a degree as to present an appearance of consolidation. No traces existed of bronchial inflammation; but the mucous membrane of the pulmonary viscera exhibited a reddish tint throughout: the secretions in these tubes were healthy, as regards both quantity and quality.

The stomach was filled with a bile-tainted fluid, as well as in fact the whole intestinal canal: no trace of disease could be made out in these tubes. A few patches of ecchymosis existed along the great curvature of the stomach; the contents of this viscus were subjected, by Dr. Barham, to a systematic analysis, and his report is, "That a careful application of all the usual reagents affords no evidence of any metallic substance existing in the fluid subjected to analysis."

The annals of hospitals afford not unfrequent examples of such catastrophes as the one just recited,—instances in which every-day events give rise to quiescent passions of a life-destroying intensity.

FEES AT UNIVERSITY COLLEGE.

MR. ATKINSON, of University College, has called our attention to the circumstance of the amount of fees for the lectures required by the College of Surgeons and Society of Apothecaries having been over-rated in the table which we published last week. We have in consequence made the necessary corrections, and re-published it on the wrapper of the present number. The general total being correct remains unchanged, and the School still in its place at the bottom of the list, being, whether for unlimited attendance, or that required by the College and Hall, considerably the most expensive in London.

THE LATE MR. BROUGHTON.

A VERY curious letter, by some one signing himself "A Friend of the Deceased," has been addressed to the Editor of the *Times*, to inform him that the account of the late Mr. Broughton, copied from this journal, was altogether erroneous, and that his death might be called "accidental," having arisen from a vein taking up some pus, which it carried "to the centre of circulation." These and other expressions prove the letter to have been written by some unprofessional person, and it would, therefore, be unreasonable to expect that he should know much about the case in a medical point of view. As to the rest, while professedly correcting, he inadvertently confirms our account in a very remarkable manner. Our information was derived from a gentleman on very intimate terms with Mr. Broughton, who visited him between the performance of the operation and his decease, and who saw the will to which we alluded. The only error of the slightest importance into which we fell was that, in speaking of "Cooper," we supposed he had meant Mr. Cooper, whereas it was Sir Astley who attended. If the writer in the *Times* be really "a friend of the deceased," we can only say he is a very injudicious friend.

NEW MEDICAL WORKS.

Traité de Physiologie, considérée comme science d'observation. Par C. F. Burdach, Professeur à l'université de Königsberg. Traduit de l'Allemand par A. J. L. Jourdan. Vols. 1, 6, 7, 8, are out: the others will appear in three months. Baillière.

Traité des Maladies des Reins. Par P. Rayer. 1ère Liv. grand, in-fol. containing five coloured plates. This work will be completed in 12 Livraisons. Baillière.

The Edinburgh Dissector, or System of Practical Anatomy; for the use of Students By a Fellow of the Collège of Surgeons in Edinburgh.

Guy's Hospital Reports, No. V. Oct. 1837.

INTRODUCTORY LECTURES.

WE have to apologize to those gentlemen who have requested us to publish their Introductory Lectures, for declining to do so: their interest is very much confined to the pupils and friends of the Lecturer.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, Oct. 5, 1837.

William Maw Tronsdale, Sheffield. — James Michael Knapp, Minthead, Somerset. — John Kersley, Lipscomb, Alresford. — James Musgrave Gane, Frome. — John Parsons, Goathurst, Bridgewater. — Eli Vickop, Blackburn. — George John Amsden, Huxton. — Thomas Browne, Walden, Essex. — James Liddlerdale, Hungerford, Berks. — Robert George Broxholm, Sunbury, Middlesex. — Thomas Keele, Bristol. — Thomas Jones Dyke, Merthyr Tydfel. — Henry Umpleby, Armin, Yorkshire.

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 32" N. Longitude 0° 3' 51" W. of Greenwich.

Sept.	THERMOMETER.	BAROMETER.
Thursday . 28	from 39 to 62	29.99 to Stat.
Friday . . 29	30 61	29.98 29.96
Saturday . 30	30 64	29.89 29.89
Oct.		
Sunday . . 1	42 65	29.86 29.89
Monday . . 2	51 68	30.03 30.09
Tuesday . . 3	49 70	30.03 29.90
Wednesday 4	56 68	29.92 30.02

Prevailing wind, N.E.

Except the mornings of the 28th ult., and 1st inst., generally clear; with rain at times.

Rain fallen, .0625 of an inch.

CHARLES HENRY ADAMS.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, Oct. 3, 1837.

Abscess . . . 4	Inflammation . . 18
Age and Debility . 40	Bowels & Stomach 5
Apoplexy . . . 5	Brain . . . 5
Asthma . . . 6	Lungs and Pleura 7
Cancer . . . 2	Influenza . . . 1
Consumption . . 55	Insanity . . . 2
Convulsions . . 27	Liver, diseased . 1
Croup . . . 1	Measles . . . 11
Dentition or Teething 9	Mortification . . 5
Diarrhoea . . . 2	Paralysis . . . 3
Dropsy . . . 12	Rheumatism . . 3
Dropsy in the Brain 8	Small-pox . . . 3
Dropsy in the Chest 1	Spasms . . . 1
Erysipelas . . 1	Thrush . . . 1
Fever . . . 16	Veneral . . . 1
Fever, Scarlet . . 1	Unknown Causes 11
Fever, Typhus . . 9	
Heart, diseased . . 2	Casualties . . . 9
Hoooping Cough . 7	

Increase of Burials, as compared with the preceding week . . . } 103

WILSON & SON, Printers, 57, Skinner-st., London

THE
LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, OCTOBER 14, 1837.

LECTURES
ON THE
PHYSIOLOGY AND DISEASES OF
THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL
AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of
1836 and 1837,

At the Anatomical School, Kinnerton-Street,
near St George's Hospital,

BY CHARLES J. B. WILLIAMS, M.D. F.R.S.

LECTURE III.

Vital relations of Respiration—Respiration an instinctive action—Its relations to the Nervous System—Nerves of the Lungs—Nerves of the Muscles of Respiration—Mutual dependencies of the Lungs, the Nervous System, and the Heart; illustrated by a Diagram—Asphyxia, its causes and pathology—Why the Circulation ceases in Asphyxia—Dr. Alison's Experiments—Succession of Death through the Organs—Hybernation—Various effects of Cold on the Functions.

WE have thus briefly surveyed the mechanism and the chemistry of respiration; it now remains for us to consider the function in its vital character—the relations in which it stands to the peculiar attributes of life—and why it is so essential to the life of the body at large. What is the cause which determines the respiratory movements? Is it something inherent in the muscles concerned; or are their actions dependent on a sensation, or other stimulus of the nervous system? Is respiration a voluntary or an involuntary act? How is the act excited first in the newly-born infant? Such are some of the questions that have occupied the wits of

great physiologists of many ages. They shall not detain us long. Time will not permit us to run through the diversified answers which successive authors have given to these questions; it will be enough to develop that which, as the most consistent with physiological, and especially pathological facts, will be most useful to us in our subsequent allusions to the function as related to life.

That respiration is not, properly speaking, a voluntary act, is obvious, inasmuch as it usually takes place without any act of the mind, even in coma and complete stupor, when the mind is incapable of willing. But we know, also, that we can at pleasure control respiration; we can increase it, quicken it, and for a certain time stop it. All the muscles of respiration are, therefore, subject to the will, although acts of volition are not necessary to their regular action. What is it, then, that excites them in this regular action? We may find out, by trying to its utmost the power of our will to stop respiration. We stop our breath for a little while by a direct control of the muscles of respiration; but soon we feel a sensation of oppression, which irresistibly throws these muscles into action; and the only way in which we can still stop our breathing is by closing the glottis, and keeping the diaphragm pressed upwards by the contraction of the abdominal muscles. In the course of a few seconds, however, the sensation of uneasiness in the chest becomes intense, the respiratory muscles make violent efforts, and the voluntary act which stops their play is soon conquered by the sensation becoming insupportable. It is stated by some writer on legal medicine, that a man once intentionally put an end to his existence by holding his breath; but it is not likely that the death was one of simple asphyxia, because the mental act of holding the breath could not continue when that mental stupor, that *defectio animi*,

came on, which always precedes this kind of death. But, as we shall afterwards see, the act of holding the breath causes such congestion and obstruction in the circulation, as might readily lead to a fatal result by other ways, such as hæmorrhage in the lungs, syncope of the heart, or apoplexy, in those individuals who are predisposed to these affections.

I would, with Dr Alison, class the action of ordinary respiration among those which are called instinctive, as being excited by an impression of which, in its ordinary degree, the mind is not conscious. Of exactly the same kind is the action of the eyelids in winking; but similar instinctive motions, equally unattended by any distinct volition, are excited in other voluntary muscles, by particular impressions or sensations—as vomiting by a certain degree of nausea, or by tickling the fauces; laughter by tickling under the armpits; the rapid withdrawing the hand or other part from fire burning, or any thing else severely paining it; and the very action of taking a breath on the sensation of sudden cold applied to the face or surface. The great distinction between these instinctive acts and those which are voluntary, is, that instinctive acts, however complex, are uniform in all individuals, and perfect from the earliest age; whereas the simplest voluntary acts which are preceded by a mental process, vary in different individuals, are imperfect at first, and require practice and education to make them perfect. Thus the child breathes well at first, but does not walk or talk well till after long practice. The sensation which prompts the respiratory act is one which remains after common sensibility ceases, and when the mind gives no sign of activity, as in coma, or in profound sleep; and it has been proved by the direct experiments of especially Legallois and Flourens, that of all parts of the nervous centres, the medulla oblongata is alone essential for its continuance. The brain and cerebellum may be removed without stopping it; but whenever the medulla oblongata is removed, or materially injured, respiration immediately ceases. So respiration is stopped by those influences which destroy sensibility; but it requires a greater degree of them than that necessary to destroy common sensibility, the sensibility which prompts the respiratory act being one of the last to abandon the body. Thus opium in a large dose first throws a person into that state of stupor in which he is insensible to common pain, pinching, &c., but he breathes still. He breathes, it is true, in a laborious and imperfect manner; and although the impression which is acting on the remaining sensibility must be of a very intense kind, it becomes power-

less as this is lost; and the respiration then ceases. A similar result is produced by apoplexy, or any injury which presses on or hurts the medulla oblongata. In apoplexy the pressure is more or less general on parts of the brain connected with common sensations, and these go first; whilst, if the pressure is considerable, the sensibility connected with respiration follows, as in the case from opium. But in direct injuries to the medulla oblongata, or upper portion of the spinal marrow, as where the neck is broken, respiration is destroyed from the first, and death is immediate. But the effect of these injuries or influences may be twofold: it may paralyse those nerves which give sensibility to the lungs; and it may paralyse those motory nerves which convey to the muscles of respiration the stimulus of the sensation or impression; and this brings us more closely to the relations which subsist between the lungs and the nervous system.

The nerves of the lungs (to which I did not advert in speaking of the mechanism) are from the eighth pair, or par vagum. These nerves, just before they enter the lungs, inosculate with the great intercostal; but, according to Reisseisen, the par vagum is the only nervous trunk distributed on the lungs.

Now the eighth pair appear to be nerves of motion as well as of sensation. Their sensitive function we have already dwelt on; and we find in the bronchial muscles, and especially in those muscles which open the glottis at the moment of inspiration, and are supplied by the recurrent branch of the eighth pair, the objects for the motory office of these nerves. The share which this nerve has in transmitting to the sensorium the impressions which excite the muscles of inspiration, was manifested in some recent experiments by Dr. M. Hall and Mr. Broughton, in which pinching the nerve was immediately followed by an effort at inspiration; and that this does not depend on the motory functions of the nerve, is obvious from the fact, that the same effort is excited when, after the division of the nerve in the neck, its upper segment only is compressed, which is exactly what occurs with nerves of sensation, while the contrary happens with motory nerves. The division of this nerve does not at once destroy the sensibility on which the respiratory movements depend, but it greatly impairs it; for mechanical irritation of the bronchi will not then excite coughing, and expectoration consequently ceases. Another effect of the division of the nerve has been noticed by Mr. Swan, namely, that the air which is introduced into the lungs in inspiration, is not duly expelled by expiration, and, accumulat-

ing in the cells, causes a certain degree of permanent distension. This implies a degree of paralysis of the bronchial muscles, the contraction of which we have before noticed as a part of the forces which complete the act of expiration, not as being of much force in itself, but as equalizing and aiding the diminution of the volume of the lungs, which is mainly effected by the pressure on them of the diaphragm, and of the parietes of the chest. I would beg your attention to a circumstance which seems to shew that this incomplete expulsion of air is also to be greatly ascribed to a loss of sensibility in the bronchial membrane.

If, after an ordinary expiration, we hold our breath until the sensation in the chest becomes painful, and then breathe, the first act is usually a quick, but short expiration, followed by a deep inspiration, as if the lungs were even more anxious to expel the remaining foul air, little as it is, than to draw in pure air. When we hold our breath after inspiration, the disposition to expire air is of course much stronger; and the act of merely breathing out a little slightly relieves the sense of oppression, and enables us to hold on a few seconds longer without taking in more air. These things seem to imply that the bronchial membrane is irritated by the presence of foul air, and seeks to get rid of its presence, both by the contraction of its own muscles, and by exciting those distant muscles concerned in expiration, which instinctively obey the peculiar sensation or impression which is present. It is not unimportant to notice these circumstances, as shewing that expiration is not merely mechanically necessary to prepare for inspiration, but that it is also a vital action under the influence of its own sensibilities and corresponding forces, as we have seen with respect to inspiration.

We have seen that the division of the eighth pair in the neck, although it greatly impairs respiration, does not arrest it; and a sufficient explanation of this may be found in the innervation of these nerves with the intercostals, through which the impression which excites the respiratory act may still, in a certain degree, be transmitted to the medulla oblongata.

The relations between the lungs and nervous system that we have hitherto considered, are chiefly those of sensation; but there is an indirect relation between the lungs and certain motory nerves, which is equally important in maintaining the circle of communication between the lungs and the mechanism by which they are filled and emptied. The chief motory nerves supplying the muscles of respira-

tion are the phrenic, which go to the diaphragm, the intercostal twigs of the spinal marrow to the intercostal muscles, the spinal accessory nerve to the sternomastoid and trapezius, the external respiratory to the serratus magnus, various of the cervical nerves to the scaleni, and other muscles more or less concerned in raising the thorax. It has been found by experiment that the section of these several nerves, or of the spinal marrow above their origins, suspends the action of their respective muscles in the respiratory movements. There are, besides, some motory nerves which co-operate, not through muscles which alter the capacity of the chest, but through those which keep open the canals through which the air passes into the chest. Of these the recurrent branch of the eighth pair supplying the crico- and thyro-arytenoid muscles which keep open the glottis, and the portio dura of the fifth pair, through which the nostrils are dilated, are the chief; and of these it has also been found by experiment, that a section of the nerves prevents the free opening of the respective passages. Dr. H. Ley has pointed out pathological facts which illustrate the same point with regard to the recurrent nerves, in the case of spasm of the glottis from tumors under the upper part of the sternum compressing these nerves, which generally antagonize the superior laryngeal nerves which influence the muscles closing the glottis.

The various motory nerves concerned in respiration, may be excited by other sensations besides that from black blood and foul air in the lungs: thus cold water dashed on the face will cause a deep inspiration; tickling the nostrils will cause sneezing; irritating the glottis, coughing; pain, sobbing, &c. We avail ourselves of these sympathetic associations of nerves and muscles to excite respiration in new-born infants, and to restore it in cases of suspended respiration. That there is sufficient ground for constituting these various motory nerves into a system distinct from those which move the muscles in obedience to the will, as supposed by Sir C. Bell, is very doubtful; and the reason assigned by him for their exciting and combining the respiratory movements, that they originate exclusively from the lateral columns of the spinal cord, from which also the great sentient nerve of the lung, the eighth pair, arises, is, as Dr. Alison remarks, by no means established. It would be easy to prove, by a reference to the anatomy and functions of the nerves, that two nerves arising from one root do not necessarily act in concert. We must, therefore, add that the reason of the concert is not explained; but its cause, its object, is

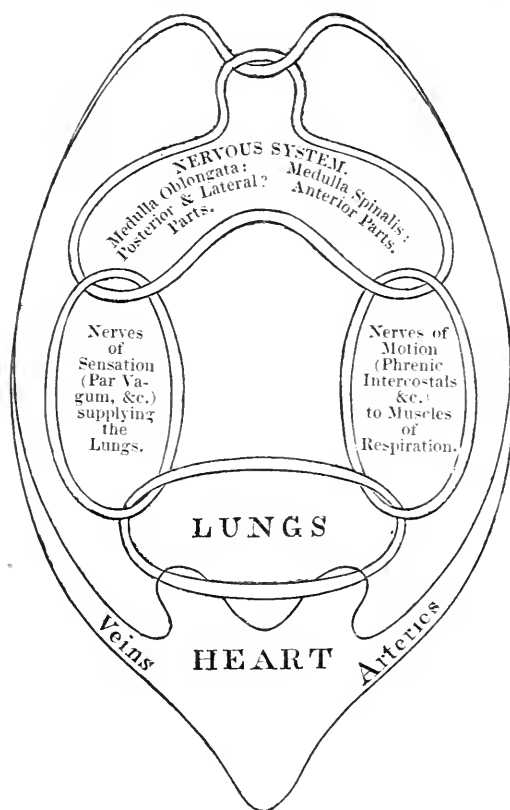
obvious, as Sir C. Bell himself observes, in the nerves and muscles employed to aerate the blood, "being put under the guidance of a sensibility more certain and more powerful in its effects than the will."

There is thus made out this double relation between the lungs and the nervous system—that of *sensation*, acting from the lungs to the medulla oblongata; that of *motion*, from this nervous centre, through various nerves, to the muscles of the chest which move the lungs. The diagram given further on exhibits these relations. That there are, besides these, other *occasional* relations between various parts of the nervous system and the lungs, capable of *influencing* the functions of the vessels of the latter, is very probable; but that these relations are constant, or in any way necessary to the function of respiration, or to the nutrition of its organs, as supposed by Dr. Wilson Philip and others, is a matter of mere hypothesis, unsupported by any unequivocal fact, and rather opposed than otherwise by the known attributes of the nervous system. It might be interesting, but I doubt that it would be profitable, for the purposes of these lectures to enter further on these matters.

We have thus seen how the function of respiration hangs, as it were, on that of the nervous system: we have now to trace the converse link—how the nervous function is also dependent on respiration. This link, most important in itself, and equally important in spreading the effects of respiration through the system, is constituted by the heart and the double circulation connected with it. All that we learn from the mere mechanism of the heart and its sets of tubes is, that it is admirably adapted to carry the blood in one direction—from the right auricle to the right ventricle, from the right ventricle to the pulmonary artery, which subdivides again and again until its branches are of that size which constitutes the capillary bloodvessels of the lungs. These form a network around the air-cells and minute tubes, from which network the many smallest branches of the pulmonary veins arise, which unite into fewer larger ones; these, again, converge into the four pulmonary veins which bring back the blood to the heart, now entering the left auricle; from this it passes to the left ventricle, and thence is distributed by the aorta and its various branches throughout the system. These branches subdivide, until, as in the lungs, they form the capillary network, from which again arise the veins, which converge into trunks; all these unite in the venæ cavae, which, entering the right auricle, completes the circulation. All this course is plain from the mechanism of the

valves of the heart and veins, and can be imitated in the dead body, by injecting liquids through the vessels. This is the *hydraulic* apparatus of the circulation, and, as such, it is regulated by hydraulic laws, which deserve their share of our attention, and will be further considered in the third part of this course, when we come to treat more particularly of the heart and great vessels.

But we have now to think of the vital properties that are added to the mechanism, the contractility of muscular fibre, and the relations in which that contractility stands to the quantity and quality of the blood that excites it, and the relations which subsist between it and the nervous system, which can influence it. We have also to think of the blood, not merely as an inert liquid, passively propelled through pipes, but as a liquid teeming with chemical and vital properties, which are continually acting on the vital properties, and, through them, on the mechanical powers, of the various solids through which it passes. Between all these properties there are certain relations or proportions which may be called healthy or natural, inasmuch as they tend to support each other with such an even balance as leads to durability and permanency. Thus, for the proper or natural action of the heart, its right cavities should receive a due supply of venous blood, which being their proper stimulus, excites them to propel it into the pulmonary artery; its presence there excites, in the nervous system, the impression which leads to the movements of respiration; these, by bringing the air to act on the blood, renders it arterial, and thereby fitted to return by the pulmonary veins to the left cavities of the heart, of which, thus arterialized, it is also the proper stimulus; from these it is propelled through the various arteries to all parts of the body, of whose functions and textures it is the proper support and pabulum. Among these parts which thus require the supply of arterial blood, are the substance of the heart itself, which thus supports its own function, whilst its function is necessary to effect this support: the nervous system, which as it is necessary to maintain the function of respiration, so in receiving arterial blood it becomes through the heart dependent on that function; and the muscles of respiration in like manner depend on their own work through the heart, which supplies them with its product. Thus is the chain of mutual connexion and dependence complete, and it may be seen in this diagram how any break in this chain must destroy the order of the whole arrangement, and arrest that circulation of influences which is the characteristic of life.



THE CHAIN OF LIFE.

The dependence of the heart on the nervous system through the lungs, is sufficiently plain in this diagram; but you may see that there is an attempt also to represent that the action of the heart is otherwise independent of the nervous system, although it may be influenced by it. Thus the nervous system hangs on the heart for its supply of blood; but the heart hangs on the nervous system only through its link with the lungs. Substitute for this link artificial respiration, and you may then *quietly* cut away the nervous system without disturbing the heart's action. But if you do *violence* to the nervous system, you may shake and disturb the heart also. You may find this diagram useful in studying the causes of asphyxia, syncope, and other kinds of death.

Having thus analyzed the relations, vital and physical, of the chain or tripod

of life, the heart, the lungs, and the nervous system, the nature of *asphyxia*, which consists in an interruption to those relations, will be generally intelligible. The etymology of *asphyxia* implies the very converse of the pathological condition to which the name is applied. It means no *pulse*; whereas the condition to which it is applied, consists essentially in no *breath*. It is true, the pulse must soon cease, as you perceive, when there is no breath; but the characteristic of *asphyxia* is, that there is pulse when breathing has ceased. The name is, therefore, obviously improper. With this understanding, however, and using the term under the figure of *lucis à non lucendo*, we inquire what are the causes of *asphyxia*, that is, what will stop the breath? We see in the diagram an answer with regard to the nerves and their centres,—any thing that cuts off communication with, or any thing

which destroys the function of, this system, whether by physical injury to their mechanism, or by poisons acting on their vitality. But, besides, we have the mechanism of respiration,—any thing which stops that, any thing that renders its movements ineffectual, by cutting off the supply of pure air, annihilates the function of respiration, and stops its essential important effect, the arterialization of the blood. Well, now, let us trace the successive phenomena of this stoppage of respiration, how they follow one another. The oxygenation of the blood in the lungs being arrested, this blood necessarily remains venous; and from the mechanism we should expect it to pass on in this venous state to the left cavities of the heart: a little does so pass; but it is only a little; the greater portion is arrested at the lungs, waiting, as it were, for the supply of air, and being refused any onward progress, the accumulation of blood distends the right cavities of the heart, then the venous trunks, and a great portion of the venous and capillary system; the left cavities and arteries being nearly empty. It was supposed, by Goodwyn and Bichat, that the black blood became distributed through the arteries, and acted on the parts through which it passed as a sort of poison, that suspended their functions, and that thus the death which began with the lungs was transferred throughout the brain and other parts of the frame, destroying speedily the heart also. But Dr. David Williams, of Liverpool, first shewed that very little black blood passes to the left side of the heart, but it is somehow arrested in the now motionless lungs. And Dr. Kay found that slowly injecting large quantities of venous blood into the carotid artery of an animal, did not suspend the function of the brain, nor cause the loss of sensibility which attends asphyxia. Dr. Edwards further proved that the irritability of the muscles of cold-blooded animals is preserved longer when only venous blood flows through them, than when they receive no blood at all, which could not be the case if venous blood had a positively noxious quality. Hence it is probable that the sudden failure of animal life in asphyxia is caused by the great want of supply of blood in quantity, rather than by its venous quality, and that this obstruction in the lungs destroys by soon arresting the circulation throughout the body. Hence the latter phenomena of asphyxia, the failure of the senses, sometimes with slight convulsions, resemble those of syncope, in which the heart at first ceasing to act, the circulation is at once stopped, and the functions of the nervous system also fail. Although, however, the defective quan-

tity of the blood may be the chief cause of the failure of the functions in asphyxia, I cannot but consider the venous quality of that which does circulate, or stagnate, as a concurring injurious influence. This blood, deficient in oxygen, and abounding in carbonic acid, produces in the brain the same symptoms that Nysten obtained by injecting carbonic acid into the carotid artery; and whether this venous blood is supplied directly through the arteries, or made to stagnate in the vessels of the head, by an impediment to its progress onwards through the veins to the lungs, all forms of asphyxia seem to agree in this condition—the presence of black blood, and of only black blood, in the brain and other organs.

A question of great interest arises from the view of the pathology of asphyxia, which the experiments just alluded to develop. The venous blood, not finding oxygen in the lungs, will not pass. What is it that stops it? The right side of the heart continues to beat, even after the left side, no longer receiving its accustomed stimulus, has stopped. The obstruction must, then, be in the lungs; and Haller supposed that the absence of the alternate motions of inspiration and expiration, which must assist the passage of the blood onwards through the capillaries, is sufficient to account for the obstruction. But Professor Alison has lately made experiments which throw doubt on this explanation. He found that when an animal is confined in azote until its breathing becomes laborious, and then killed instantaneously by concussion, the right side of the heart and the veins were as much distended with blood as where the respiratory movements had ceased. Here, he maintains, were the mechanical motions of the lungs, described by Haller, kept up to the last, but the pulmonary obstruction nevertheless existed. Dr. Alison views this obstruction as one of many other phenomena, which seem to prove that the motion of the blood in the capillaries is independent of any contractions or movements in the vessels themselves, or in any of the living solids; that it is caused in great measure by certain vital attractions and repulsions subsisting between the particles of blood and their containing vessels; and that these vital attractions and repulsions are greatly modified by the chemical changes which the blood may undergo in the various organs. In the lungs, the absorption of oxygen promotes through these powers the passage of blood through the capillaries. When oxygen is not present, this power of capillary circulation ceases, and the blood necessarily stagnates. A single breath is, however, enough to set it in motion; and by artificial respiration the

circulation of the blood may be restored after the heart has ceased to act, and when it (the heart) is re-excited by the oxygenated blood reaching it, propagated by these other powers. The correctness of this explanation is involved in the question as to the existence of these powers of vital attraction and repulsion, which is far too extensive for us to enter into here. The view that there are *physical properties peculiar to living structure, vital attraction and repulsion*, as we know there are *peculiar chemical properties, vital affinities*, is quite philosophical in principle; and the number of facts which Haller, and many of his countrymen since his day, have adduced in support of this view, are highly deserving of your attention.

But I confess that I wait for further arguments before I can be quite convinced that this view is absolutely necessary to explain the phenomena of asphyxia. 1st. I do not think that Dr. A.'s experiments are altogether conclusive against the explanation of Haller—that the obstruction of the blood in the lungs is caused by a stoppage of the *natural motions of respiration*. Dr. A. substituted azote for air, and supposed that the mechanical motions of respiration of this would be the same as of air; but from what I have before said, it is probable that air, deficient in oxygen, cannot be so freely respired as common air. Air that is kept until much deteriorated in the lungs, causes a forced expiration; and the reception of the same air again must very promptly excite the same action, until oxygen is supplied; so that the respiration of azote must consist in a preponderance of expiratory over inspiratory efforts, which would tend to retard the passage of blood through the pulmonary vessels, instead of promoting it, as the regular succession of equal in and out breathing does. I do not, however, suppose that this is more than one of the causes which obstruct the progress of blood through the lungs. But, 2dly, there is a contractile power in the blood-vessels which is capable of obstructing, although it cannot promote, the motion of the blood in them. The capillaries themselves do not seem to possess any power of this kind, but the larger vessels (both arteries and veins) have it, and are seen to show it on the application of an unusual stimulus. Now there seems to me nothing unreasonable in the supposition that venous blood is such a stimulus to the small branches of the pulmonary veins, which, being adapted to carry only arterial blood, may thus refuse a free passage to unarterial blood. These considerations will, I believe, go some way in explaining the impeded pulmonary circulation in asphyxia. The passage of blood is not wholly arrested,

but it is impeded enough to cause great distension of the right ventricle; and this distension, as we shall hereafter find, impairs the power of its propulsive action, by keeping open the tricuspid valves enough to cause regurgitation and venous pulse. This venous pulse doubtless answers the salutary purpose of giving a certain motion to the blood stagnating in the veins, which might otherwise coagulate; but it must add to the difficulties against the onward progress of the blood. The left ventricle receiving so little blood, and that not adapted to excite it to its usual activity, and sending so little of the same fluid through the coronary arteries to revive its substance, soon ceases to act; the arterial circulation also ceasing, those parts die which depend on its support, the nervous system dies, the muscles die, and at length the right side of the heart, failing to receive its pabulum through the coronary arteries, dies; and last of all, no longer receiving the vivifying operation of the functions and of motion, the blood coagulates, and dies also.

There is a condition illustrative of the relations of circulation and respiration, which is worthy of attention, as it presents us with a sort of separation of these functions—*hybernation*. When hibernating animals are exposed to a certain degree of cold, instead of exhibiting that power of reaction, of increased activity of respiration and secretion, which enables other animals to generate more heat, and thus to preserve for a time their temperature, they become torpid, gradually lose their temperature, the respiration becomes more and more rare, and at length ceases; and the circulation becomes very slow and very languid. In these animals, cold suspends entirely the animal functions, and greatly reduces the organic, but does not destroy the irritability of the heart, which, continuing to beat, circulates slowly a little venous blood; and this little, which would be wholly insufficient to support the functions when warmth develops them into activity, sustains them in the degree of vitality to which cold has reduced them. Warmth reapplied is the most effectual key to the locked-up properties of life: as the animal becomes warm, its sensibility returns; it feels the want of breath, muscles obey the call, respiration begins, circulation is quickened, secretions are excited, the self-warming power is regained, and the animal is raised, in the course of half an hour, from a state of animation below that of a cold reptile, a condition more like vegetation, to the vivacity and activity of a warm-blooded mammal.

When a hibernating animal is in its most perfect state of torpor, there is apparently no respiration, and the blood

which circulates is only venous. Now as by our diagram we have represented respiration to be the chief link by which circulation depends on the nervous system, this link here no longer existing, we should expect that the dependence which it maintains will be in a measure suspended. Accordingly, Dr. M. Hall found that the circulation of an animal in this state went on for nine hours after the gradual but complete destruction of the brain and spinal cord. Observe, however, that even in this state the nervous system could still influence the circulation; for a violent injury, such as crushing the brain, would arrest the circulation by a positively noxious influence, just as would happen from crushing a limb. So also sensibility is not entirely destroyed, but becomes of a latent and conservative kind, and is capable of being excited and developed into greater activity by strong impressions, or those of a destructive tendency. Thus even an intense cold, such as that of zero, will be felt by a hibernating animal; the sensation will excite the dormant functions, and the animal will awake from the intensity of the same agent that brought him to this state of torpor. The effect of gradual cold seems to be to lower the function of the nervous system in such a way that the lungs and other parts do not feel the want of arterial blood, but in their degraded state to be content with merely venous blood. The effect of sudden intense cold is different, but it illustrates the same point. A warm-blooded animal, suddenly exposed to intense cold, as by being immersed in ice-cold water, is brought into such a condition that the arterial character of the blood is not needed, and the veins return it to the heart in its flacid state; those functions which need arterial blood are paralyzed, and the chemical change from arterial to venous, itself also partly dependent on temperature, is therefore suspended. The paralyzing effect here, however, extends to the heart, which, although receiving arterial blood, cannot obey its stimulus.

The diminution of animal functions which hibernating animals present in the highest degree, is to a certain extent exhibited in most other animals. Thus on a severe winter's day, we see crows and other birds perched still and almost inanimate on the trees; the hunger which they must feel not impelling them to exertion. Even domestic poultry prefer to roost half their day, without food, to the exertion of wandering as they are wont to do in search of it. Nay, the longer sleep which the protracted absence of light in the winter brings on animals, is wisely intended for the same purpose as hibernation—to lower those functions the activity of which would

cause acute suffering at this intense season, and for the maintenance of which there is no longer an external supply. Man, and the animals which he domesticates with him in his artificial state, exhibit the least of this natural adaptation to seasons: for this he substitutes artificial protections and seasons; and thus keeping the functions in their condition of full activity, he incurs, on any accidental neglect of his artificial means, risks from sudden transitions or exposure, from which other animals are comparatively free.

ON THE HUMAN VOICE.

BY PROFESSOR MÜLLER.

[Concluded from page 43.]

IN the cut-out larynx two completely different sets of notes may be produced, when the vocal ligaments have a very slight degree of tension; notes, generally deeper, which have the most exact resemblance to the chest-notes; and others, generally higher, and the highest, which are in sound just like those of the falsetto. These notes may be produced with the same degree of tension; sometimes the chest-notes are given out, and sometimes, with the same tension, the falsetto. In a certain tension of the ligaments the notes have always the sound of the falsetto, whether one blows weakly or strongly. In greater relaxation of the ligaments those produced are the chest-notes, whether one blows weakly or strongly. In very slight tension it depends on the force of the blowing whether chest or falsetto notes are produced, the former being most easily produced by very gentle blowing. For these experiments, which succeed far best with male larynges, the plan last-mentioned must be followed, *i. e.* the arytenoid cartilages and whole larynx must be fixed vertically, the glottis closed posteriorly. They seem to prove that the chest and falsetto notes do not merely depend for their production on particular degrees of tension, as Liscovius supposed, since they may be both sometimes obtained with the same degree of tension; and the chest notes vary not according to the narrowness of the glottis, but according to the greater or less degree of relaxation of the ligaments.

If the vocal ligaments have so slight a tension, or so slight a degree of relaxation, that either chest notes or falsetto notes may be produced by the same means, it is evident that the falsetto notes are not at all of the same class as the flageolet notes (harmonics) on strings, which are pro-

duced by the vibrations of aliquot parts of the length of the string, for the vocal ligaments may be clearly seen to vibrate in their whole length. The essential difference of the two sets of notes consists in that, in the falsetto, the thin edges only of the vocal ligaments vibrate, while in the chest notes, the whole ligaments vibrate freely, and with great excursions. In accordance with this, it is observable that in the production of the falsetto notes, the edges of the ligaments only vibrating, the aperture of the glottis may be clearly seen between them; while, in the chest notes, the excursions are so great, that the glimmer of the vibrations of both ligaments is mixed; and not only they, but the limiting membrane below the lower vocal ligaments, which is covered by the lower part of the thyro-arytenoid muscle, enters into free vibrations.

The chest notes become deeper the more the thyroid cartilage is approximated to the arytenoid; but at last, on great relaxation of the ligaments, notes cease to be produced. If the thyroid cartilage, on the other hand, be drawn forward, the deep bass notes of about one octave may be produced in a good male larynx; but after this the notes pass into a falsetto sound, which is alone generally produced after a certain tension of the ligaments. There are two means, however, by which other and higher chest notes may be produced, viz. either by increasing the force of blowing, by which they may be raised to a fifth, though then they have generally a very shrill harsh sound; or by approximating the sides of the walls of the larynx immediately below the lower vocal ligaments, by a pair of flat instruments, as the handles of scalpels, pressed on the exterior of the exposed elastic membrane which is found at this part, lying upon and within a tolerably thick muscular layer, the lower part of the thyro-arytenoideus. This narrowing of the part of the larynx immediately below the vocal ligaments would seem to be the office of this portion of the muscle, which is also probably of further importance in the parts of it which cover the sides of the vocal ligaments and the ventricle, to which it may act as a damper when they are vibrating with the ligaments, and so produce an elevation of the tone. And again, another office of this thyro-arytenoid muscle will be to alter the vocal ligaments, by the influence its contractions will exercise on their outer borders, to which the muscles are firmly attached. When, for instance, they contract, a lax ligament, such as is necessary for the low notes, may become somewhat stiffer, as in the action of the sphincter oris

on the tension of the lips in trumpet playing.

The action of this muscle may also be imitated by pressing on the sides of the thyroid cartilage and chest: notes may thus be obtained as high as generally attained by the male voice. For example, in the greatest relaxation of the vocal ligaments, by approximating the thyroid to the fixed arytenoid cartilages, a larynx gave the chest note *c*; with less relaxation and stronger blowing, it gave *c'*, but this octave was the highest note attainable by this means. But by compressing the vocal ligaments and the parts below them, from the sides, higher chest notes could be obtained, and even up to *c''*, another octave, and up to this the falsetto notes were perfectly avoided.

From these observations, the following explanation of the chest-notes may be given. The ligaments vibrate in their whole breadth, and the membranes and thyro-arytenoid muscles connected with them, vibrate coincidently. The deepest chest tones are produced during great relaxation of the ligaments, by the approximation of the angle of the thyroid cartilage to the arytenoid: in this great relaxation the ligaments are not only quite unstretched, but are, when at rest, even wrinkled and folded; but the air passing through stretches them, and gives them the tension necessary for vibration. By increasing the tension of the ligaments, the chest-notes may be raised one octave. The most easily produced are those at about the middle of this scale of one octave, which are about the pitch of the natural speaking voice, and are obtained when the ligaments are neither stretched nor wrinkled. The chest-tones are raised to their greatest height by the lateral compression of the vocal ligaments, and the narrowing of the inferior passage to the glottis by the thyro-arytenoid muscles, or, as before observed, by more violent blowing. In the chest-tones, too, the tension of the lips of the glottis by the thyro-arytenoid muscles is to be considered.

In the falsetto notes, the inner portion, or merely the edge of the ligaments, vibrates; they depend for their height on the degree of tension of the ligaments.

None of the parts lying above the lower vocal ligaments are necessary for the production of either the chest or falsetto notes.

The notes easily produced on the female larynx are in general higher than those obtained from the male. The vocal ligaments in the female are generally shorter than those of the male; and from this circumstance (with in a slighter degree the

size of the whole larynx and the strength of its walls) may be deduced the different characters of the male and female voices, as well as the differences in each sex—as

the bass and tenor, the alto and soprano. Müller has measured the length of the ligaments in several cases, and the general result is as follows:—

Average length of vocal ligaments in males, at their greatest extension, 23½ millim.

Ditto	ditto	females	ditto	15½	„
Ditto	ditto	males, while at rest		18½	„
Ditto	ditto	females, ditto		12½	„

Hence, in both conditions, the lengths are in the proportion of 3:2.

These measurements are taken from the attachment of the ligament at the angle of the thyroid cartilage to its attachment to the anterior process of the arytenoid, and the small continuation of the ligament which goes off somewhat further backwards towards the anterior edge of the arytenoid cartilage, is included. The extension in the first measurements is made by drawing the thyroid cartilage forward, while the arytenoid are fixed.

By increasing the strength of the current of air during the same degree of tension of the ligaments, the original note may be raised through a series of half-notes to its 5th, or even higher. Thus if it commence at *g*, which is produced during the weakest blowing, it will, by increasing the violence of the current, go through *g* sharp, *a*, *a* sharp, *b*, *c*, *c* sharp. In this respect the human larynx agrees with the artificial ones made with the moist elastic coat of an artery; but the same effect cannot be produced by the artificial caoutchouc larynges. Hence it seems that there are two distinct modes by which, in the human larynx, the same note may be produced, viz. either by a certain degree of tension of the ligaments proper to it, or by an increased current of air, when the ligaments have the degree of tension proper to another note less than an octave below it. But the character of the note is very different in the two cases; in the former it is full and round, in the latter harsh and squeaking.

The alteration of the length of the tube below and above the ligaments produces very little alteration in the notes,—in no case more than a single note, and generally not more than half a note; yet in the artificial larynges, made either with laminae of caoutchouc or of arterial membrane, the difference is generally considerable, probably from the different nature of the walls of the tubes above and below, which, in the human larynx, are only in certain cases fitted for vibrations, and the greater extent to which the artificial laminae may vibrate. The only influence that the varying length of these tubes would seem to have, is that which may be observed in living persons, viz. that the

elongation of the upper tube by the descent of the larynx, renders the production of the low notes more easy, and that its shortening by the ascent of the larynx facilitates the production of the high notes.

The partly membranous nature of the trachea does not seem to have any influence in modifying the note, which is the same when it is used as when a modern wind-pipe is employed; and the double termination of the upper tube in the mouth and nose seems to be of equally small importance in altering the height of the note, though it somewhat alters its expression by its resonance.

The covering of the glottis by the descent of the epiglottis lowers the note somewhat, and at the same time makes it somewhat duller, otherwise it appears to be of no importance in the modification of the notes; for Müller's experiments do not confirm the ideas of Grenie, Biot, and Magendie, that it serves to raise the note when it is lowered over the glottis, while the current of air is made stronger.

The arches of the palate are narrowed, and the uvula is shortened in the higher chest notes, as well as in the falsetto; and in the same high notes the isthmus faucium is narrowed, whether for chest notes or falsetto. And in either case the arches of the palate may be touched with the finger without the note being altered.

A narrowing of the parts immediately above the local ligaments has no material influence on the note produced, and the only use of the Morgagnian ventricle is probably that of leaving the ligaments free to vibrate, just as, according to the comparison of Malgaigne, the hollow in the mouth-piece of a trumpet permits the free vibrations of the lips.

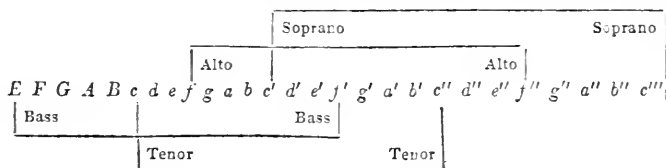
We pass over the admirable examination of the opinions of other authors which follows the detail of these experiments, which are too convincing to leave any doubt of the correctness of Müller's general conclusion, that the larynx may be compared to a musical apparatus in which the notes are produced by the vibrations of two membranous tongues; but to any who are desirous of being acquainted with the literature and history of the subject, this section presents

itself nearly all that can be required, and gives sufficient references to sources for still further information. We pass on to the section on *Singing*.

He observes, that there are three kinds of sequence in which the notes of the human voice may follow each other. The first is the monotonous, as in common speaking, where the principal differences are produced by the articulation in the mouth, and the notes are only made occasionally higher or lower for the sake of accent and emphasis. The second kind is that where the notes rise or fall in a successive passage from one to another without appreciable intervals, as in the howling and whining both of men and animals. It is imitated by the wind, by a string, which, having been put into vibrations, is gradually drawn tighter while sounding,—or by the increasing violence of the current of air blown into some pipes, or on membranous tongues, or on the vocal ligaments. The third kind is the musical, in which each sound has its proper number of vibrations, and the following notes have certain definite proportions of their number of

vibrations, or are marked by definite intervals according to the musical system of notes. It has rhythm also in common with poetry.

1st. *Of the Compass of the Voice*.—The compass of the voice is from 1 to 2 or 3 octaves,—that of singers is generally from 2 to 3. But the voices of males and females begin and end at different parts of the scale. Taking as *C* the great *C* of the 8-feet open or 4-feet covered organ pipe, the male voice would begin at *E* or *A*, or *c*, and would go up to *a* or further, or to *f'*, or to *c''*. The female voice is as deep as this only in viragos. The voices of women, children, and eunuchs, begin between *f* and *c'*, and reach to *f''*, or *a''*, or *c'''*, or at the very highest to *f'''*. Thus the lowest note of the female voice is about an octave higher than the lowest of the male, and her highest about an octave above his highest: so that the whole compass of the human voice is about 4 octaves, viz. from *E* to *e'''*, taking as *C* the 8-feet open *C* organ pipe, and as *c'''* the 6-inch open organ pipe. This is better explicable by the following diagram:—



The elder Fischer could sing as low as *F*; the youngest of the sisters, Sessi, had a compass of 3 octaves and 3 notes, viz. from *c* to *f'''*; Zetter had one of 3 octaves, and Catalani of 3½.

But the different range of the voice is not the only thing that distinguishes the male from the female, or, in the same sex, the alto from the soprano, or the bass from the tenor: there is a peculiarity in the tone of each,—such, that although their compass might be the same, or though both singing the same note, the one might be distinguished from the other; and on the same differences, in a slighter degree, depend the less evidently distinguished kinds of voice, called barytone, mezzo-soprano, counter-tenor, contralto, &c.

The difference between the pitch of the male and female voices depends principally, as before observed, on the different lengths of the vocal ligaments, which have the proportion of 3 to 2. The difference in their peculiar tone depends on the form and quality of the resounding walls, which in the male are much larger, and form a greater angle at the anterior part of the thyroid cartilage. The difference of tone between the tenor and bass, and the alto

and soprano, depends probably on some peculiarities, at present undiscovered, of the ligaments and resounding walls,—as in various instruments peculiarities of tone are produced by different materials, as metallic or catgut strings—metallic, wooden, or membranous tongues—or metallic, wooden, or paper resounding walls; for these instruments, though set at the same note, have each a different and peculiar tone or timbre.

Boys' voices are altos or sopranos, like women's; and after the further development of the larynx at puberty (before which it presented the same form as that of females) become basses or tenors. During the development of the larynx, the voice is peculiarly harsh and uncertain, and hence at this period is generally said to be *breaking*. In eunuchs deprived of virility before puberty, the voice is not altered, and they retain the female tone throughout life.

Most persons, and especially males, have the power of modifying their voice according to a double set of notes, viz. that of the chest notes, and that of the falsetto. The chest notes are fuller and rounder, and the lowest notes of the voice are heard only in them; the falsetto are more shrill,

and contain the highest notes of the voice, while the middle notes can be produced in either scale. The two scales are therefore not exactly limited by one another, but in the middle notes of the voice run into each other. A tenor, for instance, begins to pass into falsetto at *a'*, while the notes below this can be sung with either kind of voice: the bass, of course, begins falsetto earlier. In women there is seldom a sufficiently evident difference between the chest and falsetto notes.

The peculiar slight differences distinguishing the voice of each person also probably depend on some slight peculiarity in the form of the air-passages and membranes and their resonance; and hence, by alterations of the form of their own organ, many persons can imitate the voices of the most different individuals. The nasal voice or twang may be produced in two ways: with the nostrils closed externally, both it and the natural voice may be produced; the latter when the arches of the palate are open, the former when they are approximated,—in which case the larynx is much more raised than when the same note is given in the natural voice. In this case the nasal cavity becomes a separate resounding chamber. Or secondly, the nasal voice may be produced with both nose and mouth open externally, by raising the larynx, approximating the arches of the palate, and moving the back of the tongue towards the palate, so as even to make them touch; the air then goes only through the narrowed palatine arches, and receives the resonance of the nasal cavities with that of the cavity of the mouth.

The voice of old people loses in tone, in certainty, and in compass; the first by the ossification of the cartilages, and the changes in the condition of the ligaments; the second by the partial removal of the command of the nerves over the muscles, from which here, as in the other parts of the body, a trembling motion is produced; while, from both these causes, the voice becomes weaker, and shaking.

The power of the voice depends on the capacity for vibrations of the vocal ligaments, and partly on the fitness for resonance of the membranes and cartilages of the larynx, thoracic walls, lungs, oral and nasal cavities, and the cavities adjacent to the nose. Hence much of it is lost in inflammation of those parts producing a copious secretion of mucus, or other changes. The variations in loudness with which the same note is sung by the same voice, cannot depend merely on the varied energy with which the air is forced through the glottis, because, as was before shewn, a mere increased force of the current of air, while the vocal ligaments retain the

same length, has the effect of raising the note even to its fifth. It is probable that this elevation of the note which would be produced merely by an increased force of the air, is compensated by a corresponding relaxation of the ligaments; so that, for instance, in proceeding from a piano expression to a forte, the ligaments are relaxed just in proportion as the force of the current of air is increased, and the opposite in proceeding from forte to piano. Another means of compensation may be by the narrowing or widening of the portion of the air-passages immediately below the vocal ligaments. It has been shewn that an elevation of the note is produced by the contraction of this part, and hence it may be, that as the current of air is increased in force, this part of the tube is widened, and *vice versa*.

Man, like a singing bird, learns unconsciously the internal alterations of the larynx, and the muscular actions necessary for each note. Accidentally uttered notes, and the muscular actions producing them, become associated, and in time are made to follow each other in order when a melody is to be imitated. In methodical learning of singing, a knowledge of the signs indicating the notes is added to the association of the notes heard, and the muscular motions necessary to produce them. For all this, and to give to each note a harmonious expression, a good ear is necessary, for without it there may be a voice of extensive compass and great beauty, but there can be no good application of it, or singing.

One remark must be made on the beautiful arrangement of the apparatus of the human voice. No musical instrument can be compared with it, for even the most full organs and pianos are in some respects incomplete. Some of these instruments are incapable of passing from piano to forte, as in the labial pipes; others cannot rest long on the same note, as all those which are sounded by striking. The organ possesses two scales, from the labial and the tongue-pipes, and is in this respect comparable with the human voice, with its chest and falsetto notes; but none of these instruments unites all advantages like the human vocal organ. It belongs to the class of those that have tongues; and these, when united with a system of compensating pipes, are (next to the violin) the most complete of all. Yet again, the vocal organ has this perfection, that from one tongue-pipe the compass of the whole gamut, and all the required variations, may be produced, while in the most complete artificial apparatus each note must have its own pipe. An artificial imitation of this organ would be in some measure attained, by the arrangement of

a tongue-pipe with an apparatus easily manipulated to produce the required degrees of tension of the elastic bands; but the tone of such an instrument, for which only dry elastic bands could be used, would not be able to imitate the soft full tone of the moist animal elastic tissue, and there always would be a great difficulty in manipulating it.

DISSECTION OF AN OLD DISLOCATION OF THE THUMB,

WITH REMARKS AND EXPERIMENTS,

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I AM indebted to Dr. Hunter, Andersonian Professor of Anatomy, for permission to examine and make public the following case:—A female subject was brought into Dr. Hunter's dissecting rooms during the course of last winter, with an unreduced dislocation of the phalanx of the right thumb, on the back part of the metacarpal bone. It was ascertained on inquiry, that the dislocation had existed for three years, and that several unsuccessful attempts had been made to reduce it. The motions had been partially restored and performed without pain. Previous to dissection, the phalanx was seen to be thrown back, with an anterior and posterior prominence. The thumb shortened, the second phalanx not bent on the first, and the articulation between them capable of free flexion and extension. There being no swelling, the nature of the accident was obvious.

Dissection.—*Bones*: the end of the phalanx was thrown on the back and inner part of the metacarpal bone, to the distance of at least an inch. The circumstance of the phalanx being placed on the inner side of the back part of the metacarpal bone is worthy of notice, as I believe that it is almost uniformly so situated in this dislocation. The end of the metacarpal bone projected forwards to a distance corresponding with the displacement backwards, free from muscle or ligament.

Ligaments.—The anterior ligament torn from metacarpal bone; the posterior pressed back, but apparently nearly entire. The anterior portion of external lateral torn; the posterior por-

tion stretched, thrown back and across the metacarpal bone; the internal entire. New ligamentous connexions had formed between the displaced bones.

Muscles.—Extensors thrown back, and somewhat stretched over the end of the phalanx; of these, however, I cannot speak with certainty, as they were cut before I examined the preparation minutely. Abductor thrown back, and a little stretched. Opponens a little changed. Flexor brevis: on this muscle, and the flexor longus, the alteration of position was most remarkable. Dr. Hunter thought that the brevis was not torn, but that the end of the metacarpal bone had passed between its two portions, one of which was on each side of it, grasping it firmly. Further examination induces me to think that the greater part of the outer head was torn, the end of the metacarpal bone having passed through its fibres, and that the inner head was uninjured, having slipped to the inner side of the metacarpal bone. The tendon of the flexor longus lay on the inner side of the metacarpal bone, along with the inner head of the brevis, pressing on the abductor, and pushing it back, which last muscle, with this exception, was unchanged.

Nerves.—The first, or external digital nerve, is thrown to the inside of the metacarpal bone, and lies imbedded between the phalanx and metacarpal bone, at the points where the former rests on the latter.

Sesamoid bones were not connected with the metacarpal bone. It was found that it would injure the preparation if the dissections were carried so far as to ascertain if they were placed between the metacarpal and the phalanx, in which situation I have, however, no doubt that they are.

Although there are several points in the above dissection which were new to me, they had rather the effect of unsettling my previous opinion as to the cause of difficulty in reducing this dislocation, than of satisfying me as to what that cause really is. In the hope of farther elucidating this point, I proceeded to make experiments on the dead body. To detail all these experiments and dissections (and I have made a great many), in the order in which they were made, would be tedious and unnecessary. I prefer arranging them into three classes, according to the

amount of dislocation produced, and giving one dissection as an example of each class. The experiments consisted in dislocating the articulations, by grasping the phalanges of the thumb and bending them powerfully backwards; and the dissections, in dissecting the parts, and removing one layer of muscular fibres and of ligaments after another, so as to ascertain the true cause of the difficulty of reduction.

I. Partial dislocation.—The external characters are—anterior and posterior prominences slight; the posterior projects directly backwards. Phalangeal articulation bent like the dog-head of a gun, and cannot be extended; disfigurement considerable.

Dissection.—No muscular fibres torn; abductor and both heads of flexor brevis somewhat stretched and thrown a little back; tendon of flexor longus considerably stretched, but not displaced, retaining its natural position on the forepart of the articulation between the two heads of the brevis.

Ligaments.—Anterior partially torn; posterior entire, stretched; external lateral has a few of its anterior filaments torn, its posterior stretched; internal lateral entire, a little thrown back. Articular extremities of bones partially displaced; that of phalanx backwards, and very slightly inwards. Reduction was easily effected; all that was necessary was to extend the articulation between the phalanges; the flexor longus tendon, when brought into a straight line, drew the bones into their place.

REMARKS.—I have never seen this dislocation on the living subject. It probably seldom requires surgical treatment, being reduced by the action of the flexor longus or by pressure made by the patient over the articulation. The muscles are too little displaced to oppose much resistance to the reduction.

II. Complete dislocation.—External characters: anterior and posterior prominences well marked; the latter a little to inside. Articulation between phalanges bent, but can easily be extended.

Dissection.—Abductor and adductor nearly as in last experiment. Inner half of external head of flexor brevis torn by the end of the metacarpal bone; remaining part lies on the outside of the

bone, and thrown back. Internal head lies to the inside of the bone, and somewhat backwards. Tendon of flexor longus thrown from off forepart of articulation to inside of metacarpal bone, and carried considerably backwards, pressing on inner head of brevis and adductor; that part of its sheath connecting it with the articulation, and the end of the metacarpal bone, torn; that connecting it with phalanges entire. Anterior ligament torn, excepting at inner side; posterior entire, stretched; internal lateral entire; anterior part of external lateral slightly torn; posterior part stretched, thrown back, and a little across end of metacarpal bone. Displacement of phalanx backwards does not exceed half an inch. Sesamoid bones remain attached to phalanx. Reduction on dead subject easily effected.

REMARKS.—I should not anticipate great difficulty in reducing this dislocation on the living. The causes opposing reduction I conceive to be the following:—

1st. *Muscular contractions.*—The muscles least affected and most easily overcome, are the extensors, abductor, and adductor. The flexor brevis and longus are less easily dealt with; we have seen the metacarpal bone driven through the external head of the brevis, and so situated as to be firmly grasped between its remaining fibres and the internal head, the tendon of the longus on the inside of the articulation dragging the phalanges inwards, and locking them in contact with the metacarpal bone. These, though rather formidable obstacles, are within the control of bleeding, antimonials, and the pulleys.

2d. *Ligaments.*—My experiments lead me to conclude that the lateral ligaments have been reckoned of greatly too much importance in this dislocation. If they are the causes of difficulty, the reduction ought to be nearly as difficult on the dead as on the living, which in this degree of dislocation is by no means the case. It is quite a mistake to suppose that the end of the metacarpal bone is grasped by the lateral ligaments, and cannot escape. The internal lateral ligament is little, if at all, implicated in the matter; the external may result in this way. The phalanx being thrown inwards, the external lateral ligament is dragged with it, and thrown a little across the back of the metacarpal bone,

and in attempts to reduce the dislocation, catches on the prominence, on the outer and back part of the same bone. If the phalanx is pulled forwards and bent across the palm of the hand, the resistance will probably be considerable, but never such as to require or justify the operation of cutting the ligament. To overcome the difficulty, use the pulleys, press the bones asunder by pushing the metacarpal bone towards the palm, the phalanx towards the dorsum, extend the thumb towards the points of the fingers, not across the palm; next press the metacarpal bone backwards and inwards, the phalanx forwards and outwards; lastly, smartly bend the joint.

3d. The third cause of resistance is the locking of the prominences on the back part of the metacarpal bone, and the fore part of the phalanx. The manipulations recommended ought to overcome this difficulty.

III. This dislocation is also complete, and differs from the last in the following respects:—The displacement is greater, amounting at least to an inch. The end of the metacarpal bone is driven completely through the inner fibres of the external head of the flexor brevis. The anterior ligament is completely torn from the metacarpal bone, and remains attached to the phalanx and sesamoid bones, in such a manner that the torn ligament and sesamoid bones are carried backwards by the phalanx, and placed between it and the metacarpal bone. This state of parts is aggravated and rendered permanent by the contraction of the muscles attached to the sesamoid bones and anterior ligament, which muscles, together with the tendon of the long flexor, only differ from their position as described in last section, in being carried farther back. The result is, that the opening in the ligaments by which the metacarpal bone escaped, is thrown back nearly half an inch, and the remains of that ligament and sesamoid bones form a partition between the displaced ends of the bones, which form a mechanical obstacle to the reduction of the dislocation, in some instances I fear insurmountable. This, I am satisfied, is the true cause of the difficulty experienced in replacing the bones in some cases, and of total failure in others. The position of the digital nerve, in Dr. Hunter's preparation,

proves that besides the sesamoid bones and ligament, whatever parts are more firmly connected with the phalanx than with the metacarpal bone, will follow the former backwards, and be wedged between it and the latter. It must further be kept in mind, that, in addition to this mechanical obstacle, we have all the causes of resistance mentioned in the last section.

How are these difficulties to be overcome? I fear, as already hinted, that in many cases the reduction is impossible, without the infliction of an unwarrantable extent of injury on the thumb. I have not yet, in practice, met with an "impossible" case; but whenever the displacement is great,—and I believe the amount of mechanical obstruction will generally correspond with the extent of displacement,—I would recommend the following:—Take a door-key, the open part of the handle of which will rather exceed the length of the first phalanx; put the thumb through it, apply the pulleys, give tartar emetic, and bleed to faintness; extend the pulleys (in the direction recommended in last section) as far as can be safely done; employ the manipulations already described, assisting the flexion with the key, so used that the curve which lies on the dorsum will press on the nail end of the first phalanx, and the opposite curve on the end of the metacarpal bone. This failing, reverse the key, carry the under curve towards the point of the thumb, place the posterior on the displaced end of the phalanx, and throw the parts into forced extension, relaxing at the same moment the pulleys.

I disapprove of cutting; but if a knife is used, employ one which is narrow but strong; pass it into the joint and between the bones, in such a manner as (if possible) to disengage the anterior ligament and sesamoid bones from their unnatural position. This attempt should be made while the pulleys are in operation. As the external lateral ligament is not the cause of the mechanical obstruction, it is cruel to cut it: moreover, it is useless. I have cut it on the dead subject, and failed to replace the bones. Indeed, I believe that those cases in which the external lateral ligament is most extensively torn, are the most difficult to reduce, because in them the displacement is greatest; and in proportion to the extent of displace-

ment the mechanical obstacle is increased.

Glasgow, August 1837.

P.S.—Since writing the above I have had the great pleasure of being introduced to Sir Astley Cooper, during his short visit to Glasgow. I took the opportunity of asking him what he considered the cause of the difficulty of reducing the dislocation. He at once replied—"The sesamoid bones." I asked if he had verified this by dissection or experiments. To which Sir Astley answered—"In the great toe, but not in the thumb; but I am satisfied that the causes are the same in both." I then mentioned to Sir Astley what I had done on this subject; and I have great satisfaction in being allowed to give his very high authority in confirmation of the opinions given in this paper.

Sept. 26, 1837.

NITRATE OF SILVER IN SOME CUTANEOUS DISEASES.

To the Editor of the Medical Gazette.

SIR,

SINCE the publication of Mr. Higginbottom's Essay on the Use of Nitrate of Silver, cases have appeared, from time to time, in the MEDICAL GAZETTE, and other periodicals, bearing testimony to its value, applied in the manner or to the class of affections in which it is recommended by him. If the following observations on its local use in certain cutaneous diseases possess sufficient interest to procure their admission into the pages of the GAZETTE, they are at your disposal.—I am, sir,

Yours obediently,

HENRY T. CHAPMAN.

2, Argyll-Street.

Having been engaged, whilst house-surgeon to St. Bartholomew's Hospital in 1830, in testing the efficacy of the nitrate of silver in external inflammations, according to the method employed by Mr. Higginbottom, I was led to extend my experiments from erysipelas and inflammation of the absorbents first to cases of erythema nodosum, and afterwards to squamous affections of the skin. Before I proceed further, it may

not be amiss, perhaps, to remind your readers of Mr. Higginbottom's mode of applying it. After cleansing the skin with soap and water, and drying it, the surface to be submitted to its operation is again moistened, and a solid stick of the lunar caustic is rubbed lightly over it once or twice, according to the delicacy of the skin, and allowed to dry. No dressing is required, and the part must be kept cool. The slight vesication produced after the first twenty-four hours soon subsides, and about the fifth day the black pellicle loosens and peels off.

The following are the results obtained by me. Of erythema nodosum two cases were treated; in one of them the redness and induration disappeared entirely after a single application of the caustic; in the other it was necessary to repeat it once more. My next experiment was made on squamous affections of the skin; lepra, namely, and several varieties of psoriasis*. I possess the details of more than a dozen cases successfully treated by it, and have since employed it with equal success in a great number of instances, of which I have neglected to take notes. I select two of those by me as examples of its action.

Thomas Henshaw applied Oct. 8th, 1829, as an out-patient at St. Bartholomew's Hospital, with an eruption of psoriasis diffusa upon both arms. He has been teased with it about eighteen months. His general health has been always good.

November 23d.—During his attendance up to this time a variety of means have been tried; antimonials, mercurial preparations, and mineral acids, internally; locally, sulphuret of potass, as a lotion, and the diluted nitrated mercurial ointment; all without the slightest effect on the disorder. The worst part of the scaly surface was, therefore, rubbed over with nitrate of silver.

30th.—The cuticle having desquamated from the part acted on, has left the skin in a tolerably healthy state; the situation of several of the worst patches being merely indicated by a little roughness. A fresh portion was treated in the same manner, and Plum-

* Dr. Graves, of Dublin, expresses an opinion unfavourable to the use of the nitrate of silver in psoriasis. See the *Medical and Surgical Journal*, March 1835.

mer's pill directed to be taken in five-grain doses every night.

Dec. 7th.—The second application has been as effectual as the first. No re-appearance of the disease over that part of the skin to which it was first applied. The whole remaining surface treated by the nitrate.

14th.—On examining the arms to-day, a roughness only of the skin, where the disease existed, is to be observed. No fresh scales have made their appearance. The pills to be continued.

Jan. 4th.—Came to show his arms, which are now quite free from eruption.

Mary Ghost, æt. 17, attended first as an out-patient, in August 1829, with *lepra vulgaris* over the arms, legs, and body. It commenced by circular patches on the body, more than twelve months back, and has since extended over the limbs.

The liquor arsenicalis, hydrarg. oxy-murias, pilul. hydrarg., submur. comp., warm baths, and other measures, have been employed up to this time, Dec. 14th, not only without improvement, but without arresting its further progress, and I determined to try the nitrate of silver, as in Henshaw's case.

Jan. 11th.—Applied it to the right arm, and on the 15th to the left.

19th.—Where the cuticle has peeled off, the scales have disappeared, leaving a slight roughness here and there; to these spots it was re-applied.

Feb. 1st.—The arms are perfectly cleared of the eruption. The back, and afterwards the legs, were treated in the same manner.

March 26th.—The *lepra* has wholly disappeared. As a precaution, I made an issue in the arm.

May 24th.—She applied for some aperient pill; no return of the eruption. Health better than it has been for some time.

Neither in the above cases, nor in any others, has the treatment been solely and exclusively local. Alteratives were given, and other general means always adopted preparatory to, and continued during, the employment of the nitrate of silver; but it is fair to attribute the immediate amendment to the local remedy, since the same measures had been persevered in fruitlessly, for many weeks in some instances. In *psoriasis palmaris* I find it necessary to soak the hand in warm water

for some time before the nitrate can act with effect through the horny, ragged, and everted flakes of cuticle. The surface, also, bears rubbing over more freely, and three or more series of applications are required to restore the skin to its natural condition. It is very true that relapses occur sometimes; but the repetition of the process will again remove the disease, and the intervals between its recurrence, when it proves obstinate, are generally considerable.

Besides the squamous disorders above mentioned, I have made trial of the nitrate of silver upon other cutaneous diseases.

In *porrigo*, a strong solution of it is recommended by some practitioners almost as a specific. It was, I believe, a favourite remedy with Mr. Wilkinson, who acquired some celebrity for his treatment of this troublesome malady. I have tried it repeatedly both in solution and in the solid state, but in neither form was any permanent advantage gained by it.

In *sychosis menti* and *eczema*, in both of which disorders, as well as in *herpes zoster*, its efficacy has been rated highly. I have been equally disappointed. In short, as far as my experience of its beneficial effects in diseases of the skin extend, it exerts a decided influence over those of a squamous character alone.

With regard to the management and curative powers of the nitrate of silver in *erysipelas*, inflammation of the absorbents, and other superficial inflammations, I have a few remarks to make.

Erratic *erysipelas* I have sometimes seen confined within the belt of blackened cuticle produced by it; but in severe cases it more frequently steals beyond the margin, and spreads as before. Nor has the strong solution, as recommended by Dr. Elliotson, been more efficacious.

In inflammation of the absorbents arising from punctures during dissection, &c. the nitrate, resorted to early, almost always proves completely successful; but when the case has reached the verge of suppuration, or after that has actually occurred, its operation may be highly injurious. The symptoms become masked, and fluctuation difficult of detection; for, instead of making its way to the nearest surface, the pus appears to shrink (if I may so express myself) from the coating of ni-

trate of silver, and spread to a more remote point in search of a vent; producing, in some instances, serious mischief by its separation of the tissues. The following case will exemplify this:—

—Dunn, a man about 24, was admitted into St. Bartholomew's Hospital in the beginning of April, 1830, with inflammation of the absorbents along the inside of the knee and thigh, proceeding from a cut across the front of the leg, received a few days previously. The nitrate was applied, with speedy relief to the pain and heat, and in four or five days the cuticle peeled off, restoring to the skin its natural appearance. The next day a slight blush was noticed on the inside of the thigh, accompanied by trifling fulness; but the most careful examination failed in discovering fluctuation. The caustic was once more resorted to; but, before the period of the separation of the cuticle had arrived, a general fulness of the inside of the thigh became manifest. A poultice was immediately directed, and on the removal of the black coating, the presence of matter, deeply seated, was distinctly ascertained.

April 6th.—The abscess was opened, and more than half a pint of pus evacuated from a cavity dipping between the rectus and adductor muscles. This space filled, in the course of the succeeding twelve hours, with a coagulum of venous blood, which was renewed as fast as removed, distending the cavity to such a degree that the probe met with no obstruction to its passage near the inside of the neck of the femur, to within a short space of the back of the thigh; and the finger could reach, without difficulty, the soft parts which close the obturator foramen. A bandage and compresses were applied; a discharge of pus, free from blood, was soon established; his health, which had suffered much from sympathetic fever, improved daily, and the cavity gradually filled up and cicatrized. A stiffness of the hip-joint remained for some weeks after his discharge, which finally disappeared altogether.

In a second case treated by the nitrate, one of incipient sympathetic bubo, a considerable accumulation of matter formed insidiously beneath the blackened surface, and pointed behind the adductors, near the buttock.

Such results should teach us, therefore, to be cautious in resorting to this

remedy where suppuration is impending.

It still remains to consider the principle of action of the nitrate of silver, upon which Mr. Higginbottom does not advance an opinion. Its effects under certain circumstances, as that gentleman observes, are clearly something more than those of a mere escharotic. Indeed, destruction of surface can scarcely be said to follow its ordinary application to the mucous membranes of the eye and pharynx. By its stimulant or astringent property, or some peculiar power resident in it, the calibre of the enlarged vessels is reduced, and they appear to recover their tone. It may exercise an influence over the nerves of the part: at any rate, two of the characteristics of inflammation, pain and heat, always yield to it*.

Supposing similar effects to be produced on the capillaries and nerves of the skin, the influence of the nitrate on cutaneous and subcutaneous inflammations will not be difficult to explain; and, reviewing its operation in the foregoing cases, such a supposition appears by no means destitute of foundation. In the successful instances of its employment in inflammation of the absorbents, we are justified in concluding that it directly controls the increased vascularity, &c. of the lymphatic vessel, and subdues the inflammation at once. In the cases where, from its tardy application, or from some other cause, it did not prevent the formation of pus, it appears to have occasioned the burrowing of the matter between the muscles, by checking the operations of the capillaries, and transferring the evacuating process, which would otherwise have been conducted at the dermal side of the abscess, to another part of the sac.

In several cases of small collections of matter about the throat and neck, as soon as obstruction to its external escape has been offered by the presence of the black pellicle, I have observed that the pus has been gradually taken up, and entirely carried off, by the absorbents—a practical point of some value in the treatment of these abscesses in females.

In squamous disorders of the skin, besides the separation of the loaded cuticle, the nitrate appears to correct the diseased action of the capillaries, on

* I have lately heard the term "alterant action" applied to its effects on inflammation of the conjunctiva, by Dr. Kerst, of Utrecht.

which the accumulation depends, and restore a healthy tone to the vessels.

This result, however, can never be expected, while the original source of the disease still exists in the system; and before the local remedy can produce any permanent benefit, constitutional measures must be steadily enforced. Some may exclaim that the cure, in that case, is unjustly ascribed to the nitrate; but is it not highly probable that the diseased product still continues to be formed *habitually*, long after the state of health giving rise to such an excretion has been completely rectified? In these affections, therefore, the nitrate of silver is only to be looked upon as an auxiliary to other remedial means, which must always occupy the foremost place.

NITRATE OF SILVER IN THE GONORRHŒA OF FEMALES.

To the Editor of the Medical Gazette.

SIR,

HAVING observed, in a late number of the MEDICAL GAZETTE, a communication from Dr. James McCune Smith, regarding the use of the solid nitrate of silver in gonorrhœa of females, I have been induced to submit to you the following observations regarding that most valuable remedy, which I have made since 1st July last, whilst officiating as clerk to Dr. Hannay, in the Lock Hospital here.

I have seen the solid nitrate of silver employed in now upwards of forty cases of gonorrhœa, and shall make a few remarks on each of the effects produced in their order.

Pain.—When the caustic-holder is well lubricated with any simple ointment, its surface being free from all irregularities that may tend to injure the walls of the vagina—when no rudeness is used in its application, and, above all, when no ulceration of the parts to which it is applied exists—the pain is very trifling indeed; in many instances none whatever. Often have I seen amazement depicted in the countenances of the by-standers, on witnessing the placid and careless appearance of the patient after the operation. As a proof that the patients themselves think

lightly of the pain, I may say that in not a few instances have I been strongly urged by themselves to apply it a second time, even after they were completely recovered, and generally on the day they received their dismissal. Their reasons for this singular request I could not for a long time obtain, until one, more forward than the rest, told me, “that as one application cured them so quickly, a second would perhaps make them proof against a future attack of the disease.”

In the first cases in which I saw the nitrate employed, the patients evinced symptoms of great fear and seeming pain; these symptoms, however, were found to be equally severe when, instead of the caustic, a common silver catheter was introduced. After considerable investigation made by Dr. Hannay into the origin of this fear, sufficiently obvious reasons were made apparent. An example of these is given in the following report, taken from the journal, page 230.

“M. F. July 4.—Will not submit to treatment from fear. She says Dr. McC. Smith frequently told her of the ‘*terrible roastings*’ that Dr. Hannay would give her when he came on. She is therefore so terrified, that she would rather leave the house than submit to treatment, though told that nitrate of silver was not necessary in her case.”

This terrifying system, according to the testimony of several of the patients, had also been exercised by Dr. S.’s successor in office. It would be needless, on my part, to make any comment on such conduct as this; it of itself speaks volumes.

Irritation.—Many individuals, I am aware, consider pain as a concomitant to irritation, and, I believe, it generally is so; but experience teaches me that it is not always the case. Patients whom I have interrogated every hour, for hours after the application of the nitrate, as to whether they suffered much pain, have generally answered me, “No, not much; not worth the speaking about;” and the like,—yet on the following day displayed some irritation. In one instance, owing to prolapsus vaginae, half of the caustic cylinder (about a drachm) was broken in the vagina. Knowing, however, that no evil consequences followed similar accidents at a previous time, it was allowed to remain.

Next day there was very considerable irritation of the external parts; but even in this case the pain was comparatively slight.

In those cases where the irritation is any way severe, the patient is ordered to inject tepid water, which always allays it in two or three days.

Efficacy in suppressing the discharge.—It is completely established in my mind, from observation, that all cases of uncomplicated gonorrhœa in the female may be cured by the solid nitrate of silver in from *three to ten* days. Strangers visiting the Lock, after witnessing the examination of from 25 to 30 patients, have often asked me, with astonishment, whether we had no case of gonorrhœa at all in the house? The answer generally was—not a single case. It is seldom, indeed, that we have an opportunity of exhibiting a case of gonorrhœa to a stranger, unless it happens that a new patient labouring under it is admitted at the time.

Relapses.—The number of relapses that required a second application of the nitrate, amounts to about eight or ten per cent. There is, however, every reason to believe that the manipulation, in these cases, was improperly done, as since the latter end of July no instance of relapse has yet occurred, I having seemingly, from experience, become more perfect in the mode of its application.

In conclusion, I would remark that Dr. Hannay certainly deserves the highest meed of praise for his introducing a remedy so efficacious, and at the same time so economical: efficacious, from its producing a cure in about as many days as it formerly occupied weeks—and economical, from the short space of time the patients are kept in the hospital, independent of the small cost of the remedy. And I can now confidently assert, from fifty cases in my own experience, that it is devoid of the shadow of an evil.

You will confer on me a particular favour by giving the above observations a place in an early number of your most valuable Gazette.

I am, sir,

Your most obedient servant,
JAMES CROSS.

Glasgow,
30th September, 1837.

DR. GRAVES ON THE TREATMENT OF CHOLERA.

To the Editor of the Medical Gazette.

SIR,

As spasmodic cholera seems likely to fix itself permanently in Europe, and has lost nothing of its original malignity, every practitioner is imperatively called on to communicate whatever he thinks may prove serviceable in its treatment. This disease prevailed epidemically to a fearful extent in Dublin in 1832 and 1834. In the former year my practice among cholera patients was very extensive, but by no means successful. In 1834, about the latter end of July, when the disease was raging most violently among the middling and better ranks of society, and at a time when I had very frequent opportunities of treating it, too often had I to regret the inefficacy of the means employed, and though aided by the advice of the most eminent members of the profession in Dublin, too often had I to witness the loss of cases, not apparently very dangerous when they began, and which seemed to leave full time for the employment of remedies, if such were known. Under these circumstances I lost an intimate and highly esteemed friend, Surgeon Ryan, of Camden-street. He was a young man, of vigorous health, and the late Mr. McNamara and I saw him many hours before any symptoms of collapse had set in; calomel and opium, blisters, frictions, sinapisms, stimulants, and all the most approved medicines, were diligently tried, but in vain; and from that moment I resolved to lay aside the mercurial treatment, which had so very often disappointed my expectations, although pushed to the greatest extent, and applied with the greatest activity and perseverance. During the preceding months of May and June, I had treated several cases of diarrhœa in fever with large doses of the acetate of lead, according to Dr. Bardsley's plan, and I had had frequent opportunities of admiring the efficacy of this salt in checking profuse alvine discharges. Just as Mr. Ryan died, and while my mind was filled with regret at our failure in his case, I was called by Dr. Percival Hunt to see a lady in Nassau-street, labouring under dysentery; I advised

the free use of acetate of lead, and with marked success. Immediately after, I saw a case of cholera still in the stage of premonitory diarrhœa, or rather just passing from the bowel complaint into the fully formed disease. I tried the acetate of lead boldly, and with the happiest success. Thus encouraged, I applied this new method of treatment in every case to which I was called, and I was employed both night and day in visiting cholera patients, and every hour gave me additional proofs of the efficacy of the remedy. My formula was as follows:—

R Acetatis Plumbi ℞j.; Opii, gr. j. m.
fiat secundum artem massa, in pilul.
xii. dividenda.

The premonitory diarrhœa has almost invariably stopped by taking one of these pills, at first every hour, and as the stools became less frequent, every third or sixth hour, according to circumstances. When the vomiting, spasms, and the state of collapse had begun, it was necessary to give a pill every quarter of an hour: after a couple of hours the effect of the pills became perceptible, in a diminution of the serious evacuations upwards and downwards; then the pills were given only every hour, and as the symptoms yielded they were given less and less frequently, and could in general be laid aside altogether before twenty-four hours. In some it was found necessary to give the acetate of lead in solution, combined with a little vinegar and minute doses of acetate of morphia. Minute doses of opium were useful; any thing of large doses hurtful. Mr. Parr, the able and respected apothecary of the Meath Hospital, was saved by acetate of lead, after the usual astringents, combined with large doses of opium, had been fully tried. He was found by me to be sensibly under the narcotic influence of opium, but the peculiar symptoms of cholera had not been thereby checked. Many took more than forty grains of the acetate of lead in twenty-four hours; it usually darkened, or even blackened, the alvine discharges, before they ceased altogether. Were I to enumerate all the cases of violent cholera that yielded to this treatment, I would be led into a tedious but not an uninteresting detail; I shall, therefore, merely refer to some of the most malignant cases, where the recovery of

the patient was undoubtedly owing to the bold exhibition of acetate of lead, and where the success of this practice was acknowledged by witnesses the most unexceptionable and competent.

Mr. Peile, Deputy Inspector General of Hospitals, and Staff-Surgeon Coleclough, will not readily forget the apparently hopeless case of an assistant-surgeon in the army, whose life was thus saved. Dr. Marsh was so struck with the effects of this medicine in the case of a young gentleman residing in the house of the Rev. Mr. Bermingham, Charlemount Mall, that he did not hesitate to my trying the same remedy in the case of Mr. Kerin, then President of the College of Surgeons, whose case seemed to be desperate. Mr. Maturin, son of the celebrated writer, was attacked in the most violent manner, and, indeed, neither I, Mr. McNamara, or Mr. King, had any hopes of his recovery; yet he too was saved. Nothing could be more appalling than the state of Mr. Wilson, of Charlemount Street. The effects of the acetate of lead in his case were so striking, that Mr. Mulock immediately adopted the plan of treatment, and he assures me that he has thereby saved a great number of very bad cases. Dr. Davis, surgeon of the 18th, or Royal Irish regiment of foot, witnessed with extreme interest the acetate of lead tried, and tried successfully, in a very bad case at the Richmond barrack. Equally successful results followed the trial of this remedy in the hands of other practitioners, both in Dublin and various towns in Ireland, during the epidemic of 1834.

After I found out the benefits resulting from the employment of acetate of lead, I no longer desponded when called to cases of cholera, knowing that in the great majority of instances the disease would yield. Of course there are cases of cholera which admit of no treatment, and which an experienced eye will at once recognize as fatal; they occur generally among the aged, or the very young, and are fatal in the course of a few hours, often without any premonitory diarrhœa. But this constitutes no valid objection to the practice; for in what disease do not cases occur which baffle all our efforts? Fever, scarlatina, pneumonia, croup, inflammation of the brain, of the bowels, and many other

affections, occasionally exhibit a degree of intensity which renders them as intractable, and as speedily destructive, as even the worst cases of cholera. But does this occasional intensity and occasional intractability, of certain cases, lead us to regard the diseases above enumerated as beyond the reach of medicine and the control of the physician? By no means; for although we feel our efforts in the particular cases specified to be unavailing, yet we also feel that where the intensity of these diseases is less, we can save numerous lives that would otherwise be lost; so it is, likewise, with cholera.

I may remark, that since 1834, cases of true Asiatic cholera occur sporadically every now and then in Dublin, as I believe happens also in most large towns in Europe once visited by this pestilential epidemic. Of these I have lately seen two decided cases; both were likewise visited by Mr. Mansfield. Both were saved; and yet one was so violent as to have reduced a powerful young man to a state apparently hopeless, in the course of three hours.

I cannot conclude without imploring the profession, in every part of the world where cholera prevails, to give my plan of treatment a fair trial, for I feel confident of its efficacy.

I have the honour to be, sir,

Your obedient servant,

ROBERT J. GRAVES, M.D.

Dublin, Oct. 9th, 1837.

MEDICAL GAZETTE.

Saturday, October 14, 1837.

"Licet omnibus, licet etiam mihi, dignitatem Artis Mediceæ tuæ; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

THE STARVING EXPERIMENTS.

THE Poor-Law Commissioners, in their experiments on the poor of England, appear to us very much to copy the conduct of Magendie, as described by one of our earlier correspondents. They affect, in their cold-blooded waggery, to be astonished at the writhing of their victims:—"What," they cry, "you

greedy sensualists! are you not satisfied with twelve ounces of bread daily?" Don't you know that Cornaro lived on less? "*Il n'entend pas le Français*," said Magendie, apostrophising an English dog that neglected his admonition not to howl as he was cutting him up alive. They don't know what a nice thing pork-water is, say our Guardians(?) of the poor, in allusion to the complaining subjects of their equally humane experiments.

We shall be thought by many of our readers, and perhaps justly, to belong to the sect of optimists, when we confess that we think it almost impossible for any proved and obvious abuse, in this country at least, to go on without some attempt at a remedy. This is saying, in other words, that we think it impossible that the inquiry into the working of the present Poor-Law, which was broken off by the prorogation of parliament, can remain where it was. The glimpse which it afforded of the real working of this cruel Act must excite every one's curiosity—to say nothing of better feelings—to have a full view of the case; and though Mr. Walter is now unfortunately removed from public life, it is to be hoped that a worthy successor will not be wanting. The oppressed have good reason to exclaim—

Exoriare aliquis vestris ex ossibus ultor!

Mr. Walter's case, it must be admitted, was quite made out. The systematic famishing—the neglect of the sick—the decoying into the manufacturing districts under false pretences—the cruelty to the aged and infants—and the general contempt of all the charities and decencies of life, were, alas! but too easy of proof. Let us recapitulate some of this evidence.

Systematic famishing.—As this is one of the most glaring and sweeping of all the evils introduced by the utilitarian faction in their interpretation of

the new Act, it was natural that all their rhetoric should be employed to varnish it over. Sometimes it is denied altogether, with a coolness worthy of the Central Court, and twelve ounces of bread are asserted to be a very handsome provision; at other times it is alleged that the diet of those confined in a union Bastille ought necessarily to be worse than that of the labourers outside. Now, as no man can tell to what minimum of bread and gruel competition and the fear of the union may drive the labourer, it is clear that the twelve ounces may be abridged to something below even Cornaro's limits. Thus when the victim inside gets his twelve ounces a day, the labourer outside has, we may suppose, thirteen; but competition and the fear of entering the *asylum* compel him likewise to put up with twelve; it will then be necessary to reduce the union allowance to eleven, and so on *ad infinitum*, to the marvellous economy of parochial funds, and great advancement of experimental philosophy!

We showed in our last article on this subject*, that Mr. Mott fell into a strange error in this matter of diet. He thought that persons using little exercise should have from twelve to eighteen ounces of food daily; soldiers and sailors on ordinary peace duty, or labourers, or mechanics, from eighteen to twenty-four; and persons working hard from twenty-four to thirty ounces. So far, however, were Mr. M.'s fancies from the truth, that sailors, as we formerly observed, though included in this second class, have a far better diet than he allows to the third, and are still rather under than over-fed. As to soldiers, Mr. Mott and his coterie are still more grossly in error. We have just seen that he places soldiers in the second class, who ought to consume from eighteen to twenty-four ounces of

food daily. This agrees with the following quotation from a report which we find in a utilitarian print.

"I have shown that the difference in the quantities of food consumed by agricultural labourers and artisans, when procured in return for their labour, as compared with the allowances which persons obtain by becoming inmates of workhouses, prisoners in our gaols, convicted thieves, or transported felons, are as follows:—

"As agricultural labourers . . .	122
As artisans (of the highest wages)	140
As paupers	150
As soldiers	168
As prisoners in gaol	217
As convicts on board the hulks	239
As transported felons or convicts	330."

The diet of soldiers, as given in this quotation, and as Mr. Mott thinks it ought to be, agree together most remarkably; but how do they agree with the reality? Why, not at all. The rations of soldiers are stated as follows, by a much better authority than Mr. Mott, or any of his clique:

"*Mode of living of the soldiers.*—It is unnecessary to enlarge much on this subject, so familiar to every military man. The soldier's ration is, one pound of bread, one pound of meat, and [in certain places abroad] one pint of wine, daily, with two pounds of wood for cooking. He is provided, out of his pay, with a hot breakfast of tea, coffee, or cocoa, and an additional pound of bread previous to his going on duty; at *one*, he has his ration of meat and bread, for dinner*."

Thus we see that, in bread and meat alone, the soldier has twice as much as the utilitarians have assumed: nay, that he has six pounds more in every week than "transported felons or convicts," who, in the above quotation, stand at the head of the eating list, like so many guilty aldermen!

There is a pleasant story in the books of a highwayman who saved his

* Vol. xx. p. 357.

* Hennen's Sketches of the Medical Topography of the Mediterranean, p. 218.

neck by a most elaborate artifice. The prosecutor swore that the robbery had been committed while the moon was shining brightly; but on the prisoner's counsel handing up an almanack to the judge, it appeared that the moon did not shine at all on the night of the robbery. The almanack had been printed for the occasion! We do not think, however, that the Poor-Law people, who are now on their trial at the bar of public opinion, will be able to escape from their uncomfortable predicament by a similar stratagem. It is in vain that they print their dark almanacks—in vain that they tell us of accetic soldiers and sailors (*pour encourager les autres*,)—in vain that they garble the letters addressed to them*, and attribute every casual improvement to the fear of their gruel-houses,—the people of England cannot be gulled in this manner; and either the evil must be remedied, and the huge lie swept away, or the growing estrangement of the poorer from their more opulent classes will produce consequences which we shrink from describing.

In the Report of the Select Committee, it appears, from the evidence of Mr. James Lambert (who was governor of Westhampnett Workhouse, from Nov. 3d, 1832, to March 1st, 1836,) that as soon as the new diet table came into operation, the weekly allowance of bread was reduced from 126 ounces to 78; of meat-pudding, from 20 ounces to 14; of suet-pudding, increased from 20 ounces to 28½. So that the experimentalists had reduced the daily slice of bread to even less than twelve ounces! This gave

rise to a curious scene. One of the starved came before the Board of tormentors with his bit of bread in his hand; and our readers will naturally suppose, that the very sight of such a mouthful would be a sufficient answer to the shameless assertions and falsified tables of the utilitarians. "The bell rang," says Mr. Lambert, in his evidence, "and the Duke of Richmond asked me, if that was all the bread I gave to this man; I answered, 'Yes, I think it is, by what I can see.' The Duke asked me how much it should weigh; I said, 'Twelve ounces, when I gave it to him.' And he said, 'Fetch me your weights and scales;' and I did so. I put it in, and it weighed twelve ounces. The man made an answer, and said, 'Do you think this is enough for a man to work with all through?' The Duke said, he could not give him any more, as that was what the diet-table expressed, and that was all; but the man made answer and said, that it was not so much as he had in Petworth gaol*."

Neglect of the sick.—Under this head it would be sufficient to allege the perverse ingenuity of the Commissioners in obtaining medical men at the cheapest rate, without regard to any but the technical qualifications; well knowing that the rate is often so low as not to cover even the expense of proper drugs; we have often alluded to this part of the subject before, and at some length; but we think it right just to mention the case of Honor Shawyer. This unfortunate woman died of mortification of the bowels in Bishop's Waltham Workhouse; and though the grossest neglect had been made out against the master and mistress of this receptacle, and formally presented in the Coroner's

* Mr. Dainty, clerk of the Petworth Union, in a letter to Assistant-Commissioner Hawley, after speaking of an improvement in the conduct of the poor, says: "I must add, that many circumstances have occurred in tending to produce the effects I have described: the first is the establishment of an active and efficient police in the town of Petworth." But Mr. Hawley omitted this passage, because, he says the police in the town of Petworth could have nothing to do with the parishes in the Petworth Union! (First Report, p. 61.)

† Seventeenth Report, p. 66.

* Seventeenth Report, p. 17.

From the subsequent answers of Mr. Lambert, it appears doubtful whether the man said that it was not so much as he had in Petworth gaol, or not so much as *those* had who were confined in Petworth gaol.

inquest on Honor Shawyer, they were not dismissed from their situations until seven weeks afterwards. They were succeeded by John Murphy; and so little impression had been made upon the experimentalists by this lamentable case—so little disposed were they to return to the paths of humanity, that it was during Murphy's mastership, but against his wish, that the famous attempt was made to give the paupers in Bishop's Waltham Workhouse pork-water for soup, and puddings made of the skimmings of pork-water instead of suet*. The Rev. Mr. Brock, seeing how the sick were neglected, used to send them food from his own house; for which he was found fault with by the *Guardians*, and called meddling and officious†.

Decoying into manufacturing districts.—Among other whims and oddities of the Commissioners, they imagined that they were qualified to regulate and equalize the labour-market all over England; and accordingly endeavoured to transfer the victims of their experiments from the agricultural to the manufacturing districts. The case of poor Sopp, whom they transplanted in this manner, will occur to many of our readers. To aid the scheme, one Muggeridge, who is called a migration agent, sends from Manchester the price-lists of a puffing shop-keeper; and though they would hardly take in any reasonable person, yet they might easily impose on a famished labourer, ready to catch at a straw. Thus, for instance, the Manchester advertiser tells us that he sells—Men's trowsers, fustian, from 1s. 10d.; beaverteen, from 3s.; beaverteen lined, from 4s.; moleskin, from 4s. 9d.‡ Is there any one but Muggeridge who does not know that lists of this

kind, in which the lowest prices only are mentioned, are purely deceptive; and that the trowsers in question (if at all forthcoming) would be, like Peter Pindar's razors, made only to sell? If the man does not know this, instead of being fit to be a "migration agent," he is hardly fit to walk about by himself.

Cruelty to children and the aged.—

Three children, between four and five years of age, named Withers, Cook, and Warren, were sent to the Fareham from the Bishop's Waltham Workhouse. One remained in his new abode for twelve weeks, the other two for eight weeks each. Mr. Wm. Harrison, the master of the Bishop's Waltham Workhouse, describes their state when they returned. They were standing against the wall in a passage; he desired them to walk into a room; they attempted, but could not. It was a cold day moreover, and they were without proper clothing. After about a quarter of an hour, food was offered them, which Warren vomited up again. It appears, that during their stay in Fareham Workhouse, in order to cure their dirty habits, these miserable infants had been additionally starved, flogged, and placed in the stocks—sometimes from 9 to 12, and from 2 to 5. One of them had a prolapsus ani*.

This monstrous case was rather too bad, even for the Poor-Law Commissioners; if they had no compassion, they had some fear of public indignation, and accordingly reproofs were dealt forth to all the persons concerned in tormenting these poor little creatures. The misfortune is, that every one suspects that the rigour of the Fareham people proceeded in part from their desire of acting up to the spirit of the new law, and consequently the Commissioners do not get all the credit they could wish for their humane reproofs. It is so loudly avowed, say the censo-

* Third Report, pp. 104, 105. . Seventh Report, p. 14.

† Fourth Report, p. 4.

‡ First Report, p. 38.

* Third Report, p. 43.

rious, that to make the poor systematically uncomfortable is the object of the Union receptacles, that it cannot be wondered at if the directors of these fastnesses sometimes overshoot the mark, and worry their inmates to a degree that cannot be publicly defended.

Nor has the other extremity of life been a protection from the rigour of the new regulations. Jas. Sparshott, *aged ninety-one*, had his allowance reduced from three shillings and sixpence to half-a-crown a week*. The Rev. Mr. Spencer, indeed, Chairman to the Board of Guardians of the Bath Union, and a starvationist of the first water, speaks with indignation of a pauper, aged 84, who refuses to reside in the workhouse, as well as of certain mischievous ladies who not only abet and comfort him in this his nefarious obstinacy, but actually give him a weekly allowance!† Our austere chairman says, that rather than be washed, shaved, and have his hair cut, many a pauper has gone away from the workhouse. Perhaps this aversion to being shaved, Mr. Chairman, may arise from your shaving them so very close.

We will proceed with the other articles of our charge in a future number.

ST. GEORGE'S HOSPITAL.

ABSTRACT OF A CLINICAL LECTURE ON DISEASE OF THE KIDNEY,

Delivered Oct. 10, 1837,

By SIR B. C. BRODIE, BART.

SIR BENJAMIN BRODIE commenced his course of Lectures on Clinical Surgery at the usual hour (half-past one.) He began by stating, that he would not trouble the class with any thing in the shape of an introductory lecture, as they had probably heard the subjects of such addresses already sufficiently dwelt upon elsewhere. He would only observe, that the course

was necessarily limited by the number of weeks in the medical session, and that, as he was anxious to make it as useful as possible, he should select for illustration those points which were calculated to afford the greatest amount of practical information. Now these would be found to consist, not in the rarer or greater cases merely, but would also comprehend a due attention to the smaller points in surgery, which gained in importance from their frequency what the others did from their magnitude. He should chiefly connect his lectures with cases in the hospital, but would occasionally devote two or three lectures to any particular subject which he was desirous of illustrating, or which they themselves might wish to have explained to them. He wished the pupils to talk together about the cases, and to agree upon those points on which they required farther information; he would always be happy to attend, as far as possible, to their wishes in this respect; and he meant the observation to apply to the junior pupils as much as to the senior, for they had an equal right to have their interests attended to, and the hospital practice made as useful as possible to them.

He had been absent for some weeks, and had not had an opportunity of becoming acquainted with the cases; but his clerk had put one into his hands, which seemed well calculated to be the text of some remarks.

James Lambert, *ætat.* 40, admitted September 20, 1837. Complaints of pain in the region of the bladder, which first came on ten weeks ago, when he likewise perceived the urine to be muddy, with a thick sediment. The pain is most severe when the urine contains the largest quantity of sediment; and it then extends to the loins and thighs. The flow of urine often stops suddenly, and this is followed by cutting pain in the urethra; after which he cannot make water for more than half an hour. Has never passed blood, but there is a white purulent deposit in the urine, which is albuminous; has no stricture.

Infus. Diosmæ, ʒiiss. ter die. C.C. Lumbis, ad ʒx.

Pain in the loins was relieved by the cupping.

25th.—Urine became a little clearer, but this lasted only for a short time.

Morphiæ Acet. gr. ʒ, 8vis horis. Extract Colocynth. C. gr. v. om. nocte.

Oct. 8th.—No change.—*Omitt. Morph.*

The prominent symptoms here were—the frequent desire to make water, the pain referred to the penis and urethra in voiding it, and the purulent deposit in the urine. The urine, it was farther to be

* Tenth Report, p. 21.

† Second Report of the Poor-Law Commissioners.

remarked, was acid and albuminous. Its acidity was proved by its reddening litmus paper; and the albuminous character shewn by its dropping a coagulum on the addition of nitric acid. There had been pain in the loin, and down the inside of the thigh. Now this was just such a case as used formerly to be referred to irritable bladder, but he believed it would be found to depend upon disease of the kidney. When he began practice, a patient with such symptoms used to be examined to see if he had any stricture or enlargement of the prostate gland, and it was also inquired whether he had any appearance of calculus; and if he had none of these, it was then thought that the case was sufficiently explained by calling it one of "irritable bladder."

He had, many years ago, attended a lady who voided her urine in small quantities, with a severe pain in the urethra, &c. and he had sounded her more than once, supposing there might be a calculus in the bladder, but without finding any. None of the remedies used were of much avail, but some time afterwards she was seized with pain in the course of the ureter, which suddenly ceased, and next day she voided a mulberry calculus, from which time the symptoms were relieved. In this case there can be no doubt but that the symptoms, which were referred entirely to the bladder, depended upon the lodgement of a calculus in the kidney. He attended a gentleman whose bladder could not retain above a table spoonful of water; the urine was albuminous and purulent. No stricture of the urethra existed, and there was no stone to be found in the bladder. In this case, too, the remedies proved inefficient: at last he was seized with pain in the course of the ureter, extending to the bladder, and followed by the expulsion of a solid portion of lymph, with a fimbriated extremity. He had several similar attacks, and under one of these he died in the country.

Morgagni relates a case of this kind.—A boy had pain referred to the urethra and bladder, with inability to retain his urine; he died, and on examination after death, the urethra, prostate, and bladder, were found to be healthy, but there was a stone in the kidney. Sir Benjamin had himself seen several cases more or less similar—that is to say, in which symptoms referred to the bladder had been proved to depend upon disease in the kidney. He had attended a patient who laboured under frequent desire to make water, pain in the urethra and neck of the bladder, and albuminous urine, and who ultimately died with disease in the kidneys, and none in the bladder.

In another case the patient had similar symptoms, and died from a cause foreign to urinary disease—namely, from bursting

of the gall-bladder, and subsequent peritoneal inflammation, for which he was attended by Dr. James Johnson. Now here there were found a dark coloured and spongy condition of the kidney, with deposits under its external coat of bodies of transparent appearance, resembling hydatids, though essentially different in character, while the tunic of the kidney adhered so firmly to the fat of the loins as to remain *in situ*, while the kidney was peeled out from it. The bladder was free from disease, although all the symptoms had been referred to it and the urethra.

But it is only when patients labouring under disease of the kidneys die early—cut off as it were by some accidental occurrence, that the dependence of the symptoms exclusively on such affection of the kidney can be proved, because disease of one part of the urinary system soon brings on disease of other parts. One of the first of such extensions is to the bladder, the mucous membrane of which becomes irritated by the morbid state of the urine, and then inflamed; being once inflamed, it in its turn secretes aropy tenacious mucus, which sticks like bird-lime to the interior of the vessel into which it is voided. Portions of phosphate of lime frequently show themselves; and now the urine becomes alkaline, and is often extremely fetid.

But another and peculiar circumstance which sometimes attends disease originating in the kidney, is abscess at the neck of the bladder. Sir Benjamin has seen several examples of this—i. e. of patients having disease of the kidneys, whether suppuration, calculus, or other change, becoming affected with abscess at the neck of the bladder, and in one such he had opened the abscess in the perineum, and evacuated a large quantity of pus, the probe passing into an extensive ulcerated cavity connected with the prostate gland. Perhaps this phenomenon was explicable on the principle of the nerves of the part being in a constant state of irritation,—a condition which, under other circumstances, was productive of inflammation and its consequences. Thus he remembered that in the case of the late Dr. Peimberton the face was swollen on the side where he had suffered most from pain. Now his disease was an aggravated instance of the *tic douloureux*, and Sir Benjamin inferred that the excited state of the nerves had, after a certain time, led to inflammation in the cellular tissue of the parts. So, likewise, with regard to abscess which formed at the neck of the bladder in disease of the kidney. The nerves were then in a state of constant irritation, as indicated by the inability to retain the urine, and this, in some constitutions, was productive of inflammatory

action on the contiguous parts, ending in suppuration.

With regard to the kind of disease which took place in the kidney in such cases as he was considering, Sir Benjamin did not think that it was necessarily always the same. Sometimes it seemed to be a form of chronic inflammation, either limited to the tubular part of the gland, or pervading its whole structure: sometimes there appeared to be distinct abscess formed; at others the pus would rather appear to be formed by the lining membrane, without any ulceration. Sometimes there was calculus impacted in the kidney: sometimes there were those serous cysts to which he had already alluded; and sometimes the gland was altogether converted into a collection of cysts or vesicles, the glandular structure being nearly or wholly destroyed.

The symptoms which characterised disease of the kidney at its early stage, such as the case before them, were pain referred to the penis and urethra, with incontinence of urine, so that the patient is obliged to void it sometimes so frequently as every half hour; uneasiness, sometimes amounting to pain, in the region of the bladder itself; and occasionally, but not necessarily, pain in the loins, or in one loin, corresponding to the diseased kidney. The urine at this period is sometimes quite clear and healthy, at least it is at times free from albumen, but always becomes opalescent sooner or later, and coagulates on testing it with nitric acid.

To these symptoms at a more advanced stage become added paroxysms of pain, extending from the kidney to the bladder, with affection of the testicle or inside of the thigh. The urine also contains pus; and when this comes suddenly in considerable quantity, and afterwards nearly disappears, it may be conjectured that it is from an abscess. But in other cases pus continues long to show itself as a deposit in the urine, without the symptoms of abscess, and is probably secreted by the kidney without abrasion of surface. Frequently, as in the case before them, there was a deposit of purulent matter; and whether this were so or not, it was acid and albuminous; not that it invariably had these last characters, but they were always present at times in the course of the disease.

Then the urine is usually possessed of another and very peculiar character in its smell. No peculiarity of this kind admits of description; but of the familiar odours which it most resembled, that of musk came nearest. Now this musk-like odour must not be confounded with that which arises from mucus secreted in the bladder; this last is ammoniacal, and the urine often absolutely stinks. This

latter state is owing to inflammation of the mucous membrane of the bladder, and the decomposition of its secretions, so as to evolve ammonia.

With respect to treatment: one of the most distressing circumstances was the inability to retain the urine, from the irritability of the bladder, which, as already stated, was sometimes intolerant of half an ounce of that fluid. Now something might be done to relieve both this and the pain accompanying it, by the use of opiates; but he was, upon the whole, averse to these, as they sooner or later interfered with the functions of the stomach, which it was of great importance in these cases to cherish: besides which, the opium had no tendency to cure the disease eventually. He therefore limited his patients to a suppository of opium introduced into the rectum, or a clyster at night. In regard to the power of the bladder, he had known a patient improve this by accustoming it to the presence of quantities gradually increased: he had injected two ounces of tepid water, and, persuading the bladder to retain that, had increased the injection to half a pint; thus, by perseverance, enabling the bladder to acquire better habits, so that he was able to go into society, which he had previously been prevented from doing. But those two (the opiates and injections), where it was expedient to try the latter, could only be looked upon as palliative means. Considering the symptoms during life, and the result of post-mortem examinations, there was reason to regard such cases as very generally connected with inflammation about the kidney; and in keeping with this, was the fact that he had seen considerable and permanent relief from counter irritation being established in the loins, either by means of a seton or caustic issue. He had also seen blistering of much service.

As to internal remedies more particularly directed against the disease, he thought that a distinction was to be made between the condition of the bladder often consequent upon disease of the kidney, and original disease of the latter. The former, when attended with secretion ofropy alkaline mucus, was frequently much benefited by the *Pareira brava*; but this had little if any effect on the kidney. In the case before them, the decoction of the *Diosma* had been ordered, in doses of $\mathfrak{z}\text{ss}$. three times a-day. This was a remedy from which he had repeatedly seen unequivocal benefit in such cases; and another which also occasionally proved of service, was the *Uva ursi*, but for this purpose it required to be given in much larger doses than those usually administered; perhaps the best form was a decoction of $\mathfrak{z}\text{ss}$. or $\mathfrak{z}\text{j}$. in 6 oz. of water. There was a popular medicine, too, which he would

mention, though it had not found its way into the Pharmacopœia: he meant the seeds of the wild carrot. This plant, which had long been used for such purpose among the people, was always kept at Covent Garden, though not to be had at the druggists.

The hour having expired, prevented Sir Benjamin from concluding the subject in the present lecture.

HÔPITAL DE LA PITIÉ.

SECOND SERIES OF CASES*.

CASE I. — *Pleurisy, with Effusion.*

AN ironmonger, aged 32, of a strong constitution, was admitted a patient, under M. Louis, January 17th.

He has lived freely, and is generally tipsy once a week, and is subject to epistaxis. He has been ill six weeks. His illness commenced with a pain in the right side; fifteen days afterwards he had a cough, with fever, so as to be confined to his room: the fever has continued from that time, and the pain has never left him. He has never expectorated blood, nor been subject to cold.

18th.—Countenance bloated; skin moist and warm; pulse 96 yesterday evening, 62 this morning; anxious; thirst; no headache; lies with most ease on the left side, more so than on the back; tongue moist, and whitish; sputa clear. Percussion loud under both clavicles; dull below the right mamma; dull posteriorly in the inferior half of the right side; in the latter situation the respiration is nearly inaudible, but there is no ægophony or bronchophony. Sounds of the heart very distinct.

Potass. Acet. ʒss. in Mist. Mucilaginis. Venæsectio ad ʒx. Low diet.

19th.—Coagulum firm, and covered with a grey coat; less cough; slept well; tongue white; respiration dry and harsh under the right clavicle. No other change since yesterday.

Rep. Mist. Potassæ Acet. Pulv. Digitalis, gr. j.

21st.—Pulse 64. Less cough; tongue natural. The same dull sound on the right side posteriorly; respiration weak posteriorly, absent inferiorly, on the right side. No irregular bruit. Has had a pain in the left side since yesterday.

24th.—Pulse 72; no expectoration. Very little difference between the sounds on percussion posteriorly, but the respiration is rather weaker on the right side. In a few days he was able to leave the hospital.

REMARKS.—The general symptoms were

not, in this case, sufficiently acute to be the consequence of a pneumonia of so great an extent; besides, there was complete absence of crepitation and the characteristic sputa. Bronchophony would also have been heard. The dulness upon the right side must, then, be attributed to inflammation of the pleura, which was approaching its termination when the patient was admitted. As there was no ægophony, M. Louis gave it as his opinion that but little fluid had been effused, but that false membranes had been formed. There was no reason, either from the history of the case or from the local signs, to suppose the existence of tubercles; but a few days before he left the hospital, he complained of a pain in the left side. Should this pain return, the existence of tubercles in the lungs would be rendered very probable, as it is very rare to see idiopathic pleurisy attack, at the same time, both sides of the chest, while it is comparatively common when produced by tubercles.

CASE II. — *Pleurisy with Effusion. — Emphysema.*

A man, aged 30, of a strong constitution, was admitted a patient, under M. Louis, March 2d.

He has resided in Paris several years, and always enjoyed good health. His present illness commenced a fortnight ago, previous to which time he had not the slightest ailment. His work is not laborious, but the day he was taken ill he worked more than usual. In the evening a pain came on under the left mamma, which has continued since, but not very acute. He had a little fever also, but was not prevented from pursuing his occupation; his strength, however, failed him, and he lost his appetite. Two days before his admission the pain became more acute, accompanied with cough and considerable dyspnoea.

3d.—His face is pale; heat of skin natural; pain in the side not very acute; pulse 88; respiration 24; percussion dull posteriorly, on the left side, in the three inferior fourths; bronchophony and bronchial respiration in the whole extent. When placed on his abdomen, the sound is clear and loud, and the bronchial respiration ceases, but the voice resounds still in parts, and very distinctly just below the angle of the scapula.

Ptisan. Venæsectio ad ʒxiv. Low diet.

4th.—Pain and dyspnoea relieved three hours after the bleeding, but he bore the loss of blood badly. Coagulum not very firm, without any coat, and covered with much serum. Pulse 88; no headache; heat of skin natural. Complete dulness

* The first series was given in our last volume.

upon percussion in the three inferior fourths of the left side; bronchophony to within four inches of the summit, but less marked inferiorly. Percussion and respiration natural on the right side. Anteriorly, percussion is louder for some extent under the left clavicle than on the opposite side, and the respiratory murmur is weaker; below the left mamma percussion is dull. Above the left mamma, the chest projects slightly, and the intercostal spaces are nearly obliterated. Pulsations of the heart feeble. He had never been subject to dyspnoea.

Acet. Potass. 3ss. to be put in the Ptsan. Pulv. Digitalis, gr. j.

6th.—Dyspnoea considerably relieved; the local signs remain the same. After lying on his belly a minute or two, the percussion is not so dull, but the difference is not so great as on the first day. No appetite.

Pulv. Digitalis, gr. j. bis in die.

On the 7th he took four gains of digitalis daily; on the 9th, five. On the 14th he vomited a greenish fluid; had two or three stools; percussion yielded more sound under the left mamma, where there was distinct crepitation. On the 21st, there was crepitation posteriorly on the left side, with a little bruit de frottement, but percussion continued dull: his appetite had returned. On the 25th he was on full diet; the dullness persisted posteriorly; there was fine crepitation in the inferior third; a little bronchophony in the middle third, also at the very inferior part of the axilla, where crepitation was also heard. On the 27th he left the hospital, his general health being quite re-established.

REMARKS.—The diagnosis of this case was very easy, though at the commencement there might have been some difficulty in distinguishing it from pleurodynia; as the pain was unaccompanied by cough, fever, or diminution of strength. The prominence of the chest anteriorly on the left side—the slight depression of the intercostal muscles—the obscure respiratory murmur—the loud percussion, and that, too, on the side of the effusion—all denoted the existence of emphysema. The patient had never been subject to dyspnoea. This is only the third case which has fallen under M. Louis' notice, where emphysema has existed unattended by dyspnoea.

M. Louis does not regard pleurisy when simple and uncomplicated, and when it does not occur in an aged individual, as a dangerous disease. Indeed, under these circumstances, he has never seen it fatal, though above 150 cases have been treated and recorded by him. His treatment consists of one or two bleedings at the com-

mencement, and in the administration of diuretics. Patients affected with pleurisy seldom apply for admission into an hospital so early as those attacked with pneumonia, and require less energetic treatment.

Cases of fractured ribs are almost always accompanied by some degree of pleurisy, and yet unless the lung is extensively lacerated, and there are other severe injuries, they rarely prove fatal. Thus, out of about 200 cases admitted into the London Hospital during the last two years, twelve only have died. Of these, eight were above sixty years of age, and three above fifty. In three others, injuries existed sufficient to cause death. In seven the lung was extensively lacerated, and several ribs were fractured. In two only was pleurisy the cause of death. One was aged fifty, and died on the ninth day; the other was aged sixty-eight, and died on the third day.

Towards the termination of the inflammation in the preceding case, crepitation was distinctly heard on the left side. M. Louis attributed this noise to the friction of false membranes imperfectly organized. The same bruit is often heard in cases of pneumonia, when they are convalescent, and which is most likely caused by the same lesion*. False membranes often present a reticulated appearance, and we can easily believe that the friction would produce a sound very similar to crepitation.

CASE III. — *Vesicular and Interlobular Emphysema.*

A very intelligent sailor, aged 56, of a strong constitution, entered the hospital April 5th. When admitted he had a little fever, much dyspnoea, and cough, with subcrepitation on both sides, posteriorly. He was bled, but without relief; some laudanum was given, and his breathing became easier. He was left-handed, on account of a wound he had received in the right shoulder. Four or five years ago, after making a violent effort in a trial of feats and strength, he had a hernia on the left side, and suddenly a difficulty of breathing, which has continued to the present time, with frequent accessions of dyspnoea and palpitations, during the last two years. Previous to this exertion his breathing was quite free. Since this time he has been very subject to colds and coughs.

April 8th. — Countenance natural; dyspnoea diminished; chest well formed; a projection on the left side, extending from a little below the mamma to

* No allusion is here made to the crepitation *redax* of Laennec, which depends upon engorgement of the lung.

within an inch of the clavicle; a similar projection on the right side, but not so great. The projection on the left side is partly due to a greater development of the pectoralis major muscle: the left shoulder is also a little more elevated than the right. Percussion clear and loud anteriorly, but particularly so in the precordial region. Respiration natural, but weaker in the same region. Posteriorly, percussion is loud; on the left side the respiratory murmur is feeble. A little subcrepitation on the left side inferiorly; more on the right. A little below the left mamma, anteriorly and laterally, a bruit de frottement is heard; the pulsations of the heart are inaudible; no œdema; palpitations frequent; digestive functions healthy; pulse regular.

REMARKS.—The accidents more frequently caused by a sudden strain in which dyspnœa and palpitations are present, are rupture of vessels and aneurisms. In one man examined by M. Louis, who, after a sudden strain, had dyspnœa, palpitations, and a bruit de lime, the aortic orifice was found very much contracted. Aneurisms, however, are generally the result, very often at the origin of aorta, where they are more difficult to detect, and generally prove fatal in a shorter period.

The pulse in this case was regular; there was no impulsion—no abnormal bruit; the sounds of the heart could not even be heard, so that the dyspnœa and palpitations could neither be attributed to aneurism nor contraction of the aortic orifice. The projection on the left side, the loud sound upon percussion even through the thick muscular parietes, the feeble respiration, the dyspnœa, the subcrepitation, indicated the existence of emphysema caused by the rupture of some air-cells at the time of the effort. The peculiar bruit de frottement renders the existence of *interlobular* emphysema very probable, as that is generally accompanied by some bruit.

The patient soon left the hospital much relieved, having taken small doses of opium.

improvements introduced by Mr. Key, whose proposition of operating without opening the sac should not be looked upon as a mere revival of an old idea, since obsolete works, in alluding to the subject, advised such an operation from mere mechanical views, dreading the descent of a great quantity of intestine if the sac were opened in large herniæ; but Mr. Key looked upon the opening of the peritoneal cavity as the great source of danger; and this he hoped to obviate, in many instances, by dividing the stricture outside the sac. Mr. C. stated that he had performed the operation seven times. Six out of the seven instances were successful: the seventh was a case in which the operation was not permitted till strangulation had existed for so long a time that recovery was almost hopeless under any circumstances. Mr. C. advised the administration of small doses of sulphate of magnesia at intervals, after the operation.

In answer to questions from various gentlemen, Mr. C. strongly deprecated the use of tobacco injection, recommending bleeding in the erect posture as a substitute. He stated the case of a robust woman, which occurred several years ago, in Norfolk, and terminated fatally in twenty-five minutes, from the use of a tobacco injection, made with half a drachm of the tobacco leaves. His reason for exhibiting saline purgatives was to induce a healthy peristaltic motion in the intestine just relieved from strangulation; this motion he considered as calculated to produce a return of the natural secretion of the intestines.

At the close of the meeting a vote of thanks to Mr. Cooper, for his valuable communication, was unanimously carried, on the motion of Mr. Dendy, seconded by Mr. Iliff.

At the next meeting of the Society, on the 21st instant, Mr. Aston Key in the chair, Dr. Guy will read a paper *On the Effects of Posture on the Pulse*.

WESTMINSTER HOSPITAL.

Severe Cut Throat.

WILLIAM LYNCH BOOTH, æt. 21, whose tattooed skin and hard hands indicated his rank to be plebeian, was admitted on the 4th September, 1837, having inflicted a severe wound on his own person. Passing by a butcher's shop, he suddenly rushed into it, and seizing a knife, drew it across his throat. The edge of the knife passed between the os hyoides and thyroid cartilage, and divided the anterior half of the pharynx. He had fainted from loss of

GUY'S HOSPITAL.

PHYSICAL SOCIETY.

THE first meeting of the Physical Society of Guy's Hospital, for the present session, took place on the 7th instant, Dr. Addison in the chair; when Mr. Bransby B. Cooper read a paper *On Hernia*.

The chief points alluded to by that gentleman were—1st, the advantages to be derived from a correct knowledge of the anatomy of the fascia transversalis, as described by Sir A. Cooper; and 2d, the

blood, and was insensible when brought into the surgery. The thyroïd arteries had been divided, but the carotids were safe. He was placed on his side, a position which he preferred, and simple pledgets were laid over the wound.

Next morning (5th Sept.) Mr. White endeavoured, but without success, to introduce an elastic tube through the nose into the stomach. A violent spasm foiled the attempt. He, however, succeeded in passing the instrument through the wound into the œsophagus, and thus formed a channel through which beef-tea and thick nutritious fluids were conveyed into the stomach. During the whole of the 5th the man was quiet, but at night a paroxysm of delirium supervened; hence it became necessary to fasten his hands and feet.

9th.—Each succeeding day he was perfectly quiescent till evening, when the delirium returned with almost maniacal violence. Last evening the usual paroxysm came on at about nine o'clock, and continued with undiminished violence till one A.M., when he became tranquil. At four A.M., three hours after this change, he expired suddenly.

During the day-time his pulse was quiet, and generally numbered between 80 and 90, but at night, immediately after the exacerbation, it reached to 150 beats. At these times the animal heat was also increased. His bowels were acted upon every morning by means of a clyster. In the intervals of the paroxysms he was rational. He could articulate words in a whisper, but there was no vocalization of sound. He generally preferred making known his wants by writing.

An examination of the body was made a few hours after death. All the viscera of the great cavities were apparently healthy. The brain offered no pathological characters; but in the cerebellum the medullary part was tinged of a uniform and homogeneous pink colour, not arising from any obvious injection of vessels. The edge of the wound was covered with lymph and exsiccated mucus, and signs of œdema were obvious in the tissues adjacent to the glottis, which, had the patient lived long enough, would have destroyed him. No injury was inflicted on the glottis by the knife, which had made a clean cut between it and the epiglottis.

It appears that when a cloth was placed over the wound, the patient had the power to vocalize imperfectly; but when the cloth was removed, though the breath passed through the mouth in sufficient quantity to enable him to whisper audibly, yet none of the vowel qualities could be produced.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, Oct. 12, 1837.

Edward Tayloe, Duffield.—Wm. Foss, Stockton-on-Tees.—James Bridge Hall, Lane-End, Stafford.—George Field, Bowlinz.—William Bell Irving, Gosforth, Cumberland.—William Garlike, Painswll, Gloucester-hire.—Joshua Parsons, Laverton, Somerset.—John Prideaux Sealy, Bristol.—James Henderson, Carfrae, Edinburgh.—George John Arnold, Wilt-hire.—John Davies, Tipton, Staffordshire.—Alfred Evans, Mile-End, London.—John Bryant Lyon, Sheffield.—Samuel George Sloman, Exeter.—Thomas Evans, Little-Hampton, Sussex.—George Samuel Liddbitter, Tunbridge Wells.

WEEKLY ACCOUNT OF BURIALS.

From BILLS OF MORTALITY, Oct. 10, 1837.

Abscess	1	Hæmorrhage	1
Age and Debility	28	Heart, diseased	2
Apoplexy	2	Whooping Cough	4
Asthma	4	Inflammation	17
Cancer	1	Lungs and Pleura	1
Childbirth	1	Influenza	2
Consumption	40	Insanity	2
Convulsions	12	Liver, diseased	3
Croup	1	Measles	3
Dentition or Teething	8	Mortification	1
Diarrhoea	2	Paralysis	2
Dropsy	3	Rheumatism	1
Dropsy in the Brain	8	Small-pox	1
Dropsy in the Chest	1	Thrush	1
Erysipelas	1	Tumor	1
Fever	13	Unknown Causes	11
Fever, Scarlet	2		
Fever, Typhus	5	Casualties	5

Increase of Burials, as compared with }
the preceding week } 104

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.

Oct.	THERMOMETER.		BAROMETER.	
Thursday	5	from 45 to 65	30.13 to 30.11	
Friday	6	53 65	30.01 30.00	
Saturday	7	42 63	30.00 30.13	
Sunday	8	45 63	30.10 30.07	
Monday	9	44 59	30.14 30.19	
Tuesday	10	40 62	30.20 30.25	
Wednesday	11	49 63	30.26 30.31	

Wind. S.W.

Except the 8th, 10th, and 11th, generally clear; rain on the 6th and 8th.

Rain fallen, .3875 of an inch.

CHARLES HENRY ADAMS.

ERRATA.—In Dr. Labatt's paper, page 8, col. 2, line 50, for "profunda," read "profundæ;" page 10, col. 1, line 30, for "when," read "where;" ib. col. 2, line 28, for "first," read "just."

WILSON & SON, Printers, 57, Skinner-st., London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, OCTOBER 21, 1837.

LECTURES ON THE PHYSIOLOGY AND DISEASES OF THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL
AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of
1836 and 1837,

*At the Anatomical School, Kinnerton-Street,
near St George's Hospital,*

By CHARLES J. B. WILLIAMS, M.D. F.R.S.

LECTURE IV.

Physical Examination of the Chest—Modes of Studying the Topography of Organs—Examination of the Chest by inspection and feeling—Natural form of the Chest—Postures for inspection—Examination of the motions of the Chest by sight and touch—Varieties of Respiration—Menturation of the Chest—Measurement of the Air respired—Limitation of the Signs obtained by sight and touch—Need of another Sense—Modes of Studying the Acoustic Phenomena of the Chest.

HAVING now anatomically and physiologically gone through the general structure, functions, and relations of the chest and its organs, we have next to study these subjects as physicians; and you will immediately see how essential our previous knowledge is to prepare us for this study. We have to find out the signs or symptoms through which, in the living body, we can judge of the condition of the various parts of the structure, and of the performance of their several functions, and thus through which we can distinguish health and disease. This department we will, if you please, call EXAMINATION OF

THE CHEST; for although it is the organs within it, rather than the chest itself, which we generally wish to study, we must never forget that the chest is, as it were, an outer coat of these organs, fitted to them, and partaking of their shape, motions, and sensibilities; and as in health these all work and feel together, so in disease we must never lose sight of their mutual dependencies and connexions. Now as we have considered the properties of the chest and its organs under the two heads, physical and vital, so you see that in my prospectus I divide *Examination of the Chest* into two kinds:—1. As practised PHYSICALLY by vision, tact, and hearing;—2. THROUGH FUNCTIONS, or through those more complex properties or actions which depend on vitality superadded to physical structure. These two kinds, although studied separately, must be used conjointly.

We examine the chest PHYSICALLY, through those properties of form, size, proportions, and relative position of its parts, at rest and in motion, which are appreciable by our external senses. Now, gentlemen, mark me; you cannot get on well in the physical examination of the chest without a topographic knowledge of the organs within it: you must know where each severally lies and reaches with regard to the exterior, so that when you inspect, feel, and listen to the chest, you may be able to map out the general outlines of the organs within.

You will ask me, how are we to get this knowledge? There are two ways to it; and I would advise you to use them both. The first and most obvious one is, by attentively observing, every time you see a body opened, the position which the organs hold with regard to the exterior. The minutes which we are apt to consider as time lost, while we are impatiently waiting to see the morbid anatomy within, may be profitably employed in taking

a lesson of the anatomy of position. As the dissector's knife cuts the integuments, our eyes may rapidly survey certain external marks which we may be ready to compare with the interior when it shall be opened. There are the nipples, the edges of the pectoral and serrated muscles, the intercostal spaces, which can generally be counted with ease near the sternum, the divisions of this bone, the clavicles and scapulae, and divers minor elevations and depressions on the exterior, all of which may serve as landmarks to indicate the positions or boundaries of the organs and their several parts. The moment the sternum is raised, and before the lungs collapse (which may be prevented by closing the nostrils,) we can glance at the extent to which these organs cover the heart and reach downwards, the position of the air and blood vessels, the height of the diaphragm, and the abdominal viscera beneath, and we can transfer these various sites to some of the marks or lines of the exterior.

I do not mean to assert that this habit of comparison of the outside with the inside in the dead body will give you a very accurate knowledge of what exists in the bodies of the living; for besides that there is some variety in different individuals, there can be no doubt that on the motions and properties of life being destroyed, some changes may consequently take place in the size and relative position of the organs, and these may vary according to the mode of death. Thus it is probable that the diaphragm, relaxed by death, permits the abdominal viscera to encroach on the cavity of the chest further than during life; and the volume and position of the heart and lungs will be affected not only by this circumstance, but by the condition of the circulation and respiration at the time of death, and by the influence of time and temperature on the stiffening of the muscles, and other changes which immediately succeed death. These are subjects which deserve further investigation. In spite of these exceptions, which it is well to be aware of, I repeat that you may gain much valuable knowledge of the topography of organs in the way that I have described, and this knowledge will be of such a personal, practical kind, that it will gradually confer on you a self-correcting tact and insight which you can never gain from mere rules and descriptions.

There is another way to the knowledge of the topography of organs; it is more exact, but more difficult. It is the *habit* of physical examination; and thus our study enlightens itself. We cannot dwell on this now, because it comprehends the details which will occupy us for several succeeding lectures; but I will tell you,

by way of encouragement, that after you have carefully examined half a dozen subjects, by the different physical means which we have now to explain, you will have gained results which may supply you with a good general idea of the topography of the living chest. In short, it comes to this—practice alone can make you perfect in this, as in every other study in which the senses are to be exercised.

I have attempted in these diagrams to set before you views of the topography of the pectoral organs, and by giving a transparency both to the chest itself, and also to the viscera, I have endeavoured to represent their form, size, and relative position. Here, for instance, is an external front view of the trunk of a man: it is sketched, as you see, in Indian ink, with all the points, prominences, and depressions, which are presented in the natural contour of the body. But through this exterior you see the lungs, the heart, the liver, the stomach, the spleen, &c. tinted in different colours, and by a convenient transparency in these viscera, you get a general view of the interior—how the several large vessels pass, and how one organ is superposed over another. Thus, here are the lungs with the large bronchi, coloured light pink, and the liver purple; and where at the margin of the diaphragm the latter of these viscera comes in contact with the external parietes, its colour is deep and distinct; but where it rises above into the vaulted hollow of the diaphragm, it is seen in a fainter colour, because it is gradually shaded off by the superjacent light coloured lung. So also the heart, in the small space in which it comes in contact with the chest to the left of the lower end of the sternum, has its crimson colour; but it also communicates its tint more faintly through the lungs which lie over its whole body, and the vessels which proceed from it, and so on with other organs. Here is another diagram representing the back view of the chest and abdomen. [These diagrams cannot be given in print.]

Now you are not to suppose that these drawings give a very exact view of pectoral topography, for, as I have said before, the parts and their outlines are to a certain extent variable and shifting; but they will assist you in the two other modes of study which I have pointed out, and they will be of material service to us in our explanations of various diagnostic phenomena of health and disease. You may, too, if you please, take from them the idea of transparency; and when you examine a chest, try, in a study of external marks, to see the outlines and positions of the organs within.

Well, now let us touch lightly on the

examination of the chest by sight and feeling. The patient standing, or sitting even, with his arms and legs in corresponding positions, and his chest, if possible entirely exposed in a good light, we view it in front, behind, and from above, and mark its form and proportions, and corresponding prominences and depressions. A healthy chest is very nearly symmetrical, the two sides corresponding in shape and size. The right side is, however, almost always slightly larger than the left, especially at its lower portion, where the difference of measured circumference amounts generally to half an inch. This preponderance in favour of the right side is in part to be ascribed to its contents, but it is chiefly connected with a law which pervades the animal creation, which gives a superiority of development to the right side. In some of the mollusk tribes you see this law prevail to such an extent that the right side is rolled round the left, as in the house snail. There is, on the other hand, in most healthy chests, an advantage on the left side in point of height. The apex of the left lung, and the corresponding portion of the chest, rises a trifle higher than that of the right. I cannot tell whether this is an original condition, or whether it results from the habitual inflated pressure of the stomach and the unyielding mass of the heart on the left side giving the chest a greater tendency to upward expansion, but the fact is pretty constant. Still these slight exceptions scarcely detract from the general symmetry of the chest, and when known can be allowed for. Where you find any considerable departure from this degree of symmetry, or correspondence between the two sides, you may be pretty sure that there either is, or has been, disease.

I have said that you may inspect the chest from *above*, as well as from before and behind. This may be done when the patient is sitting on a low seat, with his head a little bowed forward, by standing behind him and looking down on the shoulders. You thus get a view of the depth of the chest from front to back, and you may often detect a want of correspondence between the two sides, that is not perceptible by the ordinary modes of inspection.

If the patient's strength do not permit him to stand or sit up, you may inspect the chest when he is lying on his back; and this may be done not only standing by the side, but also at the foot and at the head of the bed, from which the corresponding motions of the two sides can be better seen.

Now the inspection of the chest is to be applied not only to its statical condi-

tion, but also to its motions; and here it is proper to employ feeling with it. Whilst, therefore, we are inspecting the chest, we desire the patient to breathe in various degrees; and with our hands and eyes on corresponding points of the two sides, we watch and feel the amount and equality of the motions. If the chest is a healthy one, we see the motions as uniform as the chest is symmetrical. The clavicle, scapula, and upper ribs rise; the lower ribs rise and spread; and the abdomen swells as the diaphragm descends at each inspiration. Attentively watching and feeling the chest will also often enable us to trace the limits of some of these movements, so as to indicate the boundaries of the chest. Thus we have before noticed that the lower ribs are pressed outwards, by the displacement of the abdominal viscera at each descent of the diaphragm, and constitute a fulness below the limits to which the lungs descend, with a slight flatness or hollow above. My friend, Dr. Edwin Harrison, has studied these marks so successfully, that he can discover in many cases, by simple inspection, the height of the diaphragm and liver. The intercostal spaces are also fit-marks for this mode of comparison between the two sides; and there are other useful details, which I cannot now enter into: they are soon learnt in practice, when the principle of the examination is properly understood. Applying the hand in the region of the heart, we feel the relation of the respiratory motions to that organ. After a full expiration the heart is felt beating close under the hand, about the cartilages of the fourth and fifth ribs, as well as under the sternum; but as the ribs rise, and the lung expands in inspiration, we gradually lose the beating, and if it is felt at all, it is now as low as the sixth rib. I shall not dwell longer on the healthy appearance and motions of the chest, but you should take every opportunity to study them; and you will then soon be able to scan them, just as a horse dealer judges by a few glances of the points of a horse.

We shall notice a few general varieties of respiration, which are to be determined by watching and feeling the motions of the chest. Healthy or perfect respiration is both diaphragmatic and costal; but under the influence of disease the motions may be confined either to the ribs only, or the diaphragm only. Thus, when the diaphragm is prevented from descending by acute pain in it or below it, or by pressure from below, the respiration is wholly performed by the raising of the ribs, and is called *heaving*, *thoracic*, or *costal* breathing. When the ribs, again, are immoveable, in consequence of pain, ossification of cartilages and ligaments, or paralysis of the

intercostal muscles, the breathing is wholly *diaphragmatic* or *abdominal*.

The movements of respiration may be *partial*, when one side of the chest is seen to move less than the other, or when a part of one side moves imperfectly; and this partial respiration may proceed from immobility of the parietes, or, as more usually, from impermeability of the corresponding portions of lung, in consequence of various diseases. Thus, when lymph or tubercular matter in the tissue of the lung, an obstruction of the bronchi, an effusion into, or a contracted adhesion of, the pleura, prevent the inflation and collapse of a part of the lung, the corresponding walls of the chest will be resisted in their motions, and will become fixed in proportion. Thus in phthisical patients we often see the ribs below the clavicles scarcely moving in respiration, and often sunk on one side; in pneumonia and pleurisy the lower ribs are more commonly fixed. Mark further how they are fixed, whether in a state of dilatation, or in one of collapse—whether the affected part remains full after expiration, or whether it is still sunk after inspiration, or whether it is fixed in an intermediate state: you may thus, in certain instances, go some way to distinguish between these different causes of pulmonary obstruction. But more of this by and by.

Examination by tact and vision requires, on the part of the observer, an exact eye, a good perception of shape and symmetry, or, if you will have it phrenologically, full organs of *size, form, and order*. It is often highly useful in the way of a general survey, preparatory to other more accurate modes of examination.

The plan of measuring the chest is a more exact method of detecting inequalities of size between the two sides of the chest. It is generally practised in this way: a piece of tape is fixed with one end on the spinous process of a dorsal vertebra, and carried horizontally to the middle of the sternum, first around one side, then around the other, in this way. [The method was shewn on a plaster figure.] Of course great care must be taken to pass the tape around corresponding parts; and attention must also be paid that the degrees of the respiratory act be the same during the measurement. The most accurate mode is to compare the measurements of the two sides on a full inspiration and a full expiration, as well as in the intermediate state. The process is rather a troublesome one, and we cannot, among private patients at least, use it often; but it sometimes gives results which are well worth this trouble.

Besides external measurement, which is essentially comparative between the two

sides, I may notice here a sort of attempt at internal measurement, by noting the quantity of air that can be *exhaled* or *inhaled*. Mr. Abernethy proposed to judge of the capacity, and consequently the soundness, of a man's lungs, by measuring how much air he could throw at a breath, after a full inspiration, into a jar inverted over water. Here is an apparatus which belongs to Dr. Green, of Great Marlborough Street: it measures the quantity of air taken in at a full inspiration. You see, when I close my nostrils and inspire through the tube, the water in the glass jar rises in proportion to the quantity of air which I draw in, and this is measured by the cubic inch scale on the side of the jar. There are other contrivances of a similar kind. The chief objection to them is that their indications are affected not only by the capacity of the lungs, but also by the strength of the respiratory efforts. They are *dynamometers* for the muscles of respiration, as well as *pulmometers*; and a weak, delicate, or nervous person, with sound lungs, tested by them, would be placed below a phthisical or pleuritic patient whose muscular energies are still considerable.

So much for *seeing* and *feeling*. They may often do something, but you will readily perceive that they will seldom inform us of the nature of the obstructions which they can discover, and they cannot discover many obstructions and disorders of a smaller degree. The chest may be immobile in parts, but whether from blocked-up air tubes, diseased lung, liquid or air in the pleura, or any other of the various causes, sight and touch will not inform us.

We are driven, then, to try another sense which may reach beyond the surface, —the sense of *HEARING*. Now this sense is not generally cultivated as our sense of vision is. "Seeing is believing;" to see a thing is almost the same as to understand it. The same can scarcely be said of hearing; but yet, in cases where we have exercised this sense, we by experience render its indications nearly as instructive as those of sight. Thus we know the different sounds of carriages and horses in the streets, the foot-steps of walkers, the sounds of rain falling, of wind whistling, and so forth; and these several noises are the acoustic signs of the nature or physical character of the various bodies that produce them. So also the acoustic properties of the chest and its organs, the sounds that are produced in them or elicited from them, may by study instruct us as to the nature and condition of the matter of which they are composed.

Now there are two methods of studying these phenomena:—1. By mere individual experience: just as the infant studies the

form and appearance of objects by sight and touch, and in time becomes acquainted with them. 2. Through a generalization of that experience in the laws according to which the phenomena occur. Personal experience is of course indispensably necessary to familiarize us with the character of the phenomena; but before it can inform us of their true nature and causes, experience must be extensive, and detailed to a degree that falls to the lot of very few; and even when thus obtained, its results, without generalization, must be so unwieldy and burdensome to the memory, that still fewer could duly profit by it.

We must avail ourselves of both methods. We must accustom our ears to the sounds in all their varieties, that we may be able by experience to know and distinguish them: but to understand their import, and to read the interpretation which they give of the condition of the parts that produces them, we must study them through the laws of which they are examples. We must consider what sound is; how it may be produced, transmitted, and modified; how the contents of the chest may produce it, and when produced can change it; and by comparing its general properties with the mechanism of the chest and its organs, we shall be prepared to understand and arrange the phenomena that experience has discovered, or may hereafter reveal to us. By thus learning the acoustic relations of the chest, not merely as isolated facts, but as parts of an applied science, we may be enabled to escape in great measure the errors into which unintelligible matters of memory might continually lead us, and we shall be acquiring a rational pathology instead of resting on an empirical diagnosis. In this study, as well as in many of the subsequent parts of the course, you will find my little work on the *Pathology and Diagnosis of Diseases of the Chest*, useful as a text-book; and supposing that to be in your hands, I shall dwell less fully on the matters which are contained in it. In fact, there is so much new matter to be brought before you, or at least so much new application and arrangement of that which is already known, that I shall often refer you to the best works for common details, and give our time chiefly to the development and application of principles which are not, or but imperfectly, to be studied elsewhere.

We must devote the next lecture to a subject purely physical, the nature and properties of Sound*.

LECTURES

ON

RHEUMATISM AND ITS EFFECTS;

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Atmospheric vicissitudes the chief cause of Rheumatism—Effects of Hereditary Predisposition—Relative prevalence according to Sex and Age—Fibrous Tissues the Seat of Rheumatism; supposed continuity of these—Symptoms; pain the most general, but not universal Divisions of Rheumatism—Rheumatic Fever—Seat of the pain—The Inflammation not confined to the Ligaments—Absence of permanent organic change in the parts injured—Nature of the Constitutional Disturbance—Diagnosis between Rheumatic Fever and Gout.

RHEUMATISM, in common with other inflammatory affections, is held to be most rife during the winter; but this, although true in the main, is much less remarkable than with respect to most inflammatory diseases. Dr. Haygarth, who gave considerable attention to the subject, estimates the proportion of cases of rheumatism occurring in summer to those occurring in winter as five to seven; and I may add, that the attacks during warm weather are frequently quite as severe as those which take place at more inclement seasons of the year. In fact, it would appear that the cause of acute rheumatism is not to be sought for so much in any abstract degree of cold as in atmospheric vicissitudes; so that exposure to the cool air of an evening which follows a hot day is often sufficient to produce an attack of acute rheumatism, particularly if the atmosphere has become charged with moisture. Some, indeed, have supposed that other agents besides cold and moisture come into operation, and that malaria gives noxious energy to the influence of the air; but when we consider that rheumatism occurs under almost every variety of situation,—often, certainly, where there is no reason to suppose any malaria to exist,—and when we see it immediately following the application of cold and moisture, without the concurrence of any other apparent circumstance, it appears to me quite unnecessary to have recourse to other causes, the very existence of which is in many cases entirely hypothetical.

But it has further been supposed that, in order to produce rheumatism, some peculiar state of the individual—some constitutional predisposition—was required. In reference to this we may say, that whatever considerably reduces the general tone and

* Erratum in last lecture, p. 88, top of 2d column—for *intense*, read *inclement*.

vigour of the body, renders the individual more liable to rheumatism, but not apparently to a greater degree than with respect to other inflammatory attacks, the constitutional change being merely that of a diminished power of resisting disease in general—not rheumatism in particular. There are, however, two circumstances which enable us to know that one individual is more liable than another to rheumatism: the first is, his being descended of parents who have suffered from the disease, and the second is, when he himself has had it before.

With regard to the hereditary peculiarity which causes this proneness to rheumatism, although we may be at a loss to point out in what it consists, its existence, I think, admits not of doubt: indeed, it is stated by Chomel, as the result of specific investigation on this point, that of a large number of patients treated by him at La Charité for rheumatism, not less than one-half were the offspring of rheumatic parents.

Having suffered from rheumatism once, gives but too great a probability of suffering from it again; and it will rarely be found that an individual who has had rheumatic fever once, lives many years without experiencing its recurrence; so that many persons suffer from the acute form of rheumatism several times in the course of a few years,—while with respect to the chronic, we meet with some subjects in whom this disease in a greater or less degree is scarcely ever entirely absent. I know no certain indication of an individual having what may be called the rheumatic diathesis, except the disease having manifested itself; nor is it easy to conjecture in what that peculiarity in his organization consists which, in the first instance, gives to one man a greater liability than another to become affected with rheumatism. But after it has once displayed itself, it requires no great stretch of imagination to conceive such change to take place either in the physical condition or in the mode of action in the part attacked as shall leave behind it a permanent disposition to resume the rheumatic state,—just as we observe that one who has had cynanche tonsillaris, or almost any other inflammation, is more prone than before to have a recurrence of such disease.

Rheumatism in its acute forms is more prevalent among men than women; but I think the difference in this respect is not greater than the circumstance of the latter being less exposed to its exciting causes is sufficient to explain, without supposing any innate difference in the relative degrees of constitutional predisposition.

The early period of adult age is that at

which the greatest number of cases of rheumatic fever present themselves; and here also it is probable that the circumstances above alluded to,—I mean the greater exposure to the exciting causes,—may have some influence in producing the result. Probably two-thirds of the patients are between 15 and 30; but it is also met with in infancy, and I have repeatedly seen it in children of five or six years of age; while I have still oftener had patients with disease of the heart resulting from acute rheumatism, which had affected them in infancy. As we advance in life, the liability to the disease in the form of rheumatic fever diminishes; and it is comparatively rare after 50. There is, however, a considerable difference as to the frequency of different kinds of rheumatism at different periods of life; and the preceding remarks are to be regarded as most applicable to rheumatic fever, and least so to arthritic rheumatism, which more frequently attacks those beyond the middle period of life than the younger and more robust.

Rheumatism is a disease which has its chief, and some have even supposed its exclusive, seat in the white fibrous membranes; and there can be no doubt but that here, as in other cases, the organization of the part affected has great influence over the phenomena which result. The kind of tissue alluded to occupies a very large extent of surface in the human body: it nearly sheathes the limbs,—it constitutes ligaments which knit the joints together,—it forms sacs, which envelop the brain, the heart, and many of the glands,—it is gathered into the cords called tendons, and these are continuous with the periosteum or fibrous covering of the bones.

It seems to be a favourite conceit with anatomists, that certain textures in one part of the body are in some manner dependent for their formation on a similar texture in some other part; and in conformity with this idea they have endeavoured to trace some common source whence the fibrous tissues derive their origin. Bichat placed their common centre in the periosteum, while Clarus has more recently assumed it to reside in the membrane investing the muscles. Extended as is the “centre” adopted by each of these authorities, great difficulty is found in bringing all the fibrous tissues within the circle; for example, it requires no inconsiderable faith to believe that the sclerotic coat of the eye is but a prolongation of the dura mater, or that the tunica albuginea is but an expansion of certain fibres reaching the testes in the sheathing of its vessels.

But as it has long been an orthodox be-

lief, that inflammation spreads by continuity of surface, so it has been thought of importance to ascertain the route by which external diseases of inflammatory nature sometimes affect internal parts—as rheumatism, for example, attacks the heart. Now the pericardium has been supposed to maintain its connexion with the external parts either by means of the diaphragm, with which it is continuous at its apex, or through a prolongation of fascia covering the great vessels of the neck. This last idea has been especially dwelt upon by Dr. Godman, of Philadelphia*, who informs us, that “however singular it may appear that this arrangement should not have been discovered until this time, it is by no means as singular as that anatomists during so long a time should have remained contented to believe that a *serous* membrane like the pleura could form a strong *fibrous* membrane like the pericardium.” From these expressions it is evident that the learned professor is perfectly “contented to believe” that the bag which contains the heart is dependent for its formation upon the fascia superficialis of the neck. It is curious to observe a very intelligent physician in this country (Dr. Brown, of Sunderland) quoting this supposed connexion as tending to explain the frequency of the affection of the pericardium in rheumatism, although it be notorious to every practitioner who has attended to this disease that the throat, whence the disease is assumed to spread, is but rarely affected with it, while pericarditis is of very frequent occurrence simultaneously with rheumatism of distant parts, and where all idea of its having spread by continuity of surface is entirely out of the question.

But although this attempt to establish the existence of an unity among all the analogous membranes of the body be little more than a mere exercise of the fancy, it is very different when we come to investigate the peculiarities which mark those structures in particular parts, and which give character to their diseases.

Fibrous membranes are simple or compound. The former division includes *ligaments and fascia, aponeurosis, periosteum, and perichondrium*, varieties which differ in thickness and density, but possess the common properties of being fibrous, resistant, white, and more or less resplendent, insensible, having few vessels, and scarcely any nerves which can be demonstrated. Tendons and ligaments are made up of the same tissue, only modified by the arrangement of their fibres into bundles.

The compound fibrous membranes are those which are united with a different

tissue—as the serous, examples of which are presented by the pericardium, the dura mater, and tunica albuginea, and which are therefore called *fibro-serous* membranes. In the nostrils and mouth, the fibrous and mucous tissues are compounded; and in the air-passages and the ear we find an union with cartilage. In the form of inflammation, however, to which I wish more particularly to direct attention, the textures most frequently affected are the fibrous and fibro-serous, the aponeurotic expansions which cover the muscles, the periosteum, and the fibrous covering of the nerves. But the disease undergoes important modifications, according as one or other of these textures happens to be its seat: and in fact rheumatism presents itself under such a variety of different forms, that it is difficult or impossible to give any definition which shall convey an idea of the disease at once comprehensive and correct.

The symptom most general and most dwelt upon is pain; but this may be absent when the case is chronic, and the part at rest. Nay, rheumatic inflammation of the most acute nature may be present without the part affected being complained of, and this, too, though it be constantly in motion; for when the disease attacks the heart, it sometimes runs on to a fatal termination, without any pain having been referred to the chest. In one the disease is transient, in another persistent; in one it endures long, without giving rise to any organic change; in another it speedily produces such change of structure as proves rapidly fatal, or leads to protracted suffering, and more distant but not less certain destruction.

Without for the present taking into account the forms of rheumatism which affect the internal organs, and without including some of the rarer external varieties, we may enumerate the following as well marked, generally distinguishable from each other, and requiring considerable differences in their modes of treatment.

1. The patient may be attacked with pain in one or more joints, with tumefaction and redness, spreading to a greater or less extent over the surrounding parts. The swelling is here external to the joint, the hollows and protuberances about which are obscured, apparently by effusion into the cellular tissue. In this form the disease rapidly shifts its seat, and it is accompanied by acute inflammatory fever.

2. In another case the joints likewise are affected, but in a different manner from the preceding. The pain is more limited, and the swelling evidently depends upon effusion into the capsule, which is seen to bulge at those points where the surrounding ligaments present the least degree of resistance. The bursæ of the

* Anatomical Investigations: Philadelphia, 1824.

tendons are also frequently implicated, and become distended by an increased effusion of their lubricating fluid. In this form of the disease there is less redness, and usually less violent fever, than in the preceding. The great practical distinction between these two forms I believe to have been first made by Dr. Chambers: certainly others, who have not always remembered the original source of their information, were, like myself, first taught to make the distinction systematically, when following his practice at St. George's Hospital.

3. In a third the pain is chiefly referred to the parts intermediate between the joints, and seems to be seated in the muscles or their aponeurotic coverings. Here the pain, though it may be exquisite on the slightest movement, is not unfrequently entirely absent when the parts are quiescent: and here, too, there is often little or no constitutional disturbance.

4. In a fourth variety the disease affects the coverings of certain bones, especially those which are but slightly protected by integuments, such as the shin, the ulna, or the cranium: and under such circumstances there are often spots or patches more painful than the rest, tender to the touch, and elevated into nodes.

5. Lastly, the pain sometimes follows the course of particular nerves, more especially those of the lower extremities, and is occasionally confined to a narrow line, which the patient can trace with his finger. In such cases the power of motion of the limb is occasionally affected to a greater or less extent.

Now although all these be usually classed together under the general appellation of rheumatism, they are affections so different in their phenomena and treatment, that it is impossible for them to be understood either theoretically or practically, unless the distinctions alluded to be borne in mind; and without assuming that the textures specified are exclusively affected, or that the names are altogether free from objection, we shall proceed to speak of them as *Rheumatic Fever*, and the *Athritic*, or *Capular*, the *Muscular*, the *Periosteal*, and the *Neuralgic* forms of rheumatism.

Rheumatic Fever.

Rheumatic fever is a disease exceedingly prevalent among the class of persons admitted into the hospitals of this metropolis; indeed so true is this, that I am satisfied all medical men connected with those institutions will bear me out in the assertion that, if we take into consideration the ulterior effects of acute rheumatism, it is not only one of the most prevalent, but one of the most fatal maladies, incident to our precarious climate. If

this proposition should appear startling to any one, it can only be because he is not fully aware of the prodigious extent to which diseases of the heart, and dropsies, such as baffle all the efforts of our art, may be traced to an attack of acute rheumatism at some former period.

I know no cause which can be confidently pointed out as exciting this form of the disease, except atmospheric vicissitudes operating upon a frame predisposed to receive its influence.

The pain in acute cases is scarcely ever wholly absent, but it varies prodigiously in degree. It is always excited by the slightest movement of the affected parts, as well as by pressure, and is likewise greatly aggravated at night. This has been attributed to increased warmth, and in the chronic form of the disease with apparent justice, as the pain is frequently excited by heat applied during the day; but in acute rheumatism this explanation is insufficient to account for the circumstance; for, in the first place, when a patient is confined to bed, I do not perceive how he is to be warmer at night than during the day (so far as external sources of heat are concerned): and again, it is stated by Mr. Malcolmson, in his recent account of rheumatism in India, that the natives suffer an aggravation of pain at night, although placed under circumstances which render any increase of temperature from without impossible, as their repose is taken in any corner where they can find space to lie down, and without going to bed at all. A simple and more rational explanation of the increase which the rheumatic symptoms undergo at night is to be found in the nocturnal exacerbation which is experienced in this, in common with many other febrile diseases.

Acute rheumatism is generally ushered in by feverishness and wandering pains. These, for the most part, within a few hours, but occasionally not until the end of as many days, become localized. Sometimes one joint is especially attacked—sometimes several. Upon the whole, I think the most frequent order is for the foot and ankle, and then the knee, of one or both extremities, to be affected; next the hands and wrists, and after them, in point of frequency, the shoulders. The elbow is not so often the seat of attack as either the knees or shoulders; and the hip is implicated more rarely than any other of the large joints. It has been supposed that the relative frequency of attack depended upon the comparative degrees of exposure, and that those joints which are most covered suffer least. This explanation may serve to account for the hip-joint so often escaping, but it will not

explain why the elbow should suffer less than the shoulder. Such, however, has been the fact in a very large majority of the cases of acute rheumatism which have fallen under my observation.

The pain is often very severe, and the constitutional disturbance considerable, before the parts swell, or even where no tumefaction occurs at all. But in acute cases some swelling is usually perceived about the joints most complained of, within twenty-four hours. This increases, and the parts become tense; the hollows about the joints are more or less obliterated; the skin is hot, red, and sometimes shining; occasionally there is considerable swelling without redness; occasionally there is a blush of redness, or redness in patches, with little swelling, but generally the degree of tumefaction and redness bear a relation to each other. The part has very much the appearance of a phlegmonous inflammation, but one in which neither pointing nor fluctuation can be perceived.

One of the most remarkable characters of this inflammation is the rapidity with which it changes its seat in external parts. Generally in the course of a few days, and often within twenty-four hours or less, the pain diminishes, or wholly subsides, in the part first attacked, and simultaneously comes on in another; which then proceeds through changes analogous to those above described, to become released from suffering in its turn, while the inflammation either goes back to its original seat, or what is more common, invades some joint which had hitherto escaped. When the pain subsides the swelling does so too; but this always takes place subsequently, although often with great rapidity; so that within a few hours the part which was most tense may become perfectly flaccid, and have but a slight degree of soft cedematous fulness remaining. The skin in such cases becomes shrivelled and wrinkled, but after a time accommodates itself again to the subjacent parts, the cuticle very rarely desquamating, even where the swelling and cutaneous inflammation have appeared most intense.

It is astonishing how much the pain, as regards the limbs, is confined, in the majority of cases, to the joints above mentioned, and their immediate neighbourhood. In a certain number the loins, or the back of the neck, and the parts between the shoulders, become painful, but to a much less severe degree; and here the rheumatism, even when most severe, is usually unaccompanied by appreciable redness or tumefaction.

In all the parts above described as subject to acute rheumatism, there are *ligaments*; and it is probable that they, and the other white dense fibrous tissues, are

the primary seats of the disease. But it is clear that the inflammation is not absolutely confined to them; and indeed we see it in the skin, which is often so red, and with so much effusion beneath it, that it is by experience alone we become aware that suppuration is not impending. It is astonishing how little permanent change remains about the joint, even after repeated attacks, in which respect there is a great difference between this and the synovial, capsular, or arthritic form of rheumatism. Indeed, under ordinary circumstances, no injury whatever is done to the joint in rheumatic fever. Some degree of stiffness may remain for a time; but this seldom fails to wear off very speedily, when the parts to all appearance become perfectly restored. Perhaps about the back of the hand and dorsum of the foot more frequently than elsewhere, a slight degree of thickening may remain, a certain portion of the lymph which was effused having there become organized.

Opportunities very rarely present themselves of examining the joints during the actual existence of acute rheumatism, because patients very seldom die during a first attack; I have, however, in two instances, been able to examine the state of the parts primarily affected, owing to the disease having proved fatal by extending to the heart, but without finding any very striking result. The external redness had disappeared in both cases, and the swelling was very much diminished, so that nothing appeared but a certain portion of serum, or serum and lymph, in the subcutaneous cellular membrane. The ligamentous tissues appeared *perhaps* rather thicker than natural, but without redness; the synovial membrane without any apparent participation in the disease.

In a case of rheumatic fever which recently occurred to M. Chomel, of the Hôtel Dieu, Paris, the patient was rapidly cut off by pericarditis. As there had been severe affection of the joints, with much swelling, during life, Chomel examined them carefully after death, and describing the result he says—*l'examen de toutes les articulations n'a fait découvrir dans aucune d'elles le plus léger vestige d'un travail inflammatoire.*"

The circumstances here are altogether considerably different from those attending the form of rheumatism which I shall next have to describe, and in which the internal structure of the joint is often conspicuously changed. In fact it would appear that, in the form of the disease we are now considering, although the inflammation frequently extends both to the integuments and to the deeper seated structures, yet that it is a kind of inflammatory action but little prone to disorganize

zation; and I believe it is only in cases where the synovial membrane is affected that any permanent injury is done to the articulation, however active the rheumatism may be while it endures.

Besides the parts above enumerated, there are some others which are occasionally affected—probably in their fibrous textures—namely, the scalp, and the parietes of the chest and abdomen. I have placed them in the order of frequency in which they have fallen under my own observation, but without pretending to say that it holds good universally. The affection of none of those parts, however, can be looked upon as conspicuously or peculiarly belonging to *acute* rheumatism, which, in a very large proportion of cases, is entirely confined to the extremities; or if any other part be implicated, it is usually the loins, back, or neck.

When the scalp is affected, it is sometimes tender at different points, so that the slightest pressure causes acute pain; and in a few instances there is slight but perceptible redness, and puffiness of the integuments. In one case which I treated a few years ago, the bridge of the nose became swollen, red, and tender, simultaneously with rheumatism elsewhere, and subsided when this was subdued. Upon the whole, however, the affection of the scalp is much more common in certain chronic forms of the disease, which I shall have to consider hereafter.

The most common form in which rheumatism presents itself about the parietes of the chest, is as a "stitch," generally in the intercostal spaces, and low down on either side, though it may occur at any part of the thorax. Rheumatism in this situation interferes with respiration, the act of raising the ribs causing very acute pain. This kind of rheumatism may therefore be mistaken for pleurisy; but is for the most part readily distinguished by the external tenderness. The least touch is acutely felt in rheumatism, whereas, to increase the pain by pressure in pleurisy, it must be made between the ribs, and pretty firmly. It is also to be distinguished by the partial or complete freedom from suffering when the ribs are fixed, and respiration is carried on by means of the diaphragm—by the circumstance of cough being absent, or at least not necessarily present, in rheumatism, whereas in pleurisy it is rarely wanting—by the degree of constitutional disturbance being much less than in pleurisy, if the rheumatism be confined to the parietes of the chest, while its appearance elsewhere, if it be not so confined, renders any other diagnostic mark almost superfluous.

I have sometimes supposed that the diaphragm was the seat of rheumatism,

from the manner in which the pain darted through from the sternum and edges of the ribs to the back and loins; nor is there any difficulty in believing that this may be the case. Such occurrence, however, is evidently rare, and is rather matter of curiosity than of practical importance, because even if we were pretty well assured of its presence, we could not prudently adopt any other treatment than that adapted to inflammation from any other cause.

Rheumatism of the abdominal muscles in an acute form is principally met with where some peculiar circumstance has led to their unusual exposure to cold. Thus I have seen several instances in which persons riding or driving, with a cold wind, or wind and rain, beating in their face, have suddenly been seized with acute pain across the abdomen; generally, though not always, with rheumatism elsewhere. This pain is greatly aggravated when the abdomen is protruded in inspiration, or by attempting to bring the body into the completely erect posture, and is almost or entirely prevented by tying any thing rather lightly round the belly, so as to limit the action of the muscles. It can scarcely be looked upon as one of the forms of rheumatic fever,—at least, I have never seen it attended with redness or tumefaction, nor with constitutional disturbance, unless there was also rheumatism elsewhere.

I think I have seen rheumatism of the tunica albuginea testis. A gentleman, subject to rheumatism, complained of acute pain in one testicle, coming on suddenly, accompanied by increased heat and by great tenderness to the touch, but without tumefaction; and this again shifting from one testis to the other, and at last disappearing as suddenly as it had come on—just in the manner we see rheumatism change from one joint to another.

The constitutional disturbance which attends acute rheumatism is of a well-marked and striking character. When any of the dense ligamentous structures are in a state of active inflammation, the general system sympathises largely with the suffering part; in fact, it would appear that the less vascular and more insensible the texture, the more ardent is the fever which is lighted up when it has become inflamed. So in the ligamentous form of rheumatism, we meet with as violent specimens of reaction as we are ever called upon to witness. The chilliness or shivering with which this, in common with other acute fevers, is ushered in, having passed away, becomes speedily followed by great heat of skin, with copious but partial perspirations, which are almost invariably acid, rapidly reddening litmus pa-

per, and frequently exceedingly sour to the smell. The pulse increases to 90, 100, or 110, in frequency, and has a peculiar character. In books it is usually stated rather emphatically not to be hard; but I do not think this quite correct. The pulse is large, full, and active; not so hard as the small, concentrated, incompressible pulse of serous inflammation, but often quite as hard as a pulse of such size can well be supposed to become. The heat of skin and activity of the pulse bear a relation to each other, as might be expected; and sometimes both are less exalted than I have above supposed, the heat being moderate and the pulse large, but soft. Such cases usually yield to treatment more speedily than the others.

The tongue, where the fever runs high, becomes deeply loaded, white, and clammy, or even yellowish and dry. No acute disease, except continued fever, exhibits so thick a fur, and the coating of the tongue is even more uniformly present in acute rheumatism than in the common fevers of this country. The appetite is impaired, but generally not so absolutely annihilated as in fevers proper; there is also much less urgency of thirst. The bowels are sluggish and loaded, the evacuations dark and offensive. The urine scanty, generally much, and sometimes prodigiously, loaded with the lithates.

A remarkable difference between this and other forms of fever approaching to it in violence, is the comparative infrequency of what are called nervous symptoms. Delirium is a very rare occurrence; indeed I have never seen it, so long as the rheumatic inflammation has been limited to the external parts, and in many very acute cases there is no headache from first to last.

How long a case of this kind of medium severity might endure if left to itself, I am unable to say; but, with the common methods of treatment, probably five or six weeks may be about the average duration of rheumatic fever. Sir Charles Seudamore says, that "in a case of which the issue is favourable, the fever and pains are brought to a close at the end of the third week, and in slight attacks at an earlier period; but when the course of the disease is untoward, a period of two months scarcely serves to exhaust its power in producing even acute symptoms." This, indeed, is, I believe, in accordance with the generally received opinion, which has led to rheumatic fever being looked upon as in some sort like the eruptive fevers; having a more or less definite course to run, and through which it was the business of the practitioner to endeavour to guide his patient safely, rather than to arrest the disease by any very active interference. So far as I am able to judge, this opinion is not well founded;

and a more correct pathology would induce us rather to class rheumatism among the active inflammations, and one which, like them, may often be speedily extinguished if met at the onset by appropriate means.

When the disease is at its height, it is difficult to imagine a more complete specimen of helpless suffering than the patient presents: he lies motionless as if cased in mail—deprived of that temporary relief which change of posture often affords in ordinary fever. Absolute rest, indeed, seems to mitigate the suffering to a certain extent; the slightest movement of the limbs, whether by a voluntary effort or even the passive motion effected by an attendant, exciting acute pain. But it is only during the day that quiescence brings this partial respite, for towards night the pain becomes aggravated, and continues so till morning. The fever observes corresponding changes, but not, I think, to the same extent as the pain. The exacerbation both of the local and general symptoms appears to me, however, as I have stated, to be only in accordance with that general law of diurnal revolution which we meet with in so many diseases, and not to be dependent on the accidental, and indeed often imaginary, cause assigned for it in the increased warmth of bed.

As the disease is overcome by treatment or the spontaneous changes in the system, all the symptoms progressively, and sometimes very suddenly, abate—the pain first, and then the fever, or rather, perhaps, they subside simultaneously; but I seldom can observe any thing *critical*, in the ordinary acceptance of that term. It is true that the perspiration loses its sourness, and the urine drops its sediment; but when this occurs the pain and swelling of the joints will be found already to have subsided, so that we need not look for other proofs of the disease being already on the decline.

Persons who suffer from rheumatic fever are subject to relapse, especially from stimulating treatment or premature exposure; but with these exceptions the recovery is generally complete where the heart has escaped injury; and so far as my experience goes, such individual is scarcely, if at all, more liable than any other to have the disease in a *chronic* form. There is, however, a subdued form of rheumatism which often follows an acute attack, and which, in reality, is this form of the disease in a mitigated degree. There is pain about the joints which have previously suffered more severely, and which, on but slight exposure to cold, becomes attended with swelling and some degree of fever. In this it differs from the chronic form, which is situated more about the

muscles intermediate between the joints, unattended with any perceptible swelling, and, unless during a severe nocturnal exacerbation, unaccompanied by fever.

I know of no disease with which rheumatic fever can well be confounded, except an attack of gout, or of the capsular form of rheumatism.

The distinction between acute rheumatism and gout may be drawn either from the general or local symptoms; and when they are viewed collectively there cannot, under ordinary circumstances, be the slightest difficulty in drawing our diagnosis. Rheumatism so acute as to incur the risk of being confounded with gout, is almost invariably the obvious result of exposure to cold, and may attack a person previously in perfect health. Gout rarely makes its invasion without premonitory symptoms; as a general rule, it is preceded and accompanied by derangement of stomach, and accordingly, in at least nineteen cases out of twenty, more or less loss of appetite, nausea, acidity, and flatulence, usher in the paroxysm. In rheumatism, as we have seen, the bowels are generally torpid and the evacuations unhealthy, but the stomach does not suffer more than in any other form of inflammatory fever. The fever in gout has a much more marked remission, and bears a more obvious relation to the intensity of the local affection. The period of the first invasion in gout is almost invariably at night; whereas rheumatism may come on at any time. Gout is locally much more partial to particular joints—as, in the first instance, to that of the great toe; and it seldom affects many joints at a time. The swelling is not so diffused as in the acute form of rheumatism, but may be perceived, as it were, to proceed from within the joint, the synovial membrane of which is distended; and when it spreads to the integuments, which it does very rapidly, they assume a deep fiery red, with shining tenseness, and pit on pressure. Lastly, as the fit subsides, the skin itches and the cuticle desquamates: phenomena which scarcely ever occur in rheumatism, however acute.

The character of the suffering, too, is different; but this mark has the disadvantage of being dependent on the description of the patient. In gout the pain is of a gnawing, throbbing, and burning kind, with a peculiar sense of weight. In rheumatism it is acute and lancinating on the slightest motion, and sufficiently difficult to bear, but, unless when the patient moves, it does not equal the torture of acute gout. In rheumatic fever, much of the habitual distress, when the patient is at rest, depends upon the pain being so extensive; in gout it rather depends upon its being so intense.

In rheumatism fever often precedes the local inflammation; in gout, the symptoms which precede the attack are rather of the nervous kind, and the fever follows, or at least is only contemporaneous with, the articular affection. In rheumatism the first attack is usually the most severe as regards the external parts, and the subsequent invasions frequently diminish; at least there is no tendency on their part to increase in severity. In gout, just the reverse is the case; and that often in a marked degree.

The diagnosis between rheumatic fever and acute capsular rheumatism, will be best considered in speaking of the latter.

TREATMENT OF FRACTURES.

To the Editor of the Medical Gazette.

SIR,

IN the year 1833 I published an account of an apparatus for the treatment of fractures, and more especially compound fractures of the lower extremities, which, as I believed, possessed many important advantages over other contrivances for the same purpose. Since that time I have had numerous opportunities of putting to the test the real value of the instrument, and have had ample reason to be well satisfied with the results.

The instrument, which may properly be called a fracture-bed, from the perfect repose which it secures to the fractured limb, is possessed of the following peculiar advantages:—

1. The leg is supported in an easy sling, which admits of being adjusted in the nicest manner as to the actual or relative heights of the knee and foot, in accordance with the feelings of the patient or the views of the surgeon.

2. The reduction of the fractured bones, and their maintenance in the natural position; from which, with a little attention, there can be no hazard of deviation. These indications are effected by means of a screw, acting in the gentlest but most powerful manner upon the foot in fractures of the leg, and the knee in fractures of the thigh, giving the patient but little pain at the time of reduction, and none when that intention has been properly accomplished.

3. In cases of compound fracture of the leg, when the wound is large, and the discharge considerable, the dressings and bandages may be replaced, and the wound perfectly cleansed, without any

interference with the position of the limb, and with scarcely any uneasiness; certainly not more than would attend the dressing of a wound of equal magnitude and sensibility, unconnected with a fractured bone.

4. In consequence of the sling in which the leg is suspended, the patient is enabled, without hazard of displacement, to move the pelvis to a certain extent for the purpose of relieving the irksomeness of a long-continued unvarying position, or while using the bed-pan.

5. In fractures of the upper portion of the thigh, near the great trochanter, or of the neck of the femur, whether external to, or within the capsular ligament, a very perfect degree of coaptation may be secured, so as to preclude any shortening of the limb or permanent lameness. This is effected by the action of the screw on the back part of the fracture bed, until it is made to reach from the ham to the tuberosity of the ischium, while the external part is extended in a similar manner to the upper extremity of the femur, the pelvis having been fixed by a strap passed between the legs, so as to embrace the ramus ischii and pubis, and attached to the iron loop at the upper end of the instrument. In some cases greater security may be obtained by a second strap passed round the pelvis.

To detail the cases which have proved to me, in the most satisfactory manner, that these advantages are really to be obtained by means of this fracture bed, would prolong this communication beyond my present intention, but they have been witnessed and acknowledged by many intelligent surgeons; and I may add, that, in passing through this place a few weeks ago, Sir Astley Cooper had an opportunity, when inspecting our Infirmary, of witnessing its use in a case of compound fracture of the leg, of a very bad description, when he expressed his approbation of its operation. In several cases of this kind, attended with extensive lacerated wounds, and wherein exfoliation must necessarily ensue in the course of recovery, in consequence of the bone being for some extent denuded of periosteum, cures have been effected, without deformity or shortening of the limbs, which, I apprehend, could scarcely have been accomplished without the assistance of such an instrument; and the degree of ease and freedom from anxiety with

which the treatment has been conducted, is well worthy of being taken into account. A severe case of this character is at present far advanced towards recovery in this Infirmary, in which the original wound was not less than six inches in extent, and the bone, denuded of periosteum, was very considerably protruded.

Since I formerly requested the attention of surgeons to the facilities afforded by this instrument, I have made several alterations in its construction, which have had the double advantage of improving and simplifying its application, and materially diminishing its cost. These I shall endeavour shortly to explain.

1. Wood, with brass screws and mountings, has been substituted for iron in the construction of the instrument.

2. The paddings have been dispensed with, and their place is supplied by a small pillow, reaching from the ischium to the bend of the knee.

3. The bag for supporting the leg, with its numerous straps, and the studs to which they were suspended, has also been dispensed with; and, in dressing, its place is advantageously supplied by about four strips of bandages, passed in succession from one of the parallel bars, which extend from the knee to the foot-piece, under the leg, and tied at the opposite side, so as to give the most comfortable support to this part of the limb. A small narrow pillow may be interposed between these strips of bandage and the leg; but this is not essential. When the wound, in cases of compound fracture, is dressed, strips of bandage are passed round it in succession, so as to answer the purpose of a many-tailed bandage, before the suspensory strips, already alluded to, are passed from one parallel bar to the other, in the way described above. All this is done with the greatest ease, and in a very short space of time.

4. The key, which was formerly used in extending the foot-piece, is no longer needed. This action is now effected by means of a female screw, which moves on the cross bar, through which the screw of the foot-piece passes. In the thigh-piece the same object is effected, by means of small moveable bolts in the manner of the bolt which moves the screw of a vice.

5. A nicely-constructed hinge renders the knee of the instrument flexible,

the degree of flexion being determined by the height to which the sling is raised, which admits of perfect regulation in conformity with the wishes of the surgeon.

6. The support for the sling is made to resemble the frame-work of a stool inverted, the supporting leather straps being fixed to the extremity of each leg, either by means of a screw or a kind of buckle, well adapted for raising or depressing the sling, either at the knee or foot, as circumstances may require.

The difficulty of conveying in words what a glance at the instrument would enable the observer to comprehend almost in an instant, will, I fear, render this description not very distinct; and I can scarcely hope that drawings, to illustrate my meaning, would be admitted into your pages. Should I be incorrect in this supposition I shall gladly furnish you with sketches which will make the descriptions easily understood; and, desirous as I am of having the instrument tried by other practitioners, from a conviction of its practical utility, it will give me pleasure to supply any information which I have failed to introduce into this communication.

I may add, that the instrument, in its improved form, has been well constructed by Mr. Brown, an instrument maker of this place, on terms which place it more within reach for practical purposes.—I am, sir,

Your obedient servant,

T. M. GREENHOW.

Newcastle-upon-Tyne.

October 9th, 1837.

[Any drawing which can be represented by a wood engraving, we shall be happy to receive.—Ed. Gaz.]

ON THE INDUCTION OF PREMATURE LABOUR.

To the Editor of the Medical Gazette.

SIR,

If the following case possess sufficient interest to be worthy of introduction into your useful and scientific journal, you will, by its insertion, oblige

Your obedient servant,

EDWARD AUGUSTUS CORY,
Surgeon to the East London
Lying-in Institution, &c.

Cannon Street Road,
St. George's in the East, Oct. 16, 1837.

Mrs. H., of short stature, and aged about 30, had twice undergone the operation of embryotomy. I attended her, for the first time, about three years since, when the same operation was again deemed necessary, and was performed in the presence of a most respectable practitioner. The pelvic deformity was of the reniform character, the space between the sacro-vertebral angle and symphysis pubis (conjugate diameter,) being about two inches and three-fourths. It was consequently determined, should the recurrence of pregnancy render it necessary, that the premature induction of parturition, at the seventh month of utero-gestation, should be had recourse to, as affording the only means of saving the infant from the murderous application of the perforator. She had again become pregnant, and it was calculated that she had arrived at the seventh month of her pregnancy, about Thursday, the 14th of September last. From some remarks and cases which have been recently published by Dr. F. Ramsbotham, it appears that he has succeeded in effecting the induction of premature labour, "*solum ope secalis cornuti*." I was, therefore, led to employ this substance according to the formula he has suggested, viz.

R Secal. Cornuti ʒij.; Aq. Bullient.
ʒviij. infunde per semihoram, et addo
Acid. Sulph. Dil. ʒij. Syr. Simple.
ʒij. Tr. Card. Co. ʒij., ut ft. Mist.
cujus cap. cochl. ij. mag. q. quartā
horā.

The first dose of this mixture was ordered at 2 p. m., on the 14th of September. At 6 p. m., soon after the administration of the second dose, the uterine energy became slightly excited; and it was interesting, as well as satisfactory, to observe its gradual increase soon after the repetition of each dose of the medicine. On the next day (Friday) at 1 p. m., the parturient pains were tolerably active, but at considerable intervals. A vaginal examination was instituted, and the membranes were distinctly felt pressing against the dilated os uteri. Saturday, at 11 a. m. the pains had gradually diminished in force and frequency since my last visit, and she had experienced no pain from yesterday at 4 o'clock p. m. to the present time, and was, to use her own expression, "quite well again." The institution of another vaginal ex-

amination- demonstrated that the os uteri had not, in the least degree, increased in dilatation, and that the pressure of the membranes which had been previously experienced, even during the interval of pain, had now entirely subsided. She expressed great anxiety and apprehension as to the result.

On the same day, about 6 p.m., I again visited her, and found her precisely in the same situation. I was fearful of repeating the *secale cornutum*, lest it might destroy the infant. I therefore thought it most prudent to rupture the membranes, the distension of which had now completely subsided; and this, having converted the nail of the index finger into a saw, I accomplished with some little trouble.

She was again visited on Sunday, at a little after 1 p.m. She remained in a similar condition, and there had been no accession of the pains of parturition.

On Monday she had not yet experienced any pain, and the bowels being in a constipated state, I thought it prudent to prescribe an aloetic purgative, with a carminative addition, which had the effect of thoroughly evacuating the bowels, and exciting the uterus to action; so that early on Tuesday morning the pains of labour commenced with considerable activity, and continued with but slight intermission until six o'clock in the evening, when she was delivered of a living infant of healthy appearance. The foetal head, notwithstanding the severity of the parturient paroxysms, occupied several hours in its passage through the contracted pelvis, and, after expulsion, presented on its lateral portion an evident indentation, and was also considerably flattened. The whole process terminated as in a common accouchement. The placenta was expelled, with but little assistance, about half an hour after the birth of the infant, which was restored in ten minutes by immersion in the warm bath, and by artificial respiration. The child remains at the present time (Oct. 16) healthy and vigorous, takes the breast freely, and there is every probability that it will continue to live. The patient has not suffered the least pain or inconvenience since her delivery; in short, I never witnessed a more rapid recovery even in an ordinary case.

REMARKS.—The necessity for the induction of premature labour in the present instance must, I think, be evident to every well-informed obstetrician; and I also am of humble opinion, that the means so carefully adopted for the production of so desirable an object were based upon the soundest principles of obstetrical science. I am induced to believe, that had I continued the administration of the *secale cornutum* for a longer period, that the expulsion of the infant might most probably have been effected without the necessity of the least manual interference; but I was deterred from persevering in its use from the apprehension that it might exert a deleterious effect upon the infant, and also injure the mother. I was fully aware of the great importance of maintaining the membranes entire as long as possible, in order to be able with greater certainty to insure the safety of the infant; but as the *secale* failed to produce the anticipated result, I was reduced to the alternative of rupturing the membranes, even with some risk to the infant, rather than hazard, perhaps, irretrievable injury both to the mother and child, by persevering in the administration of the *ergot*. The induction of premature labour appears to have been practised by the ancient physicians, more particularly by *Ætius* and *Paulus Egineta*, who recommended it in cases of extreme contraction of the pelvis; but it was not until about the middle of the last century that the most eminent practitioners in London decided on its propriety and morality. It may be laid down as an incontrovertible obstetrical axiom, that if there be less than the space of three inches, and more than two and a half, between the sacral promontory and the pubes, that the induction of parturition at the seventh month of utero-gestation becomes indispensably necessary, and its utility will be rendered more evident when we consider the disproportion between a structure thus constituted, and the foetal head at the full period of intra-uterine maturity. From accurate and multiplied observations, *Madame Lachapelle* has arrived at the conclusion that the biparietal diameter of the foetal head at the seventh month of pregnancy does not measure more than three inches, and sometimes even less; and, therefore, allowing for its compressibility in

consequence of incomplete ossification, it may be easily imagined that no very considerable difficulty will be experienced in its passage through such a pelvis as I have mentioned. The records of the science prove most satisfactorily that the woman is not subjected to greater risk by premature labour induced artificially, when carefully performed, than by spontaneous parturition at the full period of gestation. The existence of some morbid affection, rupture of the uterus, or some accident entirely independent of premature delivery, has been invariably discovered in those cases which have had a fatal termination. Denman operated eight times with complete success (*Introd. to Midwifery*, vol. ii. p. 224). M. Salomon mentions sixty-seven, Kluge twelve, and Ferrario six, which also terminated successfully (*Journal Compl. des Sc. Méd.* §c. tome xxxiv. p. 339). In the practice of Reisinger (*Dict. de Méd.* 2d edit. tome i. p. 429), one died in fourteen; but Merriman (*Synopsis of Difficult Parturition*, §c. p. 161) has not lost one in forty-six upon whom he appears to have operated.

Artificial premature delivery does not, however, terminate so happily with regard to the infant. In forty-seven cases which occurred in the practice of Merriman (*Synopsis*, §c. p. 180), twenty-six were dead, five were born living, but not possessed of viability, and sixteen lived. Hamilton has been more fortunate, and in twenty-seven cases has succeeded in preserving the lives of twenty-three (*Ryan's Manual*); Ferrario, five in six; Kluge, nine in twelve; Salomon, thirty-four in sixty-seven; and Burekhard (*Thesis, Strasburg*, July 20, 1830), thirty-five in fifty-two. Premature delivery has also been recommended in cases entirely unconnected with pelvic distortion. Its performance has been proposed by Mai, Ritgen, and Carus, in those instances where the fetus habitually dies some time before the expiration of the full period of gestation, as well as in some diseases induced by pregnancy, which are dangerous to the mother, as metrorrhagies, retroversion, &c. Siebold, according to Kilian (*Die Operative Geburtshilfe*, vol. i. p. 380), practises it in ascites and hydrothorax, and M. Costa (*Revue Médicale*, 1827, tome i. p. 343), thinks it necessary in diseases of the heart. Conquest (*Outlines of Mid-*

wifery), Ingleby in his work on Uterine Hæmorrhage, Busch (*Lehrbuch der Geburtskunde*, 2d edit., 1833), and other authorities, have also recommended it in cases entirely independent of pelvic distortion. My limits will not permit me to discuss the propriety of its adoption in the various morbid conditions just alluded to, but it is evidently the only rational means of relieving the woman who has the misfortune to be affected with diminution of the natural dimensions of the pelvis, and of rescuing her infant from inevitable destruction.

The operative methods which have been proposed and practised for the induction of premature labour are very numerous, but the one most usually had recourse to, and upon which the greatest reliance can be placed, is the sudden evacuation of the liquor amnii, either manually or instrumentally. Some writers of celebrity have advised its gradual discharge, but the majority have decided in favour of the former, as by the sudden vacuity of the uterus that organ is more likely to take on a brisk parturient action, by which means there will be a greater probability of saving the life of the infant. I consider, also, that there can be no objection to the previous administration of the ergot, according to the formula, and on the respectable authority, of Dr. Ramsbotham; and in some cases I am inclined to believe it may (*per se*) produce the desired effect. I may add, in conclusion, another mode of procedure, which appears to be in high repute with the practitioners of the French school, although I cannot recommend it on my own personal experience. Velpéau (*Traité Complet de Part des Accouchemens*, tome ii. p. 413), thus mentions it. "La dilatation au moyen d'un morceau d'éponge, comme la imaginé M. Kluge, est d'un effet beaucoup plus certain. L'irritation qu'en résulte est permanente, progressive, régulière, et soutenue par la pression qu'exerce l'espèce de tampon qu'on maintient en même temps dans le vagin. Sous l'influence d'une pareille excitation, la matrice entre bientôt en action, et il est difficile que le travail n'acquière pas rapidement une énergie suffisante."

ON THE EDUCATION OF GENERAL PRACTITIONERS.

To the Editor of the Medical Gazette.

SIR,

IN my letter of the 7th of August, inserted in your number for the 19th, I offered some suggestions, by which, as it appeared to me, a more equal and certain acquisition of knowledge would be attainable by students, without any increase of expense.

I took a sketch of the wants of the public in medical and surgical affairs, on ordinary occasions, ventured to make a comparison between the condition of gentlemen educated to the other learned professions and "general practitioners," and submitted for consideration whether a high standard of qualification, based on an expensive education, might not lead to disappointment, tend to abridge occupation, and eventually become injurious to the public and the profession. Two gentlemen have noticed my remarks, Mr. Meade and Mr. Norton. The former thinks that my plan "of exacting a more regular attendance upon lectures" seems "only likely to aggravate, rather than relieve the evil." Now I must confess it is paradoxical, that the evil, viz. defective knowledge, should be increased, or aggravated, by a steady and systematic, instead of an uncertain and irregular, attendance at the sources of elementary instruction, unless Mr. Meade could shew that all pupils who had so absented themselves would have actually employed their time, during absence, more advantageously. Mr. Meade then jocosely observes, "I do not know whether Dr. Kerrison could devise a plan by which it might be known, whether men listened to, as well as were present at, the lectures." To this I reply, that I can neither confer intellect, nor command attention; but that I have sufficient confidence in the good sense of medical students to believe that they could understand the lecturer, and that they would *not* be so wilfully perverse as to disregard the subject of the lecture; therefore I see no reason to doubt that they will become better acquainted with the respective departments of science, when their attendance shall have been made imperative, instead of remaining optional.

The other part of Mr. Meade's communication is no answer to my letter; it merely decries apprenticeships, and extols the system of private teaching by junior lecturers.

With respect to apprenticeship, Mr. Houlton's letter, in the *GAZETTE* of the 9th of last month, presents so forcible and just an appeal to the understanding, that I am inclined to think Mr. Meade had not been fully aware of the manner in which it can be made useful in training the mind of a youth to the details of his future professional duties, as an apothecary, or a "general practitioner."

As to the other point, I am not disposed to deny the advantage of a double system of teaching, where pupils can afford to pay for it; but it has been often remarked, that the expense of a medical education is already great; and it appears to me more consistent with the dictates of reason to ascertain what improvement can be effected by a more regular attendance at the public classes, than to connive at, or even to permit, the present negligent mode to prevail, and to force young men upon the system of *grinding* and *cramping*, in order to get up a technical smattering, for the purpose of passing their examinations.

I know that much diversity of opinion exists respecting medical education, and that some think a "general practitioner" ought to be a profound anatomist, and an expert operator—capable of demonstrating the minutest parts of the body, of crushing a stone in the bladder, or of extracting a cataract, besides being equal in medical affairs to the most able physician.

Mr. Norton has commented on a detached part of my observations. He thinks "that the education of a general practitioner should be of the highest stamp, as he is in most cases the first called in, and is *expected* to be able to treat, competently, every case."

I will not attempt to deny that practitioners of the highest stamp are best able to fulfil the expectations of the public; for I have already stated that there are many gentlemen engaged in pharmacy possessing very high qualifications; but I have endeavoured to distinguish between that which seemed practicable, as to the general rule, and that which was possible, as the exception. Let us, then, take a review of professional education as it now exists,

and has existed during the last twenty-two years, or since the Apothecaries' Act came into operation, and as it used to be pursued by the respectable class of pupils long before that time*. Let us look steadily at its results, and then acknowledge frankly whether such expectations can or cannot be realised.

A few young men commence their scientific and hospital studies, who are sons of gentlemen in practice; their preliminary education has been good, and they are well prepared to imbibe knowledge: a few others have been, or still are, apprenticed to general practitioners in London, who are not indifferent to the advancement of their pupils, and permit them to proceed with this part of their education during the latter years of their apprenticeship, in the manner explained by Mr. Houlton. These two classes are not numerous; their peculiar condition leads to the expectation under ordinary diligence of high attainments.

The great majority of students, however, arrive in the metropolis from provincial districts, where no lectures had been given, or they have been apprenticed in London to masters less able, or less willing, to allow them to attend lectures and an hospital; or, at most, may have permitted a partial attendance, under limitations which nearly destroyed all beneficial result.

Such young men constitute, probably, eight or nine-tenths of the whole number of students; they come to London for a limited period (called walking the hospitals), and are often undecided to what medical school they shall attach themselves. In this age of extraordinary competition of teachers, they are not always fortunate in their choice. Those who are not resident with parents, relatives, or masters, get into lodgings in the neighbourhood of their lectures, or board in a family, during the few months of their sojourn in the

metropolis: their pecuniary means are often slender, and no extension of time is in general allowed beyond that which is absolutely necessary to complete the course of study (called curriculum), to qualify them as candidates for examination at the Royal College of Surgeons, and at Apothecaries' Hall.

Several hundred young men, thus annually thrown upon a sort of college life, with few restraints and no regular superintendence, meet in the classrooms, and soon form associations and acquaintance with each other. Assuming that they all possess good principles, they are nevertheless of various dispositions, and may be subdivided into two general classes—the industrious and the idle—with all gradations of both.

The industrious learn as much as they can, scramble through their every-day duties as well as they may, and in their meritorious endeavours to excel, often become indisposed by attempting to do too much, and by having led a sedentary life in the atmosphere of a crowded part of the town and a dissecting-room. Many young men of this class, particularly those from the country, are compelled to pause, and sometimes to return home to recruit their health, before the medical session (from October to June or July) has expired; and they lose, in this manner, part of their proposed education in the first year.

They return to London in the autumn, to complete the curriculum of study in new subjects, having an imperfect acquaintance with those which they had been forced to abandon; and these industrious students incurred the danger of being rejected at their examinations, unless an extension of time had been obtained to enable them to proceed with greater certainty of success. In the autumn of 1835, the period of attendance at lectures and an hospital, to qualify a candidate for examination at Apothecaries' Hall, was extended to three winters and two summers*.

In addition to these, the student who intends to become a "general practitioner" has to make himself sufficiently acquainted with the principles and practice of surgery, to be qualified

* The proof that a good medical education was attainable before 1815 may be taken from the fact that there were numerous practitioners in pharmacy at that time capable of forming a Board, and that many of the present examiners must have been educated prior to 1815. The lapse of nearly a quarter of a century has brought others into that situation, and the progress of science may be expected to have rendered them (*ceteris paribus*) superior to their predecessors. I hope that my remarks, so far as they may seem to have reference to gentlemen in the responsible station of examiners, may be received as from *Amicus Curia*, who wishes to see their zeal for improvement tempered by discretion.

* A similar mode of proceeding exists in those cities and large towns of England where courses of medical, surgical, and hospital studies have been lately established, and recognized as capable of conferring qualifications for examinations.

for an examination at the College of Surgeons.

The less industrious and the idle soon begin to neglect some of their studies: finding the disposal of their time uncontrolled, and their attendance at lectures optional, instead of imperative, they gradually withdraw themselves from close attention to any one subject, or perhaps limit it to one or two subjects, flattering themselves with the hope to supply, at some future time, all deficiencies by the aid of a tutor (called grinder) and a printed book of such questions, with answers to those questions, which, as they are informed by interested persons, will be quite sufficient to complete their education, and enable them to accomplish the object of all their hopes and fears.

Admitting that some young men of indolent habits have in this manner, and by a strong exercise of memory, passed through their examinations after a desultory mode of study, having too slender an acquaintance with first principles, they would soon find, and unhappily discover it too late, that the power of retaining and of applying knowledge *so obtained*, is very inferior to that possessed by those of their own school who had acquired their information by diligent attention to a regular course of study; and that their chances of success in the severe duties of professional life would be dissimilar, and bear, I fear, too close a resemblance (allowing for difference of station) to those of Hogarth's industrious and idle apprentice.

I believe this to be a faithful picture of the professional education of students who are destined to become general practitioners in town or country; by which it will be seen that they possess the opportunity of learning enough under the present routine to enable them to fulfil their obligations to society in a respectable manner; but that being obliged to study so many things, and usually doing so within a limited time, the public cannot reasonably expect their education to be "of the highest stamp" in all the following branches, namely, in "chemistry, anatomy, and physiology; anatomical demonstrations, *materia medica*, and "therapeutics; botany, and such other branches of study as may improve the student's general education; dissections; principles and practice of medicine; medical

practice of an hospital; midwifery and diseases of women and children; forensic medicine; midwifery, with attendance on cases*." In addition to these, the student who intends to become a "general practitioner" has to make himself sufficiently acquainted with the principles and practice of surgery, to be qualified for an examination at the College of Surgeons.

It cannot be denied, that most young men who are designed for the higher department of surgery (consulting surgeons) take rather a different course. They are, generally, apprenticed to hospital surgeons, devote a much longer time to practical anatomy and operative surgery, and thus qualify themselves for their future duties. As it is in this manner that such men as Cooper, Brodie, Lawrence, Keate, Earle, Guthrie, Mayo, and other hospital surgeons, have been educated, I think that, on reconsideration, Mr. Norton will have the candour to allow, that the expression, "education of the highest stamp," should be appropriated to practitioners of that class.

It is also customary, I believe, for the medical student, who intends to become a member of the Royal College of Physicians in London, either to enlarge the sphere of his studies, or to devote a much longer time to them before commencing practice, than those young gentlemen who are to be "general practitioners;" but it would be indecorous to carry this parallel to the department in which I am personally engaged.

I trust, then, it will appear that my positions are fairly sustained: first, that a real improvement would be effected in medical education, by the same regularity of attendance at lectures as is required and insisted on, in elementary instruction, at other schools; secondly, that the consulting or hospital surgeon, from the greater extent of his professional education and habitual dexterity, acquired by practice, must be considered as the referee in all important cases of surgery; and that it is probable a physician may not be without the power of contributing to the welfare of society in other cases, where doubts or difficulties occur.

* See printed regulations to be observed by students intending to qualify themselves to practise as Apothecaries in England and Wales, 1835.

Gentlemen already in practice in provincial districts, should lose no time in acting upon the suggestions of Mr. Hill, of Wotton-under-Edge, inserted in the *GAZETTE* of the 7th instant, in order to obtain a just reward for public professional services; or, in the politico-economical phrase of the day, for "the wages of labour." Benevolent and enduring as "general practitioners" usually are, they have families dependent on their personal exertions. They have a right to expect an equitable remuneration for the quantity and quality of professional labour required. They should not submit to be insulted and brow-beaten by such persons as are mentioned by Mr. Hill. I may probably trouble you, at some future time, with a few words on that subject, and a hint at the remedy.

I beg to add, in conclusion, that it is my opinion much benefit has been conferred on the public by the Apothecaries' Act of 1815. The present practitioners in town and country are more equal than before examinations were instituted. Such disparity, however, is found as will arise from superior aptitude, and closer attention to their duties; and this is seen in other professions; but they are all educated to an useful extent, and worthy of public confidence.

I am, sir,

Your obedient servant,

ROBERT MASTERS KERRISON.

12, New Burlington-street,
Oct. 13, 1837.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abrégé."—D'ALEMBERT.

NOTICES OF SIX ELEMENTARY WORKS.

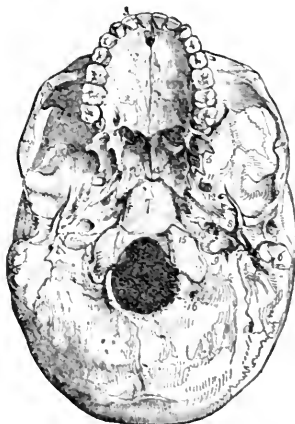
Elements of Anatomy. By JONES QUAIN, M.D. 4th edition, revised and enlarged, illustrated with steel plates, and numerous engravings on wood. Part I., price 12s. Taylor and Walton. 1837.

A Series of Anatomical Plates in Lithography, with References and Physiological Comments, illustrating the Structure of the different parts of the Human Body. Edited by JONES QUAIN, M.D., and W. T. ERASMUS WILSON. Division III. Nerves III. Oct. 1837.

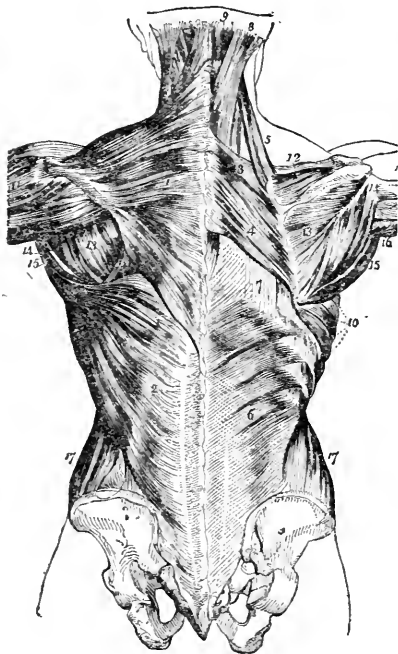
THE great novelty, and, we may add,

improvement in the present edition, consists in the illustrative engravings. Our readers may possibly remember the commendation we bestowed, some two or three years ago, on the work of Mr. Paxton, in consequence of the excellent manner in which the subjects were illustrated by woodcuts introduced into the pages, together with the descriptions, so that there was no turning over of leaves, no searching for the corresponding engraving, but the letter-press and the figure met the eye on the same page. A similar plan is now adopted by Dr. Quain; and as it is admirably adapted to facilitate the study, we have no doubt of the present edition proving a very popular one. When a fifth edition is required, we hope he will adorn it with a still larger supply of engravings; and we take the liberty of suggesting the subject of Hernia as one which might be thus simplified with great advantage to the student. The extent to which the art of engraving on wood has been improved of late years cannot be unknown to the readers of this journal, many of the specimens which we have laid before them being executed in the very best style. The advantage in such works as Dr. Quain's is prodigious; and the facility of working the blocks along with the common types, renders them in every way convenient.

The adjacent specimens taken from the work before us, will enable the reader to judge better than any verbal description. Such a figure as that representing the base of the skull, minute as it is, being placed beside the description,



greatly lessens the student's labour. And the same remark applies to the complex piece of anatomy presented by the muscles of the back, so well displayed in miniature in the other figure.



In the second part, which will complete the volume, we are promised two steel plates, and two plates of the brain. We are not convinced that the anatomy of the brain itself might not be perfectly well displayed by woodcuts; so^{*} amazingly has the skill of some of our artists improved within the last few years*.

The valuable series of Plates undertaken by Dr. Quain also makes steady progress; they continue to merit the commendations formerly bestowed upon them, and promise to form a very complete system of human anatomy. The present part contains two views of the brain.

* It is scarcely worth noticing, perhaps, but in the last and present editions (we have not the others by us to refer to) Dr. Quain, quoting Ovid, makes him say, "Natus est homo," &c., which we cannot scan. It ought to be, "Natus homo est," &c.

The Edinburgh Dissector; or, System of Practical Anatomy, for the Use of Students in the Dissecting-room. By a Fellow of the Royal College of Surgeons in Edinburgh.

STUDENTS will find this to be an exceedingly useful manual. The author (whom we conclude to be the translator into English of Cloquet's Anatomy) informs us, that he has lectured on anatomy for many years; and he is evidently thoroughly familiar with his subject. The first part of the work, comprehending the bones and their articulations, we have particularly examined, and have found to be very clearly and distinctly written. We do not remember having formerly met with the fact, thus stated by the author:—"We have for many years been in the habit of exhibiting the costal cartilages in their dry state, and then shewing the student, that by placing them for a short time in water (a day or two in winter), they will regain all the properties which they possess in the recent subject; and this may be done again and again with the same cartilage."

There are some splenetic remarks at the beginning of the volume, which appear to be misplaced.

We may add, that a good glossary is appended.

Elements of Chemistry; including the recent Discoveries and Doctrines of the Science. By the late EDWARD TURNER, M.D. Sixth Edition, enlarged and revised, by Professor LIEBIG and WILTON G. TURNER. Part I., price 7s. 1837.

THE very high place which the former editions of this volume had taken among the elementary works on chemistry, is a sufficient proof of its merits. Much of the disadvantage which was to be apprehended from the decease of the distinguished author has fortunately been obviated by the arrangements made by his brother, Mr. Wilton Turner. The portion relating to inorganic chemistry, indeed, we are told, was prepared for the press by Dr. Turner before his death; while, in other parts, the present editor has followed out various improvements contemplated by the original author. But the great feature in the present edition is the association of Pro-

fessor Liebig in the task of preparing it. This distinguished chemist enjoys a well-earned reputation for his labours and discoveries in the department of organic chemistry; and this portion of the volume, which we are informed is to be entirely new, has accordingly been intrusted to him. The period at which the different portions are to appear, has been arranged so as to be adapted to the convenience of students attending the winter courses of lectures.

Cyclopædia of Anatomy and Physiology. Edited by ROBERT B. TODD, M.D. Parts X. XI. and XII.

THIS work loses nothing of its interest as it proceeds, and its merits continue such as we have formerly described them. (See our notice, May 14, 1836.)

In the first of the parts which lie before us we have an interesting article on Endosmose, from the pen of Dutrochet, and a very valuable one on Entozoa, from Mr. Owen; while the anatomy of the Extremity, and of the Eye, is minutely and elaborately given—the former by Dr. Todd, and the latter by Dr. Jacob. In Part XI is contained the chemical history of Fat and Fibrin, by Mr. Brande; and interesting articles on Fibrous Tissue, by Mr. Grainger; and on the Fifth Pair of Nerves, by Dr. Alcock. In Part XII. the most important essays are, that on the Fœtus, by Dr. Montgomery; on the Foot, by Dr. Todd and Mr. Dod; while Mr. Grainger contributes a paper on Ganglion.

The whole of the subjects treated of are illustrated by engravings on wood; and if the publishers do not fall into the too common error of extending the number of parts so as to augment the bulk and increase the expense, the work will occupy a place in the libraries of all the literary members of our profession.

Elements of Physiology. By J. MÜLLER, M.D. Translated from the German, with Notes, by WM. BALY, M.R.C.S., &c. Part I.

THE name of Müller has for some years been known to the English reader with progressively increasing reputation. We have ourselves given many of his views and opinions to the world, and some of them coming direct from Berlin, with the advantage of his own re-

vision. Müller is decidedly the great physiologist of the day; and his precise descriptions and methodical ratiocination leave the inconsequential vivisections of Magendie at an immeasurable distance. Mr. Baly, by whom the present treatise has now been executed, is himself a graduate of Berlin, and appears fully adequate to the task he has undertaken. We have looked over various portions of the brochure before us, and find the translation to be marked by clearness and simplicity. It is to consist of three parts; and we are promised the second on the 1st of January. If the two which are to come be executed in a manner as creditable as the first, the work will necessarily become a standard one in this country.

MEDICAL GAZETTE.

Saturday, October 21, 1837.

“Licet omnibus, licet etiam mihi, dignitatem Artis Medicæ tueri; polestas modo veniendi in publicum sit, dicendi periculum non recuso.”

CICERO.

REGULATION OF LUNATIC ASYLUMS.

THE parliamentary campaign is close at hand; in less than a month the combatants will be drawn up in battle array, and so equally are the numbers balanced, that the most sagacious are unable to foretell on which side will be the triumph—*inter utrumque volut Victoria pennis*. The only parliamentary questions, however, which come under our cognizance, are fortunately those which belong not to party nor to faction, but to the great cause of the human race; where, in casting up our votes and anticipating the probabilities of success, we do not ask if A is a whig or a tory, but simply whether he is an honest man. Such topics are military flogging, the starving experiments conducted by our Poor-Law dictators, and the subject of lunatic asylums. From a discussion which took place in the House of Commons shortly before the close of the last

session, it seems probable that lunatic asylums will engage the attention which they deserve during the ensuing one. It appeared that, although the Middlesex Lunatic Asylum has been much and deservedly praised for the excellence of its arrangements and the philanthropic zeal with which it is conducted, the proportion of cures has not been so great as might have been expected. This alone would require some investigation, but other parts of this subject demand a more diligent and minute inquiry; for example, the management of private asylums, and the condition of pauper lunatics in those counties where there are no county asylums, and to these points we are most anxious to direct attention.

Before taking a view of things as they are, let us cast a glance upon them as they were; for in considering the defects of an institution or a system, it is by no means useless to recapitulate what were its acknowledged faults before their partial reformation: in the first place, we thus obtain a full view of the real truth as it existed, plain and unvarnished, before the defenders of the system had learned to gloss it over; and in the second place, we see the extremes which the vices of an institution may reach, when unchecked by the salutary fear of public opinion. Thus, in studying the question of slavery, it is instructive to read the accounts given by eye-witnesses, of its practical working before abolition or mitigation had been agitated. When we read, for instance, in Stedman's Account of Surinam, of the cruelties which he censures, indeed, but of which he does not anticipate the termination, we see to what excesses unlimited power naturally leads, while the disputed atrocities of 1830 are made but too probable by the undisputed ones of 1772*.

Again, if our readers would know into what ignorant barbarities the keepers of lunatic asylums might easily fall, if the most rigorous superintendence were relaxed, they would do well to peruse the Reports of the Parliamentary Committees, appointed to examine into the state of madhouses in 1815 and 1816. They will there find that from the violence used to make patients swallow, the mouth was often lacerated, the teeth forced out, and not unfrequently the patient suffocated.

Corporal chastisement, in its most brutal varieties, was very common. Thus, Captain Dickenson, R. N., was shockingly beaten while confined by means of a chain on his legs, and handcuffs, which rendered it out of his power to ward off the blows which were repeatedly given him on the face and body.

As severe whippings were the most obvious, so they were the most favourite method of procuring a ready obedience from the inmates of madhouses; but they were also the first abuse which it was necessary to lop off. A witness says in her evidence, "Mr. Talbot made this observation to me, 'Mrs. Humieres, we must not follow that practice of flogging Isabella Adams, or else the public will get hold of it; a whip is not to be suffered to be used in our house.'" At this time the whip was used as an engine of punishment: at an earlier period it was considered a most valuable therapeutic agent.

The measures taken to provide for the security of the patients were often worthy only of the darkest ages, and might lead one to suspect an error in the date. Thus, in a Wiltshire mad-

It was then a common practice to cut off a negro's leg as a punishment for running away, and a sure method of preventing a repetition of the offence. (Vol. i. p. 314, edit. 1806.) In one instance the master was more humane; he only cut the tendo Achillis.—(Vol. i. p. 101.)

* Stedman was in Surinam from 1772 to 1777.

house, Dr. Fowler found a man confined in an oblong trough, chained down.

"He had evidently not been in the open air for a considerable time; for when I made them bring him into the open air the man could not endure the light; he was like an Albino blinking; and they acknowledged he had not. Upon asking him how often he had been allowed to get out of the trough, he said, 'perhaps once in a week or ten days, and sometimes not for a fortnight;' he was not in the least violent; he was perfectly calm, and answered the questions put to him rationally; his breathing was then so difficult, that I thought his life likely to be affected by it".

The famous case of Norris will occur to many of our readers; he was fastened to a wall, in Bethlem Hospital, by a complication of chains and rings, and bars, with his right leg chained to a trough, and so secured that he could not repose in any other position than on his back. But perhaps the best account of his case is a transcript of the smith's bill:

"June 23d, 1804, Bethlem; to two bolts and nuts and screws, three feet nine long; to a new round rail-bar for head of bedstead in one of the cells, for Norris, seven feet long; to a new collar for Norris's neck, with two joints to ditto, and two basils for his arms, and five chains, and seven rings to go over the round bar; to two men one day and a half boring holes through brick walls, and fixing the round bar and rivets, and rivetting on the neck-collar and the basil, by order of the committee"†.

And under the date of October 6th, 1804, we find:

"Bethlem; to taking off, and taking to pieces, and lengthening the strap-jointed irons of Norris, and refixing"‡.

A man, named Murphy, was brought

* Minutes of Evidence, taken before the Select Committee appointed to consider of provision being made for the better regulation of Mad-houses in England. Ordered by the House of Commons to be printed, 25th May, 1815. (P. 46.)

† Report of 26th April, 1816, p. 42.

‡ Ibid, p. 43.

from the Batavia Hospital Ship; no harmless method of securing a lunatic having been discovered at the remote period of English history of which we are treating, his arms were cut by the rope with which he was fastened to the stage coach, the whole of the muscles were separated, and the bone was exposed. From the report of July 11, 1815, it appears that this accident, or rather result, was common in Ireland. (P. 24.)

But the state of lunatics, where no mischievousness could be alleged as a motive for extraordinary precaution, was too often miserable in the extreme. Mr. Godfrey Higgins, a Yorkshire magistrate, found concealed cells in the York Asylum. There were four cells, about eight feet square, "in a very horrid and filthy situation, the straw appearing to be almost saturated with urine and excrement." He next found a women's room, twelve feet long by seven feet ten inches, containing thirteen women, who had all come out of their cells that morning. Patients, too, were missing and unaccounted for, and false reports made out.

Other abuses, though great in themselves, may be passed over as small in comparison. Thus the language addressed by keepers to their patients appears to have been coarse and unseemly to the last degree: the lunatics were constantly robbed of the clothes, and comforts, destined for them by their friends; their medical treatment was conducted by the almanack; a bleeding on such a fixed day, and an emetic on another one, &c. &c.

The exhibition of the lunatics in Bethlem to any one who could muster four shillings, was, indeed, a monstrous abuse; it has long been discontinued, but the mention of it may serve to show how strange was the feeling, or rather want of feeling, on this point, in the last century.

In next passing from asylums as they were, to asylums as they are, we are inclined to follow Mr. Browne*, and to exclaim with the French writer whom he quotes, *nos pères n'ont pas renversé toutes les Bastilles!*

Many of the old abuses have been continued through avarice, or even through habit, long after their formal abolition. Thus in the winter of 1820 the Commissioners found a patient, in a private institution, alone in an out-house without a fire, and without shoes; the windows broken. After much shuffling, it was proved that this patient did not sleep in the apartment said to be his, but in a miserable room up a private staircase, concealed by a door, which was discovered with considerable difficulty. It was a single room, small and offensive, containing only a wet and dirty piece of sacking filled with straw, with one rug and a blanket. For this treatment the patient paid 50*l.* per annum†.

Patients have been obliged to remain in bed the whole of Sunday, in order that the keepers might visit their friends; and our only astonishment is that this festival did not occur more frequently than once a week, when we find that, in one asylum, each servant had charge of fifty patients; that in another, 164 patients were intrusted to two keepers; and that, in a third asylum, three keepers were expected to govern 250 patients‡.

To shew how difficult is the extirpation of old errors, the writer whom we have just referred to quotes a curious story of a gentleman who, a few years since, was about to place his brother

under the care of Dr. Fox, of Brislington. "I hope," said the applicant, "you will be as gentle to my brother as you possibly can." "Certainly," said the Doctor. "I know," continued the other, "it is very necessary you should exercise some severity on him; but I hope it will be as gentle as possible." Dr. Fox was at first unable to understand the gentleman's meaning; but it turned out that his theories on the treatment of madness were quite Shakspearian, and that he believed a whip to be the chief remedy employed.

But perhaps the strongest instance that can be alleged, to shew how far we are yet from a due regulation of madhouses, is the case of Willis *alias* Moseley, on which we touched in our article on the Encouragement of Quackery*. The man had been convicted, in October 1836, of keeping an unlicensed madhouse. As he still persisted in the practice, he was brought up for judgment the following January, and sentenced to twelve months' imprisonment, and a fine of 50*l.*

The evidence on the trial went to shew, that the madhouse in question was furnished with iron bars to the upper windows, and had an attached building in the yard, which was fitted up as cells and dungeons, where persons might be unlawfully imprisoned. Mr. Sergeant Adams pronounced sentence, and dwelt very forcibly upon the dangerous purposes to which the cells and dungeons might be applied.

A few months afterwards all the sane and sober were amazed to learn, that the criminal had been liberated. This was done, as appears from a paper published by his friends, under the following shallow pretences.

After his conviction, the prisoner memorialized the Secretary of State, denying the existence of cells, &c., and

* What Asylums were, are, and ought to be: being the substance of five lectures delivered before the managers of the Montrose Royal Lunatic Asylum. By W. A. F. Browne, Surgeon, Medical Superintendent of the Montrose Asylum, &c. Edinburgh and London, 1837.

‡ A useful compendium of the subject, somewhat disfigured by the stiffness of the style.

† Browne, from Report on Pauper Lunatics in Middlesex, p. 155. 1827.

‡ Ibid, p. 147.

praying an inquiry into his case. It was then determined that the Chairman of the Sessions, accompanied by some other magistrates, should go over the building. This was done; and, according to the Willis-alias-Moseley document, "the Chairman found that the formidable building, said to exist in the yard, was in fact nothing but a covered way leading to the stables, and evidently as old as the house. There was no pretence whatever, the Chairman publicly stated, for saying there was any thing like dungeon, cell, or place of restraint. The Chairman reported these facts to the Home Office, and Mr. Moseley was immediately set at liberty."

Now without asking, as some people would do, whether the interval between the conviction and the investigation might not be sufficient for the removal of many engines of restraint, we would simply inquire—

1st, Whether the ordinary cellars of every London house are not as truly cells and dungeons, as any other that could be contrived for the coercion of a maniac?

2dly, Whether, on the supposition that Mr. Willis-Moseley contented himself (as the tenor of his puffs makes it probable) with melancholic patients, and rejected those whose madness was furious, it would not still have been right to allow the law to take its course?

If we wish to prevent the incarceration of the sane, we must be chiefly on our guard in those cases where hypochondriacism may be taken for melancholic madness; in these cases the timid patient, weak in body and irresolute in mind, requires neither cell nor bar; and madhouses may be easily framed, whose moral restraints shall escape the eyes of a whole bench of magistrates.

OPERATIONS AT ST. GEORGE'S HOSPITAL.

Encysted Tumors of Scalp—Removal.—Diseased Wrist—Amputation.—Division of the Tendo Achillis for Club Feet—Novel Operations(?).—Fatty Tumor—Removal.—Hernia—Strangulation nine days—Mortified Bowel.

ON Thursday, October 12th, there were four operations: three by Mr. Keate, and one by Mr. Walker.

Mr. Keate first removed three encysted tumors from the head of a man who had had some already taken away, and one or two more were still left to be removed on a future day. One was of some size, and as the skin adhered much to it, a portion was taken away with the tumor, which adhered so as to require dissecting out, and bled a good deal. The others were turned out easily by pressure at their sides after the scalp had been divided and the surface of the cysts exposed.

Mr. Keate next removed the fore-arm of a man by the circular operation near the wrist, in consequence of disease of twenty years' duration; the joints of the wrist and carpus being extensively ulcerated, with much swelling around them, and very great pain on the parts being at all moved. The case has done well.

The next patient brought in was a little boy of six years of age, whose feet had been deformed from birth; the foot being in both cases turned in, and rotated upwards and inwards, while the toes were at the same time kept pointed by contraction of the tendo Achillis. Mr. Keate inserted a narrow knife, with the flat surface between the skin and the tendon; then turning the edge towards the tendon, it was cut across by the knife, the heel being at the same time drawn downwards, so as to separate the divided ends from one another. An instrument was then fastened upon the foot, so as to inclose it in a strap and kind of shoe, while an iron went up the leg on each side, and was buckled round the leg just below the knee. The shoe part was so contrived as to stretch the back of the leg, and allow of its being still more extended, if necessary, afterwards. The little fellow bore both operations without a single exclamation, and hardly even moved during the time. Mr. Keate afterwards explained the case to the students, and remarked that he had lately seen or heard of this operation having been revived as a new discovery, but that he had often seen it done, and as long as *five-and-thirty years* ago; and that he believed it generally succeeded very well.

A woman then came into the theatre, who had a fatty tumor, of the size of a small melon, on the back of the left shoulder,

which was removed by Mr. Walker. He made a deep cut through the skin, and half way into the tumor, which he then dissected out with the knife, removing some of the fascia and fibres of the trapezius muscle, which adhered to the tumor. There was a good deal of bleeding from the operation, so that the wound was not dressed till some time afterwards, which, indeed, was the case also with the two first patients on whom Mr. Keate operated, in order to avoid secondary hæmorrhage. - Mr. Walker made some remarks on the nature of fatty tumors, which he said were perfectly innocuous, and that they were generally unaffected by medicine, though he had seen one case in which a large fatty tumor of the chin had yielded to *Liquor Potassæ* in large doses, and that there had been no return.

On the following Monday, October 16th, Mr. Hawkins operated for a case of strangulated femoral hernia, in a woman between thirty and forty years of age, the strangulation having taken place on the morning of the Sunday week preceding, so that it had now been strangulated *nine days*. She had been constantly sick since that time, during which she had done nothing except taking six pennyworth of castor oil, and on her admission was in a very low state, apparently suffering much pain, with the bowels so distended that the turns of the intestines could be seen through the abdominal muscles, especially at intervals when the pain came on most violently. The hernia contained a small piece of bowel, intimately adherent to the sac, especially in front, and so lacerable that it gave way, and a little milky fluid escaped. Attempts were made to close the opening by ligature, so as to return it into the abdomen, but the mortified bowel would not hold the silk; it was therefore opened freely, and fastened to the edge of the opening after the stricture was divided. When this was done, some fluid fecal matter escaped, after which a poultice was applied to the wound, with camomile fomentation to the abdomen. At nine o'clock, P.M., five hours after the operation, the patient seemed more comfortable, and lay quietly in bed on her back, instead of constantly tossing about, and had not been sick; no further evacuation, however, had taken place, nor had injections brought away any thing from below the strictured part. Mr. Hawkins passed his finger into the bowel, which seemed to be free; and as there did not appear to be any mechanical impediment, five grains of calomel were given twice, with two drachms of Epsom salts, in infusion of roses and mint-water, every three hours, and some beef-tea and arrow-root were directed to

be given. The next morning, at 7 A.M., about a pint of fluid fecal matter had passed by the wound, and the patient felt much more easy and composed, the pulse being only 102, instead of being as rapid and quick as before the operation, and she had slept several times for a short time. At 1, P.M., she seemed considerably improved, a good deal of fecal matter coming away, and the abdomen being soft and not so distended. She had been sick once after some arrow-root, but had no pain scarcely in the belly, and was cheerful and free from anxiety. A grain of opium, with three of calomel, was given, and the mixture repeated at longer intervals, and the abdomen bandaged with flannel.

On the whole she was much better than could have been expected after so long a strangulation, affording some ground to hope that she might recover, but the amendment of twenty-four hours is far too short a period to be sanguine about after so protracted a strangulation. We shall endeavour to give the sequel of the case in our next number.

HÔPITAL DE LA PITIÉ.

COMMUNICATED BY H. CURLING, Esq.

[Concluded from page 111.]

CASE IV. — *Emphysema; Diseased Auriculo-ventricular Valves; Pneumonia; Abscess.*

A SEMPRESS, aged 30, of a delicate constitution, was admitted into La Pitié, February 20th, under M. Louis. She menstruated for the first time at 13, and was married at 23, but had no children. She has been subject to dyspnoea as long as she can recollect, and has always slept with the head elevated. During the last three years the dyspnoea has much increased, accompanied by palpitations, cough, and accesses of difficulty in breathing, so that she is often obliged to get out of bed during the night, and always sleeps in a sitting position. She has hardly ever been without a cough during this period, even in summer. She has never had the jaundice, and her legs began to swell for the first time six weeks ago, since which time she has kept her bed. She has been in the Hôtel Dieu a short time, and leeches were applied to the anus, on account, she says, of an enlarged liver. Her face has acquired a yellowish tint, and her urine is of a yellow colour. The border of the liver can be felt three inches below the ribs; percussion yields a dull sound to this extent; on pressing suddenly, there is a sensation as if a fluid was interposed between the liver and abdominal parietes. The inferior extremi-

ties, the left arm, and forearm, are œdematous. Percussion yields a dull sound upon percussion in the precordial region; sounds of the heart obscure; slight impulsion; a bruit de râpe after the first sound; the bruit is loudest about three inches above the xiphoid cartilage, and one inch external to it; the bruit can be heard on the right side. Percussion less loud under the right clavicle, for about three inches. A well marked bruit de frottement in this space. Subcrepitation on both sides, inferiorly and posteriorly; a little rhonchus sibilans at the angle of the right scapula, where the patient complains of a pain. Formation of the chest quite natural. To take—

Eau de Vichy, $\frac{1}{3}$ rd of a bottle; Pulv. Opii, gr. i.; nocte sumend. Hirudines x. to the anus.

Feb. 25th.—Sputa always contain traces of blood; appetite diminished; dyspnoea less. The abdomen contains fluid. The liver not diminished in size.

Rep. Med.

27th.—Sputa still streaked with blood; pulse 100, regular and feeble, and offers the same character on both sides. Posteriorly on the right side percussion is obscure in the middle third, and dull in the inferior third; neither bronchophony nor bronchial respiration, but subcrepitation exists less loud and less superficial than on the opposite side; a little craquement at the summit of the same side, for about two inches. Percussion equally loud under both clavicles. No bruit de frottement.

Potassæ Acet., 3ss. in ptisan Pulv. Digitalis, gr. i. bis in die. Pulv. Opii, gr. j. horâ somni sum.

March 5th.—The sputa have a bistre tint. The secretion of urine is much increased, but the œdema has not diminished. On the 9th, percussion was dull under the right clavicle; bronchophony, bronchial respiration, and crepitation, were heard in the same situation. On the 12th there was a well-marked bruit de souffle on the left side, two inches above the xiphoid cartilage, and one from the median line. The pulse was 100, regular, but had a distinct fremitus. The œdema continued to increase. On the 15th she had diarrhœa; her cough was increased; the precordial region was dull upon percussion, but the respiratory murmur could be heard there. There was a little bronchophony under the right clavicle, but no râle. Percussion was not very loud under either clavicle. She died on the 17th inst.

Autopsy 36 hours after death.

Extremities œdematous, more especially

the inferior. The brain healthy; a little infiltration of serum under the arachnoid, and each ventricle contains about half an ounce of a clear fluid. The abdomen contains one pint and a half of a clear limpid yellow fluid; the liver descends about three inches below the ribs.

Pericardium universally adhered to the heart; adhesions very strong, requiring much force to separate them. The heart measures three inches six lines anteriorly in height, and posteriorly three inches; the diameter of the base is three inches eleven lines. The right auricle contains much blood; the thickness of its parietes varies from two and a half to four lines. The right auriculo-ventricular orifice measures only four lines in diameter; its border is irregular, caused by little soft granulations of a yellowish colour, which form a border, projecting about $\frac{1}{3}$ rd of a line, and very easy to raise. The valves preserve their transparency; but the right and left are almost entirely adherent in nearly their whole extent, so that the orifice is much contracted. The pulmonary artery is more voluminous than the aorta, and its parietes are thicker; its ramifications are also larger than usual; no spots are seen on its internal surface. The two borders of the mitral valve are adherent, and the opening very small; the orifice measures only two and a half to three lines in width; its borders are smooth and distinct; the valves are thickened, and the chordæ tendinæ seem to be shortened. The left ventricle is healthy, having five lines in its greatest thickness. The left auricle is thickened, and its parietes firm. Small yellow spots, slightly elevated, are seen on the inner surface of the aorta. The borders of the left lung are rounded; the air vesicles are universally dilated on the surface, having twice their natural volume, and in the superior lobe five or six times. At the base of the same lung near its border is an appendix, formed of dilated air-cell; it is two inches in length and half an inch in width. The right pleura contains about a pint of serum. Several adhesions exist between the left lung and pleura, and its cavity contains Oiss. of serum. The right lung is larger at the summit than at the base; the vesicles are dilated, but compressed by the fluid. At the inferior portion of the superior lobe is a small cavity, lined by a thin membrane, and containing a yellow opaque softened matter; the lung immediately surrounding it light and elastic. A similar cavity near the base of the inferior lobe.

The mucous membrane of the great end of the stomach softened and transparent; large and small intestines healthy. The liver measures six inches in width, seven in height, and about an inch in

thickness; it contains much blood, and is firmer than natural. The gall bladder distended with bile. The spleen and kidneys natural.

CASE V. — *Emphysema — Hypertrophy of the Heart, with Contraction of the Aortic Valves.*

A man, aged 36, of a strong constitution, was admitted under M. Louis, Jan. 23rd, 1837. When 22, he entered the army, and remained in the service ten years. He then followed a business which required but moderate exertion. At 14 years of age he had a very severe illness, and thinks it was an inflammation of the chest. It came on after getting wet from passing over a stream. Some time after that he had an attack of intermittent fever. In 1823, whilst a sergeant in the army, he was laid up in the Military Hospitals of Calais and Besançon for eleven months, with acute rheumatism. The joints of both the superior and inferior extremities were affected. He has never had any venereal affection, and does not often commit excesses. He denies having had at this time a pain in the precordial region, or palpitation. He has always been subject to dyspnœa after every exertion, and was nicknamed "poussif" by his comrades. He has, ever since this time, been subject to pains in his limbs, particularly in wet weather. In March and April of last year the pains were more frequent and severe than usual. In May, without any apparent cause, he for the first time had palpitations, accompanied by a pain in a spot corresponding to the inferior part of the left scapula. He had also a pricking pain in the precordial region, in a circular space of about three inches in extent, and two above the xiphoid cartilage. The dyspnœa increased, the palpitations became more frequent, and three months after he heard a bruit de râpe on the left side. He was obliged to sleep with his head elevated, and to get out of bed during the night, from accesses of dyspnœa. In October, in addition to the pricking sensation, he had a pain in the precordial region. He then entered La Pitié, and was under M. Serres. He was bled, and felt relieved, but only remained fifteen days in the hospital.

The palpitations, dyspnœa, and pain, soon returned. He applied some leeches then, traces of which remain below the left nipple; their application was only followed by temporary relief. Five or six days before his entry all the symptoms became aggravated. His countenance was pale, otherwise healthy. The right side of the chest was natural. On the left side a projection was observed, extending to

within two inches of the clavicle, and two inches below the sixth rib. Laterally it extended from nearly the edge of the sternum to within two inches of the angle of the ribs.

Percussion causes pain in this situation, and is louder than in the corresponding part of the opposite side. A bruit de scie, or de lime, after the first sound, and terminating in a bruit de souffle, is heard throughout this region, but having its maximum of intensity an inch below the nipple, and rather nearer the sternum; it is there also that the pain is greatest. It is also heard posteriorly on the same side, its maximum of intensity being at a point two inches distant, in a transverse direction, from the vertebra, and four from the inferior extremity of the chest, in a vertical direction. The bruit de râpe appears to be more superficial on the right side, and louder than on the left side. Its greatest intensity is at a spot corresponding to the middle of a line drawn from the right nipple to the xiphoid cartilage. The respiratory murmur cannot be heard in the precordial region, and is very feeble below both clavicles; four inches below the right it is inaudible. Posteriorly the respiration is healthy; palpitations constant; pulse 72, strong and regular, not full, but rather contracted. *Fremissement cutané* universal over the chest; slight *fremissement* in the radial artery; a bruit de soufflet in the carotids; no bruit in the femoral artery, but its pulsations seem stronger than usual. His feet and legs have never been swollen. He has but little appetite. He lies on his back, with his head raised. He was bled and cupped with temporary relief. He had an attack of the influenza, when the palpitations diminished.

At present (Feb. 25th) the bruit is much diminished, and is not heard posteriorly; but he still has an acute pain under the left nipple. Eau de Vichy was administered for five weeks, but without any improvement in the symptoms. He was still in the ward when I ceased attending.

REMARKS. — The disease commenced with palpitations and dyspnœa, which would seem to indicate some affection of the heart. Soon after, the patient heard a bruit de lime, and at the same time had an acute pain under the left mamma. A bruit de souffle may exist, and yet the heart be quite healthy; but a bruit de râpe has never been heard without some lesion of that organ being discovered after death—without the existence of contraction of some of the orifices, and consequent hypertrophy. But one important symptom is in this case absent; there has been no œdema of the extremities. Out of

forty-five cases of diseases of the heart, which M. Louis recorded at La Charité, in ten only was there no œdema during life. In eight out of this number there was no contraction of the orifices, so that in two only contraction of the orifices existed without œdema. The others were cases of hypertrophy, without complication. One of these two cases was that of an individual who, after a sudden strain, was attacked with palpitations; a bruit de scie was developed, being loudest on the right side. The symptoms continued for eight years, without the extremities becoming œdematous. The disease was thought to be aneurism of the aorta. After death the aortic orifice was found to be diminished to the size of a small fissure, eight lines in length.

There is no reason, then, that a contraction of one of the orifices should not be admitted in this case. The bruit is loudest on the right side, and not at a point corresponding to the aorta. If the aortic valves are diseased, the heart must be increased in size, which, by displacing the aorta, would account for the sound being loudest in that particular situation. Percussion affording a louder sound on the left side, would, however, seem to indicate that the heart had rather diminished in volume. The existence of very extensive emphysema on the left side would account for the loud sound upon percussion, which is rendered more probable by the projection on the same side, and by the long duration of the dyspnoea, and its presence at a period prior to the commencement of the palpitations. Besides, in eleven cases out of twenty-three, emphysema occupies the left lung. There is one fact against this supposition: the respiratory murmur, which ought to be more feeble than on the opposite side, was, if any thing, louder. Might not the bruit de scie, on the right side, mask the respiratory murmur?

What was the primary cause of this organic disease? Is it to be referred to the attack of rheumatism, which occurred fourteen years ago? The palpitations and the pain only made their appearance ten months back. M. Louis was of opinion that the affection of the heart had no connexion with the former attack of rheumatism. The patient was not asked if he had experienced palpitations and a pain; but he was asked if they were as severe then as they were ten months ago. He distinctly stated, that he had neither pain nor palpitations at that period. The question was often repeated, but always with the same result.

ACCIDENTS AT THE LONDON HOSPITAL.

Surgeon for the week, Mr. LUKE.

Assistant Surgeon, Mr. CURLING.

Fractured ribs, 5; contusion of the hip, 1; compound fracture of the tibia, 1; severe burn, 1; contused foot, 2; dislocation of the jaw, and contusions on the face, 1; cut on the head, 3; fractured humerus, 1; ruptured intestine, and fractured femur, (a) 1; contused elbow, 2; contused ankle, 3; contused hand, 1; bite on the hand by a pig, 1; fractured fibula, 1; severe laceration of the integuments of the thigh, 1; fractured femur, 2; incised wound on the face, 1; contusion on the knee, 1; inguinal hernia (reduced), 1; contused wound on the head, 1; contused knee, 1; contused wrist, 1; contused side, 1; bite on the foot by a rabid dog (parts excised), 1; strangulated femoral hernia (operated on), (b) 1.

Accidents admitted as in-patients	
during the week	35
Out-patients	55
Total	90

(a). This man received his injuries from the fall of a scaffold on which he was at work. He was brought to the hospital at noon in a state of great prostration, from which he never rallied, but died at nine in the evening. On examination of the body there was found a transverse laceration of the duodenum, just at its termination in the jejunum, with feculent matter in the cellular tissue around, and a small quantity also in the peritoneal cavity, which had passed through a slight laceration in the mesocolon.

(b). In this case the attempt was made to divide the stricture without opening the sac, but it was unsuccessful. The gut had only been down nine hours, and was closely girt at the neck of the sac. The bowels were relieved within a few hours after the operation and the patient is recovering.

The five cases of fractured ribs were unattended with emphysema, and at the last visit of the surgeon were doing well.

NEW EXPERIMENTS ON THE TORPEDO.

M. MATTEUCCI has communicated to the Académie des Sciences the results of his observations, made in June and July last, on 116 living torpedos which he was enabled to examine, in some instances, immediately after they were taken out of the

water, at Casenatico, on the shores of the Adriatic, whither he went expressly.

Besides the common galvanometer, he employed frogs; which show, he says, the direction of the current when passing from one foot to the other; the limb which contracts being that in which the current passed in the direction of the branching of the nerves.

It is said that a liquid is passed into the organ to produce the discharge, and also that this act is accompanied by violent muscular contractions: in either case there ought to be change of size or form, and yet none was evident, as he found by a very simple experiment.

According to M. M., no manifestation of electricity is obtained if the animal be not touched at once in two different points. Thus an isolated frog, which touched the torpedo's body by one of its nervous filaments, received no shock. The torpedo has not, as is generally believed, the power of directing the discharge to this or that point at will. When endowed with great vitality, discharges may be obtained from all parts of the body; but afterwards, the power is found limited to the regions situated above the two electric organs.

The distribution of the electric fluid is governed, according to M. Matteucci, by the three following laws:—

1st, All the points of the back are positive with respect to all those of the belly.

2d, The points of the dorsal surface, situated above the nerves which penetrate the organ, are positive with regard to the other points of the same surface.

3d, The centre is on the ventral surface. In relation to the course of the current in the interior of the organ, the layer which touches the dorsal skin, or which is nearest to this part, is always positive with regard to the layer contiguous to the ventral skin.

The intensity of the current varies in proportion to the extent of the layers which touch the two surfaces of the organ. When the animal has great vitality, the current traverses, without sensible weakening, a great mass of salt water, even divided by metallic diaphragms; but the transmission becomes less powerful as the animal loses its strength. To obtain the spark, the author used a different proceeding to what he first intended,—merely placing the animal between two metallic plates, which communicated by two gold leaves.

All the skin, muscles, and ligaments, attached to the organ, may be cut off, without the intensity of the discharge being sensibly diminished. Even three-fourths of the substance of the organ may be cut away, and yet shocks be obtained; but the

ligature of the nerves puts a complete stop to them. The medulla oblongata and medulla spinalis may be divided without the discharges ceasing. As to the brain, it is only the fourth lobe which cannot be touched without a discharge taking place, and this, which may be called the electric lobe, being once removed, all the phenomena of electricity are put an end to. It must be observed, however, that the nerves of the organ itself, after separating it from the brain, may still give some discharges, if irritated, immediately after this separation.

When the torpedo is weakened, so that it can no longer give shocks, very strong ones may still be obtained, and these even stronger than those which it gave in its greatest activity, by touching the electric lobe. The action of the lobe in this case, says M. Matteucci, is direct; that is, if the right side be touched, it is the right organ which gives the discharge. This is the only method by which discharges can be obtained from one side only. When discharges can no longer be obtained by touching this lobe, which is the case only some seconds after the death of the animal, very energetic ones may still be obtained by wounding the organ deeply; but in this case the discharges have no longer the constant direction from the back of the belly.—*La Gazette Médicale*, Oct. 7.

M. VELPEAU'S TREATMENT OF FRACTURES.

M. VELPEAU read a paper on this subject at the Academy of Sciences, on Sept. 25. At the present day two methods are employed by practitioners: in the one, the apparatus is not applied till effusion has taken place in the limb; in the other, they act on the supposition that a moderate compression hastens the effusion, and lessens the inflammation. M. Velpeau is of opinion that in all fractures, even in those which are accompanied by wounds of the integuments, we should proceed immediately to the reduction.

The fracture being reduced, he surrounds the limb, from the roots of the fingers or toes, up to its proximal extremity, with a slightly-compressing bandage, so as to maintain the fractured pieces in a convenient position; and instead of using splints and compresses, he makes the envelope itself, which the bandage forms, stiff. He at first thought of using for this purpose the solidifying liquid of M. Larrey (albumen), but afterwards he found it preferable for the purpose to make use of starch, prepared after the method used by washerwomen, in imitation of the example of M. Sentin, of Brussels. This latter surgeon, however, uses a different

apparatus—viz. a double Sculletus' bandage, and splints of double layers of card-board.

The dressing of the whole apparatus is effected in from two to four days. When this has once taken place, the limb and the bandage are so exactly moulded to one another, that displacement is impossible. The compression being every where equal and moderate, supports the tissues, and does not produce any inconvenience. The patients can turn themselves and move about in their beds as if they had only a simple contusion of the leg. They are not obliged to remain confined and immovable for six weeks or two months; but may, without inconvenience, sit up on rather a high seat (for they may slightly fix the leg,) and may walk about with the assistance of crutches, and supporting the foot in a great stirrup tied round the neck.—*Ibid.*

SUGAR OF LEAD PILLS.

To the Editor of the Medical Gazette.

SIR,

In my letter on the Treatment of Cholera, published by you last week, I spoke of pills made of acetate of lead and opium; it may be well to remark, that the most convenient way of making these pills is to add five or six grains of powdered liquorice to the scruple of acetate of lead. They may be readily mixed into a mass by means of mucilage of gum Arabic.—I am, sir,

Your obedient servant,

ROBERT J. GRAVES.

Dublin,
18th October, 1837.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, Oct. 19, 1837.

William Frederick Shoebridge.—William Burgess, Frodsham.—Edward Storer, Northumberland.—Alfred Drew, Cambridge.—Benj. Eardley, Chesterton, near Newcastle-under-Lyne.—George Cooper, Lynn, Norfolk.—Duncan Sinclair, Halstead, Essex.—Henry J. wel, Tregony, Cornwall.—William Clarke Nutt, Plymstock, Devon.—John Lloyd, Anglesea.—Bentham Casaubon Chandler, Canterbury.

NEW MEDICAL WORKS.

A Conspectus of the Pharmacopœias of London, Edinburgh, and Dublin. By A. T. Thomson, M.D. 10th Edition, 5s. 6d. cloth; or 6s. 6d. roan tuck.

Rudiments of Physiology. By the late J. Fletcher, M.D., &c. Edited by R. Lewins, M.D., &c.; with a Biographical Memoir of the Author. Svo. 19s.

Memoranda of difficult Subjects in Anatomy and Surgery, by R. Druitt. 2s.

Dr. Steggall's Materia Medica and Therapeutics, 12mo. 7s.

Dr. J. Wilson's Practical Treatise on Simple and Medicated Vapour applied locally. 8vo. 6s.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, Oct. 17, 1837.

Abscess	2	Heart, diseased . . .	3
Age and Debility . . .	19	Hooping Cough . . .	6
Apoplexy	4	Inflammation . . .	19
Asthma	3	Bowels & Stomach . .	4
Consumption	43	Brain	3
Constipation of the . .		Lungs and Pleura . .	3
Bowels	1	Insanity	5
Convulsions	21	Liver, diseased . . .	1
Croup	1	Measles	8
Dentition or Teething .	4	Miscarriage	4
Diarrhœa	1	Mortification	3
Dropsy	12	Paralysis	1
Dropsy in the Brain . .	9	Small-pox	3
Erysipelas	2	Thrush	2
Fever	9	Unknown Causes . . .	6
Fever, Scarlet	3		
Fever, Typhus	6	Casualties	5
Hæmorrhage	1		

Increase of Burials, as compared with the preceding week . . . } 26

METEOROLOGICAL JOURNAL.

Oct.	Thermometer.	Barometer.
Thursday . 12	from 43 to 63	30.33 to 30.41
Friday . . 13	35 66	30.44 30.50
Saturday . 14	33 55	30.55 Stat.
Sunday . . 15	*18 55	30.52 30.45
Monday . . 16	35 56	30.36 30.28
Tuesday . . 17	31 55	30.18 30.09
Wednesday 18	46 59	30.05 30.13

Wind. N. and N. by W.

Except the 16th and 18th, generally clear; a little rain on the morning of the 18th. *The thermometer on the morning of the 15th remarkably low.

CHARLES HENRY ADAMS.

NOTICES.

We cannot insert the letter signed "A Non-Tripartite."

OBSERVER, on Naval Surgeons, is under consideration.

OBSERVER, who abuses University College, wants point, and we think is unfair in attacking a Professor of whose merits or demerits he cannot possibly have yet had time to judge. By the way, his own proposed motto is a false quotation: *etiam* ought to be *et*.

We cannot make room for Dr. Howison's papers on Botany. Nos. 1 and 2 have, therefore, been returned to the publishers.

ERRATUM.—In Mr. Chapman's paper, in our last number, page 98, col. 2, lines 41 and 42, for "capillaries," read "absorbents."

WILSON & SON, Printers, 57, Skinner-st., London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, OCTOBER 28, 1837.

LECTURES ON THE PHYSIOLOGY AND DISEASES OF THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL
AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of
1836 and 1837,

At the Anatomical School, Kinnerton-Street,
near St George's Hospital,

By CHARLES J. B. WILLIAMS, M.D. F.R.S.

LECTURE V.

On the Nature and Properties of Sound — Sound a kind of motion — Explanation of Vibrations of Sound — Vibrations of Cords, of Solids, Liquids, and Air — Effect of Sound on the Molecules of Bodies — Explanation of Duration of Sound — of Pitch, Harmony, and Discord — Conduction of Sound — Reflection of Sound — Modified Echoes — Sources of Sound — Modes of Increasing and Transferring Sound — Sounding-boards — Ear Trumpets — Difference between the Reflections of Sound and Light.

Now, gentlemen, to supply us with what we found needful in the last lecture, I am going to give you a brief explanation of the nature and properties of Sound. Perhaps you will be disappointed at finding that I do not propose to exhibit to you the various kinds of apparatus and instruments, by which lectures on acoustics are made attractive, if not instructing. I purposely avoid every illustration but those of the simplest and commonest kind; and if I can judge by my own experience, you will learn more of the nature and laws of sound by reflection on facts and pheno-

mena that are always within your reach, than by the most striking and wonderful displays wrought by the complete apparatus of the lecturer.

Let us begin with the abstract question, what is sound? *Sound* is not a matter, a separate thing; it is a condition of matter. It is a *particular kind of motion* in matter; and this motion being communicated to the matter in contact with our organs of hearing, produces that physiological impression which we call sound. This same motion, when examined by another sense, as sight or touch, can often be seen or felt to be motion. Thus, when I strike a cord of this guitar, I not only hear the sound, but see and feel it move. We shall find hereafter many cases in which we can feel the motions of sound; and in all other instances where we hear sound, we know there must be motion. Our organs of hearing are more sensitive of this motion than our eyes or our fingers; they can feel finer degrees of it, because they are specially adapted to it.

But what kind of motion is this which constitutes sound? It is not a slowness of motion; for, as you perceive when I move this stick slowly through the air, it gives no sound. Neither will mere velocity of motion suffice; for, as you know, the earth is moving with great velocity, "without a whisper in its silent course." Now observe, by an example, what sort of motion produces sound. When I move this stick against something which offers resistance to it, as when it strikes this table, it gives sound; or when I move it so quickly through the air that the air resists it, we then have sound. Besides *motion*, then, we must have *resistance*; and this is so essential an element of sound, that I have ventured to define sound to be *resisted motion*, or more exactly, *motion of a certain force resisted by a certain force*. The moving force, and the resisting force, impelling the matter alternately to and fro, in oppo-

site ways, constitute what are called the *vibrations of sound*.

This word *vibration* is a very good one for us, because it describes the nature of sound better than the words *wave*, *undulation*, *pulse*, &c., which rather imply the relations of sound to distance, and are less simple in their signification. Now if you want to understand what vibrations are, look at the bass cord of this guitar; it is now slack, and when I touch it you can follow, and almost count, its to-and-fro motions with your eye; these motions give no sound, or only a slight flutter, which is accidental. But observe how, as I gradually tighten the cord, its motions become quicker; and now you hear a sound. Now the silent vibrations which you could follow with your eye are precisely of the same kind as those which produce sound, except that they are slower; so are the vibrations of a pendulum: and as we can watch these, let us study in them the nature of vibrations in general. When I move this pendulum out of the perpendicular, and then let it go, you see it not only falls back to the perpendicular, but its momentum carries it beyond, to describe part of a circle on the opposite side, from whence it again sways back, past the perpendicular, to the side from which it started; and so on, backwards and forwards, for some time. Now the cause of these vibrations are two opposite forces, which alternately predominate—the disturbing motion or momentum, and gravitation. So, also, in the vibrations of a cord or wire fixed at both ends, the opposite forces are the disturbing force or momentum, and the elasticity of the cord; the first carrying the cord out of a straight line, and the latter tending to restore it to it. But I must not dwell too long on elements that may be known to you already; or that may, at all events, be mastered by a little reflection.

You will perhaps say, this is all plain enough in the case of a pendulum or a cord, but how can solid bodies, or liquids, or air, vibrate? Where are the contending forces here? I answer to this, just reflect a minute on the intimate constitution of these bodies. All matter is composed of molecules, which are held in their places by certain mutual forces of attraction and repulsion, which resist any forces tending to displace them. And now mark me, this resistance is not absolute, but of the same kind as the resistance which these guitar cords offer to my fingers; they yield for the moment, but spring back again, and vibrate to and fro until they recover their state of rest. It is an *elastic resistance*; and it is this property which constitutes in bodies what is called *molecular elasticity*. I will illustrate this by a

diagram that will very well represent the vibrations of molecules.

Suppose the molecules of matter to be elastic spheres or spheroids (which, if not in themselves, yet, in their spheres of attraction and repulsion, they certainly are), we may represent them thus—



If an impulse be communicated to the first of these elastic spheres, the first effect will be to flatten it thus—



But its elasticity causes a reaction, which not only restores its shape, and communicates the impulse and flattening to the next molecule, thus—



but springs beyond it into an oval of the opposite direction, the original impulse having been then transmitted to a further molecule, thus—



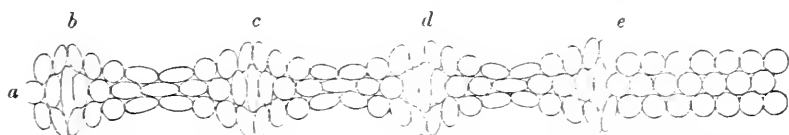
There is then another reaction, which throws it beyond its spherical into its first oval shape; and so on until the equilibrium of forces is restored, a molecule is rapidly undergoing all these changes—



Now this very diagram, which represents the changes which constitute the vibrations of a single molecule in a certain period of time, will also serve to shew how an impulse, and the vibrations which it causes, are propagated through distance, along a series of molecules; this is *propagation*, or *conduction of sound*. You will now, too, perceive what is meant by a *wave* or *pulse of sound*, which I just now said is a more complicated thing than a vibration. It is, in fact, a series of molecules in the different stages of a vibration. Thus, in the diagram above, every five molecules contain a wave, the last being in the same condition as the first, and being ready to go through the same series of changes again. As I have not time for much explanation, I have constructed the diagram below, which exhibits more fully these waves, and the changes that constitute them. You are not to suppose that it is mathematically exact, but it exhibits very well the principle of the phenomenon; and

if it sets you a-thinking, you will, making allowances for the necessary corrections, get from it a better notion of the nature and phenomena of sound, than from any of the usual illustrations. It represents the progress of an impulse, followed by its opposite reaction, through a triple row of spherical molecules, which are seen beyond *e* in their state of rest; the impulse,

which began at *a*, not having reached them. The successive waves or pulses, *d*, *c*, *b*, are the results of the alternation of the impulse and resisting, or reacting forces, before explained; and you see how an impulse, impressed on a single molecule (*a*), is communicated, laterally as well as in a direct line, just as we know sound to be.



By substituting elliptical molecules, we might represent the sonorous properties of those bodies that have a polarity, or whose density is greater in one direction than in another, as in the case of wood.

Well, then, having seen what sort of a motion that of sound is, and how it spreads itself through bodies, let us bestow a few minutes on the differences of quality of sound. Why does this piece of metal give a long ringing note, whilst this piece of wood, when struck in the same way, yields only a short knock? It is because there is a greater equality in the molecular elasticity of the metal, so that the molecules continue to vibrate without interfering with each other. Let us illustrate this by the pendulum. Here are two pendula; and you see, when I set them in motion, they swing harmoniously together, without interfering with each other, because they are of the same length, and therefore vibrate in the same time. Now I shorten one of them a little: now you see their times of vibration do not correspond, and they soon clash and stop each other's motions. It is just the same with the molecules of different bodies. Where their elasticity is uniform throughout the mass, the molecules vibrate in the same time; and not interfering with each other, the vibrations are continued, and constitute a note or tone, as in a simple piece of metal or glass. The molecular elasticity of wood, on the other hand, is not uniform; some molecules vibrate quicker than others, and the vibrations are consequently soon stopped. In softer bodies the molecular elasticity is still less uniform; their vibrations are therefore still shorter, and less perfect; they scarcely yield any sound; and they can choke the vibrations of other bodies with which they are in contact. Thus, see how little sound I get by striking this cloth; and when I bring this tuning-fork, which is sounding, in contact with it, the vibrations are immediately stopped. Yet we can get a sound out of

this cloth; and remark how: by stretching it until it is tight, which increases and renders more uniform the elasticity of its molecules. Observe the same thing in this piece of membrane: whilst it is loose and flaccid, it scarcely gives any sound; but when I stretch it on this ring, it becomes quite a little drum. You see, then, why some bodies are sonorous, and others not.

Let us take up another difference, that of *note*, or *pitch*. Why does this short piece of metal, when struck, give a high sharp sound, whilst this long bar gives a low bass note? Because its vibrations are quicker; and it is an ultimate fact, that the sensations of shrillness, or deepness, in sound, depend on the number of the vibrations in a given time. You shall see the proof of this. Here is a long steel wire, fixed at one end: it now vibrates so slowly, that you can count the vibrations; but it produces no sound. I shorten it, it vibrates more quickly; and now it gives a low tone. I shorten it more; you see it vibrates still more quickly, and the tone is raised. I now fasten it much shorter; you hear how sharp the tone is; but the motions are so rapid, that you can scarcely see them. Observe the same corresponding changes of phenomena as I loosen or tighten the cord of this guitar. Again, when I blow into this short tube, you have a shrill note; with this long one the note is deep. You cannot see the vibrations here, because the vibrating body is air; but you can easily understand that a short column of air will move more quickly than a long one; and when I blow hard into the long one, I get a note an octave or more above, because the force of the blast increases the elasticity of the column, which therefore doubles its vibrations. I could add many amusing illustrations, but our time allows me to add only a word or two on a subject immediately connected with this—harmony and discord. When two sounds strike the ear at the same time,

their vibrations will either combine or not, according as their numbers correspond or not in some simple arithmetical ratio. Thus, when a sound of 10 vibrations in a second strikes the ear at the same time with another of 10, the sounds will combine, and form the harmony of *unison*. Again, when one of 10 occurs at the same time with one of 80, the vibrations unite in the regular proportion of one to two, and the harmony of the *octave* results. So also with concords of the *third*, the *fourth*, the *fifth*, &c., there is a simple arithmetical relation by which their vibrations can unite; but when sounds of such numbers as 40 and 45, 40 and 53, and the like, occur together, their vibrations cannot coalesce, but, affecting the ear with conflicting motions, produce the impression called *discord*.

Conduction of sound is the transmission of sonorous vibrations through bodies: and this obviously depends on the same properties, of strength and uniformity of molecular elasticity, which render bodies sonorous; and I shall merely illustrate this by an experiment. This tuning-fork, when struck, gives very little sound as long as it is held between the fingers; but when its vibrations reach the sounding-board of this guitar, its note becomes loud and clear. Now, by placing different bodies as means of communication between the tuning-fork and the guitar, we may judge comparatively of their conducting power. Through this long deal rod you perceive the sound is conducted very well; the longitudinal fibres of this wood are very rigid and uniform, and its lightness makes it easily receive the vibrations of contiguous bodies. This iron rod, too, answers well, for its molecules are strongly and uniformly elastic, and resemble those of the tuning-fork, in which the vibrations begin. But observe, when I substitute this handkerchief, or this sponge, although the distance is much less, the sound is not transmitted through them. When I draw the handkerchief tight, you can then just hear the sound feebly transmitted. Rigidity, or uniformity of tension, then, and similarity in density, or lightness of mass, are the qualities which render bodies good conductors of sound; and the opposite qualities make them bad conductors, which either do not receive the vibrations, or, receiving them, promptly choke them.

I have just said that some bodies do not receive the vibrations from other bodies; neither do they destroy them. What becomes of them, then? They are *reflected* back into the media from which they come. Now *reflection of sound* is an important phenomenon, and we must look into it a little.

Suppose some light body—such as a cork ball, or an air-bladder—to strike a hard heavy body, such as a ball of metal; it would communicate little or none of its motion to the metal ball, but would rebound from it—that is, its motion would be thrown back upon itself. It is just so with the motions of sound. When the molecules of a light body, such as air, vibrate in contact with a hard body of much greater density, such as a stone wall, the motions are scarcely communicated to this body, but are reflected back; and this the more completely as the contrast between the density of the two bodies is greater. Now such a reflection of sound at a distance, you know, constitutes what is called an *echo*: here the reflected sound arrives at the ear later than the original sound, by the time which it has taken to travel to the reflecting surface and back. Sound travels through air at the rate of 1130 feet in a second; so you can easily perceive, that at short distances, unless the reflections be repeated several times, the reflected sound will not be distinct from the original sound, and there will be no echo. Now in large rooms, particularly when empty, the reflections are repeated many times, and you therefore hear a reverberation more or less continued, and generally altered by the reflection: but it is in small chambers, or cavities, of from six or eight feet down to two or three inches in diameter, that echoes present the most remarkable modification of a ringing or tinkling kind. This is what you may hear in empty barrels, bottles, and other hollow objects. When any sound or impulse reaches the air within them, it is reflected from side to side so rapidly that the number of reflections becomes as the number of vibrations, and gives a note the pitch of which is determined by the relation which the diameter of the cavity bears to the velocity at which sound travels through air. This subject requires a little consideration to understand it; but you will find in it the explanation of several phenomena intimately connected with the subjects to come before us. We have no time to dwell on it now.

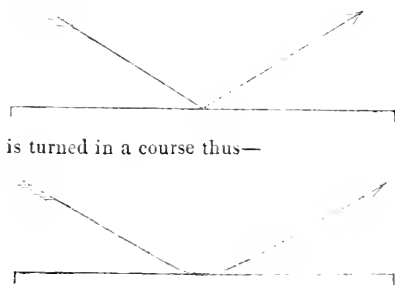
We have seen that a certain kind of resisted motion constitutes sound: we have now to run rapidly over a few instances of this, in various modes of producing sound. The most familiar modes of producing sounds in solids are by percussion, collision, and friction. The two latter are, in fact, modifications of percussion, consisting of repeated percussions of the particles of the surfaces or edges of bodies. When these percussions are repeated with a certain quickness and regularity, they constitute a continued note. This cord is

wound round with wire which forms a series of rings on it: when I rub the edges of this card slowly along it, it produces a *click* at each ring, and you only hear a succession of clicks; but when I pass the card rapidly, you do not hear the click, but a continued note, and this note is higher the quicker I move the card. Such sounds I call *click-notes*. They are not very musical, but they are interesting, as they present us with an analysis of many common sounds. The notes of the cords and wires of musical instruments, and of the parchment of the drum, need no comment: they are the result of the vibration of bodies rendered uniformly and freely elastic by artificial tension. Sounds depending on the vibration of air are instanced in explosions, and in the notes of the flute, pan-pipe, whistle, &c. These are most readily conducted by air. Many sounds result from the combined motions of solids and air; the solid opposing a vibrating resistance to the passage of a current of air, and communicating its vibrations to the current. Such are the notes of reed instruments, trumpets, the æolina, &c.; and I believe that the human voice may be classed among them. Observe what a variety of notes I can get by blowing through this tube, with a thin piece of India-rubber stretched over the end of it. These are the same kinds of notes as those of the human voice; rather a cracked one, I admit, yet I have heard worse notes sung, and you will see the instrument is quite a rough and extemporaneous one. By drawing the India-rubber tighter, the tone is raised at pleasure. I have no doubt that we might, by a little coaxing, make the note more mellow; and it requires only a little mechanism to divide this note into articulate sounds. But I leave it for others to make this artificial voice speak; it has said enough for us.

There are certain modes of increasing and transferring sound, which we must just advert to. When I strike this tuning-fork, and hold its vibrating end at the mouth hole of this flute, it produces no sound unless I stop up some of the finger-holes, so as to produce the fingering of a note in unison, or in close harmony, with that of the tuning fork; and then observe what a clear note comes forth. The column of air in the instrument so adjusted, has just that degree of elasticity that enables it to vibrate in unison with the tuning-fork, the notes of which it reciprocates. A cord tuned in unison will also reciprocate in like manner. Sounding-boards of stringed instruments are constructed with hollows of varying depth, that the air within them may reciprocate the different notes of the cords; but the chief use of sounding-boards is to assist in

transferring the vibrations of the solid wire, or cord, to a large surface of the air, which is so much more rare and yielding that it is but little impressed by the motions of so small a body as the cord alone. You have already seen this illustrated with the tuning fork and the sounding-board of the guitar; but now you may perceive that this board, which has no hollow, answers in a less degree. The knowledge of these facts enabled Mr. Wheatstone, some years ago, to astonish the public with his *enchanted lyre*; which was merely a sounding board, communicating by a small rod of wood, or metal, with the sounding-board of a pianoforte concealed in another room.

Sounds produced in air may be directed and concentrated by reflecting surfaces. It is on this principle that ear-trumpets and speaking-trumpets are constructed. The former ought to be so made as to receive the greatest possible body of vibrating particles, and to concentrate them by as few reflections and turns as possible. If you go into the shops, you will find very few instruments in which these points are attended to. The greater number render the speaker's voice noisy enough, but they sadly confound his words. A large cone of pasteboard, with a short curved tube from its apex for the ear, makes as good an ear trumpet as can be constructed; and we only want a little mechanical ingenuity to contrive one of this shape that shall be also portable, to bring it into general use. One word more on reflection of sound, which is applicable to this subject. It is generally said that the reflection of sound from a plane surface takes place like that of light, at an angle equal to that in which it fell on it. This is true with regard to sounds at some distance; but when a sonorous pulse of air approaches a reflecting surface, by compressing the layer of air next the surface, it increases the elasticity of that layer, so that it begins to be reflected, or rather *refracted*, before it reaches the reflecting solid. Its course, therefore, instead of being like that of light, thus —



is turned in a course thus—

This is the reason why it is not necessary to construct instruments for concentrating sound by reflection, with the nicety that is indispensable in those for reflecting light; and it is by a similar property that close tubes of air have the power of conducting sound so much further than open ones. But we must conclude this subject, and in the next lecture take up the acoustics of the chest.

If you wish to study sound further—and you would find it both amusing and instructive to do so—I should advise you to consult Sir J. Herschell's Essay, in the *Encyclopædia Metropolitana*, and Dr. Arnot's *Elements of Physics*. These works do not contain all that I have been giving you; for having a new application of acoustics to deal with, I have found it necessary to investigate properties of sound which had not been fully studied before. This investigation has been so far successful, as to enable me to reduce to simple principles all the acoustic phenomena which we have to consider; and I now hope that these principles will prove as intelligible and instructive to you as they have been to me.

LECTURES

ON

RHEUMATISM AND ITS EFFECTS;

By R. MACLEOD, M.D.

Physician to St. George's Hospital.

LECTURE II.

An inflammatory affection of the Heart and its Membranes the most common complication of Rheumatic Fever; this long known in England, though but lately in France—M. Bouillaud's views—Class of cases in which Pericarditis is most frequent; this occurrence erroneously attributed to Blood-letting—Local Symptoms of Pericarditis—Auscultation and Percussion—General Symptoms—Importance of attending to the Expression and Posture of the patient.

By far the most common, as well as the most important, complication of rheumatic fever, is an affection of the heart. Cases of this nature were recorded, now half a century ago, by Dr. Piteairn, since which time many English physicians have written upon the subject, so that the fact of the heart being occasionally subject to inflammation during the existence of acute rheumatism, has long been an established pathological principle in this country. Under these circumstances, it is rather startling to find M. Bouillaud, in 1836,

not, indeed, claiming as a discovery of his own, the fact of pericarditis sometimes occurring in rheumatism, but maintaining that the doctrine of such complication being any thing more than accidental, rests upon the authority of his researches, and which, according to his own account, had only been fully made out during the three previous years.

Ever ready, as some among us are, to attribute to our neighbours in France the superiority in pathological anatomy, it is not a little remarkable to meet with an illustration so palpable as this of the neglect of that which alone can render such investigations practically useful; I mean the application of the knowledge acquired from studying the changes of structure in the dead to the explanation of disease while yet progressive in the living. The observations of M. Bouillaud—more especially his claim to originality—clearly show that, among his countrymen, the closeness of the connexion between rheumatism and carditis was not known; and accordingly, when we turn to the best French authorities, we find that the subject is either altogether omitted or but slightly alluded to. Corvisart, indeed, says he is inclined to regard gouty and rheumatic affections as frequently producing adhesion of the pericardium; but, speaking of the causes of pericarditis, he does not even allude to the coincidence of this disease with rheumatism. The same want of any distinct announcement of the connexion between rheumatism and pericarditis, characterizes the volumes of Laennec, and the more recent works of Chomel. But what is still more remarkable, Louis, in his *Memoir on Pericarditis*, in which the phenomena of the disease, as regards its local and general symptoms, are excellently detailed, says nothing of its connexion with rheumatism. Of all the French writers previously to M. Bouillaud, Andral is the one who makes the most distinct reference to a connexion between acute rheumatism and certain affections of the heart. He relates (in his *Clinique Médicale*, tom. i. 2d edit.) a case of pericarditis which followed an attack of rheumatism, speaking of it as a *metastasis*; but one which he seems to have thought quite as likely to attack the pleura or the lungs as the heart; and he adds that the lesion of function in such cases appears to be greater than the change of structure: expressions which show how little familiar he was with the phenomena in question.

But if M. Bouillaud be not entitled to the merit of priority in establishing the important relation which subsists between acute rheumatism and inflammation of the heart, he deserves credit for the zeal and activity with which he has investigated the subject. Since his attention was

directed to the inquiry, he has met with so many instances of rheumatism of the heart, that he estimates them at not less than one-half of the whole number in which the disease has assumed the character of rheumatic fever. Indeed he goes farther, and assumes the coincidence of the heart affection with acute rheumatism to be the rule, and its absence the exception. Without carrying our opinions quite so far as this, no one who has been much in any of our public hospitals of late years, can fail to have been struck with the very large proportion which the cases of diseased heart bear to the mass of those admitted; and if he has carried his inquiry but a single step farther, he must also have found that a great majority of those labouring under such affections have had rheumatic fever at some previous period, although it often happens that they themselves have not suspected any connexion between the primary acute attack and the secondary chronic disease.

When attention was first directed to the affection of the heart in rheumatism, it was very generally supposed, and is still frequently assumed, that this depended upon a metastasis or change of seat on the part of the rheumatic inflammation: but more extended observation has not confirmed this view, and I have myself so frequently seen cases in which the supervention of carditis was attended by no mitigation of the external rheumatism, that it seems to me quite illogical to apply the term metastasis to this condition. Again, I have known an individual who had been exposed to cold, become affected with pericarditis, having had no pain of the limbs previously, but who has had unequivocal rheumatism in some of the external parts within twenty-four hours after the heart had become affected. The result of my experience, indeed, would lead me to infer, that if an individual has any given set of joints affected with acute rheumatism, such joints are more likely to be relieved if the disease attacks other external parts than if it fix upon the heart. It is probably because we often see that when a second set of parts about the limbs become implicated, those which were first attacked improve, that so many writers have assumed the same to occur with respect to the heart. The pericarditis supervening during rheumatic fever, then, may more correctly be regarded as an extension of the disease, than as a true metastasis.

Another important question relates to the class of cases in which this complication presents itself. Dr. Latham says, "it is incident to all the degrees, and all the stages, and all the forms of acute rheumatism." I fear it will not add to

the credibility of my opinions, that they differ from so esteemed an authority. I acknowledge, indeed, that it is incident to all degrees of rheumatism; but when it is added, "it is not more to be looked for when the disease is severe than when it is mild," I am under the necessity of withholding my assent. According to my experience, the heart affection is much more frequent in severe than mild cases of rheumatism. I do not mean to say that I have not seen pericarditis come on where the external rheumatism has been mild; but merely this—that such examples are rare, compared to those in which the reverse is the case.

Neither do I think that it is equally incident "to all forms of acute rheumatism." Indeed, on referring to my notes, I do not find that any of the cases of heart affection which I have witnessed (save one) have resulted except from one particular form—namely, the general or diffuse kind; in which the fibrous structures are chiefly implicated. It is but right to remark, however, that when Dr. Latham speaks of the heart affection as liable to supervene upon all the forms of "acute rheumatism," he may possibly limit the epithet "*acute*" to that form in which the fibrous textures are the seat of the inflammation.

As some persons labouring under rheumatic fever have, while others escape, an affection of the heart, it becomes a very important question to ascertain the cause of this difference. I am not aware that any circumstance has been specified as tending to produce the pericarditis in such cases, except blood-letting, which has been supposed to increase the risk of this occurrence. So far as my observation extends, this opinion rests on insufficient grounds. I have in many instances, during the last few years, bled patients largely and repeatedly in rheumatism, without as yet having met with one instance in which inflammation of the heart has been induced by it; and I have seen many cases of rheumatic heart, in which the patients had never been bled at all. At Edinburgh, some years ago, blood letting was practised to a great extent in the clinical wards of the Royal Infirmary; and Dr. Watson, who witnessed the treatment, says, in reference to it, "I do not recollect that affections of the heart were much noticed in those cases." Again, blood-letting has scarcely been employed in France in the treatment of rheumatism, and yet M. Bouillaud, as I have said, alluding to the frequency of the complication of pericarditis, regards its presence as the rule, and its absence as the exception—a view of the subject which clearly proves how common he has found it. And this inference, be it observed, he draws, not from those

who had been bled, but from those treated in the ordinary way.

I cannot help thinking that the idea originated in an erroneous theory, and has since been perpetuated by that strong tendency to copy opinions and practice which is so remarkable in the history of our art. Patients having severe fibrous rheumatism were bled; some of them afterwards became affected with pericarditis; the fact of this being almost a part of rheumatic fever was not known, and some extraordinary cause being thought necessary to account for the phenomenon, the blame was charged upon the depletion. But again, the pericarditis not being recognised as one in the train of phenomena in acute rheumatism, was supposed to be a transference of the disease from the external parts to an internal organ—a metastasis, in short; and then, by a natural train of reasoning, those causes, which experience had shewn to lead to other kinds of *metastases*, were assumed to operate similarly here. I believe that this view of the pathology of the affection of the heart which takes place in rheumatism, will be found to pervade the writings of all those who hold that blood-letting in rheumatic fever tends to produce pericarditis. Thus Dr. Alison, in his *History of Physic*, published in the fourth volume of the *Cyclopædia of Practical Medicine*, says he has no difficulty “in stating his conviction, that large and repeated bleedings in the beginning of rheumatism, increase the risk of this *metastasis*.” No ground, however, is assigned for this conviction; and it would clearly require, in order to prove its accuracy, a great number of comparative trials, made in cases as much alike as possible. Thus I have above stated, that, so far as my experience goes, the complication of the heart affection is more apt to occur in severe than in slighter cases; but it is in such severe cases that we most usually abstract blood; and then those who enter upon the subject with preconceived opinions, almost unconsciously dwell upon the depletion, and forget the acuteness of the disease which called for it, when they are seeking out for the causes of pericarditis.

If there be one class of patients more than another in whom blood-letting is comparatively seldom adopted in acute rheumatism, it is children; and what is the fact with regard to *their* liability to heart affections? Are they proportionally more or less obnoxious to this disease? I have no hesitation in affirming, that I have met with more cases, proportionally, in which rheumatic fever had led to disease of the heart in children than in adults. This remark is also in keeping with the experience of Dr. Watson, who says, that

although blood-letting does not cause any predisposition to the cardiac affection, yet that youth certainly does; and farther, that the younger the child is, the more likely is rheumatic pericarditis to supervene. Now, agreeing as I do with Dr. Watson in the statement of the fact, I venture to ask, with regard to its cause, whether this may not consist in that the younger the patient, the less likely is the necessary depletion to have been had recourse to?

Lastly, as regards this part of the question, let us observe the treatment. A patient has rheumatism of the limbs, and it is assumed, that to bleed him increases the risk of his heart becoming affected. But if the heart does become implicated, although the rheumatism still remains in the limbs, we are then told to bleed (moderately it may be, but still to bleed), by the very same parties who hold that to bleed in rheumatism increases the risk of the heart becoming affected! But if bleeding originally transferred a portion of the rheumatic action to the heart, surely bleeding farther must be attended with the risk of transmitting any portion of the disease which yet remains in the limbs, to that organ whose liability to it was first begotten by the depletion.

That blood-letting carried too far, in rheumatism, is productive of certain other injurious consequences, I feel persuaded; but nothing I have seen would lead me to enumerate pericarditis among those evils; and this, I may add, is an opinion forced upon me in the wards of St. George's Hospital, in opposition to the opinions I had previously derived from reading.

The symptoms which mark an attack of rheumatic inflammation of the heart may be divided into those immediately connected with the organ itself, and those manifested by other parts; or, in other words, into the local and general phenomena.

Of the *local* symptoms, that most commonly complained of is pain; yet there is perhaps none to which it would be more fallacious to trust, because it is sometimes entirely absent even in the most formidable cases. When present, the nature of the pain differs at different times. In one it is an acute stitch, aggravated by inspiration, and resembling that of pleurisy; in another it is a dull, heavy, burning uneasiness; but more generally it is of a character intermediate between these—that is to say, the pain is rather acute, but neither so sharp, nor so much aggravated by inspiration, as it is in pleuritis. The pain is distinctly, and sometimes very severely, aggravated by pressure; and may even be thus excited when not otherwise present. To ascertain this, the fingers must

be applied between the ribs, over the region of the heart; or the hand may be applied to the epigastrium, and pressure made upwards, inclining to the left. It will frequently be found that pain is experienced when either of these expedients is had recourse to, even although no uneasiness in the region of the heart has been previously complained of, or at least none of a more palpable nature than oppression and discomfort. Such examinations, it is to be observed, require to be made with a certain degree of caution, otherwise unnecessary pain, if not more important injury, may be inflicted.

In recent and acute cases the pain is generally limited to the region of the inflamed part; but in those who have suffered from similar attacks before, and in whom the heart has become organically diseased, the pain is apt to pervade the left side of the chest generally, and even to extend to the scapula, shoulder, or arm.

The extent to which the heart's action is altered, as indicated by the presence of palpitation, or of any peculiar affection of the pulse, is much less than we should *a priori* have anticipated, or than is often represented in books. Limiting, for the present, the term palpitation to that tumultuous action of the heart which directs the patient's attention to it, and induces him to make it the subject of complaint, I am quite satisfied that it is frequently entirely absent; while, with respect to the pulse, it not only has nothing which can be regarded as pathognomonic, but I should even say that it has no character which is calculated to afford us any material assistance in the diagnosis. Generally, indeed, it is increased in frequency (beating from 100 to 120,) and at the onset is also, I think, usually more jerking than natural; but this latter character often very speedily subsides, and then, although generally retaining its augmented frequency, it may become weak, unequal, or intermitting—all which phenomena, it is well known, may attend other diseases of the chest, more especially those attended with effusions.

But although, as I have said, the heart's action is not so often changed in rheumatic carditis as to excite a form of palpitation, of which the patient complains—as he does in various other affections of that organ—yet I believe there is no instance in the whole range of disease in which so great assistance is derived by an educated ear from listening to its sounds. Now the abnormal sounds met with in rheumatism of the heart are chiefly two; one of which conveys the idea of rubbing or friction, and the other that of blowing or whizzing. The former

of these seems to depend upon increased friction between the surfaces of the pericardium; the latter, upon increased friction of the blood as it passes through the internal apertures of the heart.

The rubbing sound conveys the idea of two surfaces moving over each other; and its occurrence, which is characteristic of pericarditis, has been attributed by some to the membrane becoming dry under the inflammatory action; by others, to the surfaces becoming rough. The latter appears to me the more probable explanation, because we have no evidence of the degree of dryness which is here assumed ever really taking place; while we have proof positive of the surfaces becoming rough, and that to an extent which there is no difficulty in believing to be capable of emitting sound. But as the inflammation must be fully established to produce the rubbing sound, so, though one of the most certain, it is not always the earliest among the symptoms. In more than one case I have suspected very strongly that pericarditis had supervened before there has been any change of sound (sufficiently distinct at least *for me* to appreciate,) and within twelve or twenty-four hours afterwards the rubbing sound has been evident. The nature of this sound is, I think, sufficiently expressed by the term I have employed, and which is that now generally adopted. A rubbing sound may also occasionally, though I believe it very rarely, does accompany the act of respiration; this, however, can scarcely be confounded with that dependent upon the action of the heart.

But as the sound in pericarditis depends upon a cause which is transient, so is it one which does not endure beyond a very limited period; for if either the opposing sides of the pericardium become coherent, or if they be farther separated by the effusion, then, in either case, the attrition ceasing, the sounds are no longer emitted, although the inflammation may still hold on its course. And again, if the disease be overcome, the lymph may be absorbed, and thus the cause of the rubbing sound will equally be removed.

Distinct and unequivocal as this sound occasionally is, yet in other cases, which nevertheless were certainly pericarditis, I have not been able to detect it, neither have those who will sometimes hear a sound so fine

“That nothing lives 'twixt it and silence.”

Were it worth while to speculate on the cause of our being thus occasionally baffled, we might presume that it depends upon some modification in the extent or nature of the effusion.

But there is also another circumstance which operates in producing this negative result; I mean, that we generally have a sound which is louder, more persistent, and more familiar to us, by which the rubbing is masked, and our attention arrested. This is the *blowing* sound in one or other of its numerous degrees and modifications. In many cases it may be heard all over the region of the heart, but is usually most distinct at points corresponding to the aortic and to the auriculo-ventricular valves of the left side: that is to say, that where the relative situation of the parts is not changed by previous disease, the blowing sound will generally be best heard by placing the ear or the end of the stethoscope at the lower part and towards the left side of the third bone of the sternum for the aortic, and a little further to the left for the mitral valves.

Where the "blowing" is heard with the first sound of the heart, and where it is perceived on placing the instrument over the carotid arteries, it is reasonable to conjecture that it depends upon disease having produced, in the aortic valves, certain changes which cause obstruction to the free exit of the blood from the ventricle. If the blowing be contemporaneous with the second sound, and not heard over the carotids, it probably will be found to depend on disease at the orifice between the left auricle and ventricle. Frequently a whizzing sound is heard with both actions of the heart, depending, in general, upon the aortic and mitral valves being both implicated in the disease. The degree to which the blowing sound is present varies much in different instances; but in well-marked rheumatism of the heart, it is rarely, if ever, wholly absent. Its general character, where the valves have not been previously damaged so as to admit of regurgitation, is that of a simple bellows sound; but sometimes it assumes a harsher character, and sometimes a distinctly musical tone. I have heard a clear musical whistle nearly from the onset of an attack of rheumatic carditis, diminishing in loudness as the disease was overcome, and passing into a simple bellows sound, which remained permanent. Indeed, there can be no doubt but that this abnormal sound may entirely cease in recent cases, the causes on which it depends being, it is to be presumed, wholly removed; but in older cases, and especially in those where the heart affection has not been early and vigorously met, the sounds alluded to are permanently present, affording every variety of intensity and tone, and too often resisting every method hitherto attempted for their removal—a

circumstance obviously depending upon irremedial structural changes having been produced: and this leads me to remark, that it is of great importance, in cases of acute rheumatism, to ascertain, at the earliest possible period, the condition of the heart, that we may not confound the phenomena produced by the attack then present with those resulting from previous organic change.

Another phenomenon connected with sound, which is sometimes, though not so generally, present in the first attack of rheumatism of the heart, is the extent to which the cardiac region becomes dull on percussion. In the normal condition the dullness extends over an inch and a half, or two inches square, the lung encroaching on the rest of the surface. But in disease the lungs frequently leave a much larger space over which they do not pass, and consequently the percussion is dull over a corresponding extent. Where this dullness comes on during acute rheumatism of the heart, it may reasonably be attributed to effusion into the pericardium. If the dullness over an increased extent depend upon enlargement of the heart, this will generally be found to have resulted from previous attacks of rheumatism, and scarcely affords any facility in detecting pericarditis, unless, indeed, by giving a larger surface over which the rubbing sound may be heard. If the dullness depend upon effusion, this will rarely assist us in originally detecting pericarditis, although it affords us a means of determining the extent to which such effusion has proceeded, and also whether the disease is on the increase or decrease. It does not usually assist us in detecting the pericarditis, because, before the effusion can have proceeded so far as to produce an unequivocal augmentation in the surface over which the sound is dull, the disease in all probability will have sufficiently manifested itself by other signs. And again, in that form of pericarditis which attends acute rheumatism, the effusion is most frequently chiefly composed of lymph, which, though it may be in quantity sufficient to produce embarrassment in the action of the heart, yet the mere increase of bulk thus produced is not usually to such an extent as to be satisfactorily judged of by percussion. A much more appreciable, and therefore more generally useful sign, in reference to sound afforded by the effusion which takes place into the pericardium in rheumatism, is the sound of the heart itself being observed to become progressively more feeble and more distant, while the general symptoms show that this is not dependent upon mere sinking. It is proper, however, to remark, in

connexion with dulness in the region of the heart, whether from its increased bulk or from effusion into the pericardium, that either may be masked by an emphysematous portion of lung interposing.

Of the *general* symptoms which indicate that the heart has become implicated in an attack of acute rheumatism, the most striking is the aspect of the patient. I venture to say there is no observant physician who has not had occasion, on going into the wards of an hospital, to stop at once on coming to a rheumatic patient whom he may have seen the day before apparently going on well, and proceed to examine the heart with the conviction in his mind, before he has asked a single question or applied his stethoscope, that carditis has supervened in the interval. This is one of the many instances in which the eye can detect what the pen cannot express. The system has taken the alarm at the new inroad of the malady; the consequent distress is depicted in the countenance, and told in every attitude and every movement. The expression is anxious; the breathing rather shallow; occasionally there is cough. The patient is sometimes very restless, but more generally lies on the back, or right side; at least it is rare to see him choosing the left. Here we have intense fever, but for the most part without the restlessness and tossing which usually attend that state. Indeed there is occasionally a fixedness in the general aspect—I had almost said in the deportment of the patient, quite remarkable; he becomes, as it were, passive, and while the immobility with which he retains one position would lead us to suppose that any other would be intolerable to him, yet I have known such a patient, upon being moved, remain in his new position apparently as determinedly as he had previously done in the other. Although, therefore, reclining on the back, or a little to the right, be the most common, and therefore, we must presume, generally the easiest posture, yet the unwillingness to move, even to resume this attitude, probably depends upon the effect produced by motion of any kind on the heart's action, which thus becomes for the time still farther embarrassed. In the cases where this unwillingness to any change of posture is most marked, the action of the heart is usually feeble and the sounds indistinct. Dr. Latham, who has excellently described this phenomenon, attributes it, and probably with justice, to the presence of fluid effusion in the pericardium. And here I may observe that where copious effusion takes place rather rapidly into the pericardium, unaccompanied by hydrothorax, the patient will sometimes prefer to lie perfectly

flat on the back, without having the shoulders raised at all, being just the reverse of what we are almost invariably taught in books. Such, at least, has been the fact in several cases in which I had an opportunity of examining the bodies after death, and in others where the patients recovered, after having had symptoms which led me to believe that there had been fluid in the pericardium.

When the peculiar anxiety of countenance above-mentioned, and the reluctance to change of posture, are most marked, there also will be frequently seen a disposition to syncope. I say a *disposition* to this state, because I think the patient very rarely does actually faint. The pulse, too, becomes unsteady, and varies in the character of its beats, but rarely has any considerable power. There is heat of skin, and thirst, and furred tongue, and sluggish bowels, and high-coloured urine, and all the usual symptoms of inflammatory fever.

Now when either all, or several of the phenomena above described, occur in the course of a case of acute rheumatism, there can scarcely be any difficulty in detecting the nature of the complication which has added to the severity of the primary attack; there can scarcely be room for hesitation in declaring pericarditis to be present.

But there is one very peculiar train of phenomena which may tend to divert the attention of the practitioner from the real seat of mischief, and which requires an especial notice.

ON THE

ACOUSTIC PRINCIPLES OF THE STETHOSCOPE.

By CHARLES COWAN, M.D. E. & P.

Physician to the Reading Dispensary.

To the Editor of the Medical Gazette.

SIR,

WE were much interested by an ingenious paper of Dr. Budd's on the acoustic principles of the stethoscope, inserted in the No. of your valuable journal, for May 29, 1837. We had long felt dissatisfied with the generally received explanation of how the stethoscope transmitted the sounds from the chest to the ear, and suspected that too much importance was attributed to the form of that instrument—to the shape of its internal surface, and to the conducting power of the column of air which it incloses.

Dr. Budd's observations confirmed this impression, and though not demonstrative of the fact, seemed almost to prove that the *solid material*, and not the *air*, was at least the principal medium of communication. The following week your column contained a strong stricture by Dr. Williams on the views entertained by Dr. B., and the latter not having since replied, we beg leave again to agitate the question; and we think that we shall be able to show that Dr. Williams's explanation is not "the only one consistent with the phenomena of auscultation and the laws of acoustics."

Dr. W. commences his communication by expressing "his jealousy of the attempts of those who, labouring in the same field, evince an imperfect knowledge of the science, or want of experience in the art," and seems almost to consider it a matter of course, that those who venture to differ from himself are *de facto* in the wrong; quoting long and numerous extracts from his writings to prove his prior acquaintance with whatever there may be of truth in Dr. Budd's opinions, and terminating by asserting that the latter is in error wherever he is original. Throughout Dr. Williams' remarks there is a tone of authority, a monopolizing of truth, as it were, and an anxiety to preserve favourite opinions from foreign aggression, which in subjects open to discussion had better be avoided; but which at the same time we doubt not are more apparent than real, and may fairly be attributed to the strength of his conviction, and to the intense interest which he has always taken in the subject. We think also that Dr. Budd's merit as an investigator has been underrated, and that the facts which he adduces in support of his hypothesis have not been fairly examined or satisfactorily answered. In the following observations we shall venture to advance some additional objections to the views of Dr. Williams, which he has so long and so ably promulgated; or rather we shall endeavour to strengthen the theory maintained by Dr. Budd.

There is great praise due to Dr. Williams for having so strongly insisted upon the importance of the principle, that sound is best conducted by media of analogous density to the vibrating body, and that although as a general fact the conducting power is in propor-

tion to the density, yet the influence of the condition we have just alluded to is such as frequently to falsify a rule which we have been in the habit of regarding as universally applicable.

The reason why substances of light and elastic texture, such as wood, are better adapted for stethoscopes, is simply because their density approximates more closely to that of the thoracic parietes, and not because they are absolutely the best conductors of sounds. Experience proves that metals and the denser woods are inferior to deal or cedar, when employed in the transmission of vibrations originating within the chest; and experience also proves that a hollow cylinder possesses some advantages over a solid one.

So far, then, no difference of opinion prevails; but we would ask, what application does Dr. Williams make of the principle on which he so strongly insists? The solid part of the instrument, he says, is to transmit the sounds originating in the *dense contents* of the chest, while the air conveys the sounds originating in the *more aerial contents*. "To concentrate the diffused sound of respiration," he continues, "and to expedite the examination by making the stethoscope take in as large an extent of surface at a time as possible, the cylinder is hollowed at the pectoral end into a conical cavity, the apex of which terminates in a central canal; so that all vibrations communicated to the air in this cavity are concentrated into the canal and conveyed to the ear."

Now it is undeniable, that whatever may be the source of vibrations in the interior of the thorax, their sensible effects can only be conveyed to the ear through the secondary vibrations of the parietes, which are more or less strongly agitated as the source of the internal sound originates in a rarer or denser medium. No distinction can therefore be founded upon the different nature of the sounds, as to their origin, because they must all be transmitted to one common medium, and having once caused the latter to vibrate, their further propagation can no longer be facilitated by being again made aeriform. If the vibrations from within are too feeble to act on the parietes, they are, by a stronger necessity, too feeble to produce aeriform vibrations external to the chest; but however slight the vibrations of the parietes may be, is it not far more con-

sistent with theory to admit that they would be best transmitted by a solid of more or less analogous density, than that they should react upon so rare a medium as the air, which must, from the law already laid down, be under such circumstances much less easily impressed? We cannot auscultate the aerial contents of the chest themselves, as Dr. Williams' supposition would require, but are wholly limited to the examination of their effects on the walls of the thorax, and we again say, that as the vibrating surface is always a solid one, whatever may be the source of sound, (whether the distension of the cells by air, or the rushing of blood through the cardiac cavities,) yet having once caused the thorax to vibrate, it must be far more easily conveyed to the ear by a solid than by an aeriform conductor. To suppose the reverse is to unsettle the very law which Dr. Williams is so anxious to establish.

Theory, therefore, is strongly in favour of the inference, that the stethoscope is essentially a solid conductor, under all circumstances.

Let us now turn to experiments.

The two observations on which Dr. Budd principally founds his opinion, are the facts of the respiratory sounds being inaudible, if the slightest interval exists between the ear or thorax and the stethoscope; concluding that so small an interruption to the continuity of the tube would not materially interfere with the aeriform vibrations, were they the means of transmission of the sounds. In reply to this, Dr. Williams justly observes, that the conducting power of air is immensely impaired by divergent vibrations; and we have also satisfied ourselves of this fact by direct experiment. Indeed, if we recollect the reflections of sound which take place in a tube, more particularly if evaded at one end, as is the stethoscope, we can easily understand why the slightest interruption to the continuity should greatly interfere; for all the lateral vibrations would pass off from the open extremity in the direction of their last reflection, and never enter the ear at all.

We freely admit that these experiments are by no means satisfactory, nor are they irreconcilable with the views entertained by Dr. W.; who continues—"Now the stethoscope is also a closed tube, in which all lateral divergence is prevented, and the air within it there-

fore forms an elastic conductor, which transmits from end to end the slightest vibrations; being, as I have before observed, the best conductor for sounds produced in air;" and immediately above he cites in illustration of this, the experiment of M. Biot, in which that observer found that the slightest whisper could be distinctly heard through 3120 feet of iron pipe. And from the differing rapidity of the sound when produced by striking the pipe, from what depended on the voice, he distinctly proved that air was the conducting medium of the latter. The whisper was here heard by the conducting power of the air, and not of the solid, because the medium first impressed was similar in density and continuous with the contents of the pipe. The sound therefore necessarily travelled in that direction. But the stethoscope reposes on a vibrating solid, which transmits its vibrations to the solid material of the instrument in preference to the air, on the same principle as a blow at one end of the pipe was conveyed by the pipe itself, and not by its aeriform contents. Admitting the sounds in the chest to be produced in air, they are not heard by the air, from the simple fact, that no direct communication with the exterior takes place. Had the end of the pipe been closed by a solid continuous with the tube, the result would have been very different, and the parietes of the tube have been the conductor of the vocal vibrations to the ear, if in contact with the other extremity. These are really the conditions of the stethoscope when applied to the chest. The sounds are interrupted by interrupting the contact of the ear or chest with the instrument, because we then interpose a layer of air, which, from its comparative rarity, becomes a very imperfect conductor; while, on the other hand, we re-establish the sound in proportion as we renew the contact with the thorax by bodies of density similar to the texture of the stethoscope itself. In either case, then, ceasing the contact of the stethoscope with the chest or ear must materially interrupt the sound.

Every one is perfectly aware that the respiratory murmur is heard very distinctly through a solid cylinder of wood; through a tube filled with water; but at the same time it is generally admitted that the hollow cylinder possesses certain advantages. Dr. Williams

asserts that if the stethoscope conducts sounds merely as a solid, the central perforation can be of no use. Now this does not at all follow, since by perforating the stethoscope you diminish its bulk and its inertia; you double its surfaces; you modify the amount of its contact with the vibrating body; and render it more easily impressed with feeble vibrations. "*Thinning* a sonorous solid increases the sphere of its vibrations, and *lightness of mass* renders them more excursive," as Dr. W. himself admits: on what principle, then, does he maintain that if air is not the conductor, hollowing the stethoscope *can* be of no use? This savours of prejudice. By lightening the mass we render it more susceptible to slighter causes of vibration, and we can thus explain why this form of instrument is best adapted for conducting the respiratory murmur and vocal resonance, and why a more solid form answers best for more powerful impressions. But to come to more direct proofs. Dr. W. declares "that if in *any kind* of stethoscope we close this tube at either end with a cork, we *vastly* impair the power of the instrument to transmit the sounds of respiration, and of the voice." We have made numerous and careful experiments upon this point, assisted by a pupil, who has also verified every result; and our conclusions are distinctly at variance with the assertion of Dr. W. We will now briefly refer to some of the principal results.

1st. If one or both ends of the stethoscope are accurately closed with cork*, the respiratory murmur is only *very slightly* weakened, and the instrument would still suffice for every practical purpose.

2d.—If the interval between the corks be filled with water, the sounds appeared rather louder and sharper than when the instrument was unclosed.

3d.—If the cone be filled with soft cotton, and the ear end plugged with the same material, the difference in the respiratory murmur was barely appreciable.

* Two stethoscopes were used of exactly similar structure, for the purpose of more rapid comparison. Each experiment was also frequently repeated, to avoid any deception which accidental changes in the intensity of the respiratory murmur might produce; and having for some years devoted much time and attention to auscultation and percussion, some confidence may be placed in our power of appreciating the varieties in the character of thoracic sounds.

4th.—The tube was successively obstructed with dough, wax, paper, at one or both extremities; the only appreciable result was a certain diminution of the sounds, but never to such an extent as to make them not easily heard, or to neutralize the indications of the instrument.

5th.—A tin tube of an inch and a half in diameter, eighteen inches long, and closed at both ends by the same material, allowed the respiratory murmur to be very distinctly heard. Firmly grasping the tube with the hand, very sensibly impaired the conducting power. This was only slightly the case with the stethoscope.

6th.—When the cavity was obstructed by a silk handkerchief, it produced very little, if any, difference in the sounds; filling the tube with water considerably diminished its conducting power. Leaving the extremity of the tube next to the chest open, seemed rather to diminish than increase the sounds.

In all cases accurate contact of the conducting medium with the chest and ear was indispensable. The vocal vibrations were evidently louder and more defined, through the stethoscope closed at both ends and filled with water, than when open.

The advantages of the perforated stethoscope, when listening to sounds produced in air continuous with that in the tube, were very evident.

If an individual spoke, or gently breathed through a stethoscope, and with another applied to the ear we listened at a short distance, avoiding touching, the difference in the sound when the stethoscope was free or obstructed, was at once palpable and considerable. If we applied a large glass receiver of three inches in diameter to the chest, and attentively listened at a short distance from the open but contracted end, a distant feeble murmur could be detected, which was only slightly increased by the contact of the ear with the glass. If a stethoscope was introduced through the neck of the receiver, and all contact avoided with the glass or thorax, the ear could plainly perceive a feeble respiratory murmur, which did not vary by moving the instrument nearer to or further from the chest, but which *wholly ceased* when the tube was obstructed by cork or wool. The only difference in this case, from listening to the thorax with the stetho-

scope at a distance from the parietes, was the presence of the receiver, which became an extensive vibrating surface, and capable of communicating sensible agitation to the air it inclosed; exactly as the tuning key impresses the surrounding air with sensible vibrations when in contact with an extensive surface of wood. These experiments sufficiently prove the necessity of the tube being free when conducting aeriform vibrations. Perforating a tube, as Dr. Williams observes, very materially interrupts its conducting power. But this may be explained by the injury sustained by the fibres which are intersected, and the unfavourable condition of the tube as a whole; on the same principle, that a slight crack interferes with the vibrations of a cup or glass. It may also depend (and perhaps this is the true reason) on the numerous echoes of external vibrations in the tube; and at all events is not demonstrative of the conducting power of the contained air, in the way Dr. W. would suppose.

It may still be objected, that if the solid material of the stethoscope is the conducting medium, the vibrations must ultimately be transmitted to the air in the external meatus, and that the difficulty involved in a change of medium still remains. Dr. Budd supposes that the solid portions of the auditory apparatus convey the sounds, but this strikes Dr. Williams as so absurd, that he says, p. 351, "Nay, according to Dr. Budd, these sounds being transmitted to the organ of hearing, not through the air in the external meatus, but through the cartilages, they should be heard as well with the ear closed or stuffed with any dense matter." Now we hope Dr. W. will not turn a deaf ear to us when we assert that this is actually the case. The ear, when stuffed with cotton, and when insensible to the majority of sounds around, gave no evidence of any obstruction when applied to the stethoscope with the tube either open or closed. We seemed scarcely aware that the meatus was obstructed so long as we remained in contact with the instrument, but the moment that we listened to sounds taking place around us, the confusion and indistinctness were painfully evident. My pupil, in his zeal, introduced a piece of cork, which fitted closely, into the meatus, covering it with cotton, and

the whole with a layer of putty; and he was not sensible of any increased difficulty of hearing the respiratory murmur. When both ears were obstructed, the same results were obtained. Every one admits, that where it can be applied, the respiration is as distinctly heard by immediate as by mediate auscultation; which certainly ought not to be the case if the stethoscope concentrated the sound in the way in which Dr. Williams supposes.

We may also mention that a recent German writer has made some very interesting experiments upon the auditory functions, plainly proving that a great many sounds are transmitted through the solids of the body, and not by aeriform vibrations; and he considers that the cochlea is the particular seat of these sounds, in consequence of the direct contact of the nerve with the solid lamella of bone which that portion of the ear contains.

The circumstance referred to by Dr. Budd, that individuals who are deaf from diseases of the tympanum are sensible to sounds conveyed through a solid in contact with the cartilages of the ear, may also be again mentioned.

From all these facts and considerations, we feel justified in concluding that the stethoscope acts almost entirely as a solid conductor, under all circumstances, in reference to the sounds within the chest, and that the column of air which it incloses exerts little, if any, influence upon its efficiency as a conducting medium;—that the superiority of a perforated cylinder over a close one, simply depends upon the former being more susceptible, by its form and lightness, of transmitting vibrations too feeble to affect a more solid instrument; but that the benefit obtained has been greatly overrated, and the explanation hitherto given erroneous.

We are inclined to believe, that the best form for a stethoscope would be a simple cylinder of light wood, about an inch and a half in diameter, and a tenth of an inch in thickness, carefully closed at both ends by a layer of the same wood, and of the same thickness. The exact length is of secondary importance.

This would combine lightness and uniformity of material, a vibrating surface in contact with similar media on both sides, and great simplicity of structure. It is a point for consideration,

how far the portion of the thorax corresponding to the stethoscope is better situated for transmitting the vibrations from beneath, when only in contact by its circumference, or when compressed by a uniform flat surface. We cannot at present satisfactorily determine this. The effect of putting the stopper into the instrument is merely to diminish its vibrating power, and increase its connexion with the chest.

The power of more distinctly localizing certain sounds, under these circumstances, may be accounted for by its less easily transmitting the vibrations produced in the vicinity of the exciting cause.

In conclusion, we would remark, that we have not stepped forward in the character of a discoverer, but simply in support of the conclusions entertained by Dr. Budd. We think the facts now adduced less problematical than those on which that gentleman has formed his opinions; and we cannot see how they can possibly be reconciled with the theory so ingeniously defended by Dr. Williams, and hitherto so universally received. Though differing from the last-named individual on the present occasion, we are not insensible to his great merit as an accurate and sagacious observer; and while we refuse to submit to his authority, we willingly seize the present opportunity to thank him for the benefit we have often derived from his instructions.

Hoping that you will give these observations a place in your valuable journal, we remain, sir,

Your obedient servant,

CHARLES COWAN.

Reading, Sept. 23, 1837.

ON SCURVY.

From the Annual Report of

DR. MURRAY,

Principal Medical Officer at the Cape of Good Hope, for 1836.

Communicated by Sir James McGrigor, Bart.

[Continued from p. 945 of last vol.]

Report of Assistant-Surgeon Ford,
continued.

It certainly appears very remarkable, that the men of the 75th regiment should have suffered so much from scurvy, at all the posts occupied by them in the (late) New Province; and that it should

have affected them in a manner that may be considered endemial, when those of another corps (72d,) stationed in the same province, and under exactly similar circumstances, should have wholly escaped it at two of the posts, (Forts Murray and Beresford,) and resisted its influence for a considerable time at the third (Fort Cox,) which I have already stated was particularly exposed to cold and vicissitudes of climate; and when the disease did, at length, appear in this detachment, it was to a very limited extent. It must be stated, however, that the men of the 72nd regiment were relieved and moved to Graham's Town from the New Province in the end of October, about two months after the scurvy had shewn itself among them.

Within the colonial and neutral-ground border, (*i. e.* to the west of the Keiskama River,) although men of other corps, *viz.* of the 27th, 72nd, and Hottentot cavalry and infantry, were encamped or hutted in precisely the same way as those of the 75th regiment in the New Province, had similar duties, worse water, and were not better supplied with vegetables, (except at Fort Wiltshire,) only a few sporadic cases occurred: why, therefore, the 75th regiment should have been so obnoxious to its invasion, and suffered so severely from its effects, is to me, I confess, unintelligible.

I have not heard of a single instance of scorbutic diathesis having been elicited among the coloured natives; and from the inquiries I have made, I should imagine the disease to be unknown to them. It may, then, with reason be inquired, to what their immunity is to be attributed; exposed as they peculiarly are, to the vicissitudes of the climate, and to cold and moisture, from their almost total want of clothing, imperfect housing, and general habits.

In the first place, I conceive, that being trained from early infancy to endure the change of seasons and of weather without clothing, habit enables them to resist external influences, and renders their system insensible to, or insusceptible of, effects from those sources which to civilized man would be productive of most serious evils. The Caffre appears to be equally insensible to the sun's intensest rays, to nipping frosts, to piercing winds, and to winter rains; he neither diminishes nor increases his clothing to meet any of these changes; and

yet we find that he is less subject to diseases depending upon exposure to them, than man in the civilized state, who takes much care to provide against their influence.

Secondly, these people being essentially pastoral, are by nature and education addicted to active and exhilarating occupations, in which the mind and body are both engaged, such as—the chase, herding their cattle, dancing, and endeavouring by forays, conducted with activity and craftiness, to increase their means, form their chief pursuits; and few if any of them, I should say, are troubled with *emui*.

Thirdly, their general habits of temperance, and the nature and qualities of their diet, form not the least powerful preventive to the disease under consideration. Animal food forms but an occasional and small portion of their aliment; farinaceous and other vegetable substances, such as Indian and Caffer corn, pumpkins, melons, some indigenous wild fruits and esculent roots, with honey and sour milk, which are all essentially antiscorbutic, being their usual food.

The same may be said of the Hottentots, who are also inured from earliest life to atmospheric vicissitudes, to which their nomadic life exposes them; and although they are usually denominated an indolent race, I think that this character refers to them more particularly when constrained to perform services in civilized life. They are not fond of confinement, or regular routine of duty; but in their attachment to hunting, play, dancing, singing, and wandering from place to place, they evince a disposition and fondness for varied occupations.

Those Hottentots who were employed as soldiers, and who did duty in the New Province along with the 75th regiment, when scurvy prevailed there, had warmer clothing, and better lodging, than most of them had been accustomed to; and their practice of keeping fires on their floors within their huts, (as the Caffers also do,) no doubt counteracted the effects of dampness. They had the same rations as the European soldiers; and their knowledge of indigenous roots and herbs supplied them with additional vegetable aliment.

At this post, (Fort Wiltshire,) separated from the New Province only by the Keiskama River, and only 18 miles distant from Fort White, where scurvy

prevailed, the malady never broke out, and this exemption may be fairly imputed to the combination of preventive means enjoyed by the men composing the garrison. These were—the barrack comforts of substantial houses; bedsteads and bedding; the regular supply of good wholesome rations of bread or biscuit and fresh meat; a tolerable supply of vegetables; facilities of procuring additional dietary luxuries, of which the men availed themselves; attention to cleanliness; no over fatiguing duty; and amusing sports.

Two cases of the disease do appear in my return as having occurred here, viz. in men of the 27th regiment; these, however, were of a doubtful nature, the only symptom being an unhealthy state of the gums, (perhaps habitual to them,) rendering them more easily made to bleed than natural. Having detected these at inspection, and dreading the occurrence of scurvy among the men under my charge, I ordered them at once into hospital; where being detained, the one for eight, the other for sixteen days under observation, (with only a few saline aperients, and a daily allowance of vinegar, as remedial agents) I was satisfied that the state of their gums did not arise from actual scurvy, and discharged them.

I may further remark, that several men of different corps underwent solitary confinement in the cells at this post, for periods varying from twenty to forty days during the time of the prevalence of scurvy in the New Province, not one of whom shewed the least symptom of the disease.

Numerous theories and hypotheses as to the proximate cause of scorbutus have been advanced, which are important to be considered, as being the foundation upon which the different modes and indications of treatment have been constructed. The chief points of controversy have been, whether the disease is to be attributed to a primary deranged and faulty state of the fluids, and of the blood especially, or to a morbid condition essentially pertaining to the solids; but by far the greater number of physiologists and physicians are in favour of the former.

Admitting the proximate cause to consist in a morbid change in the condition of the blood, it would not be difficult to account for the atonic or asthenic state of the solids. The primary

tissues, particularly the muscular fibres, being in great part made up of the fibrine of the blood, if this fluid be vitiated, it is evident that the muscles must also become affected; and that the fleshy fibres will become weak, lax, and flaccid. In the combination of both these states, it is probable that the true nature of the disease consists; and thus the more prominent symptoms of ecchymosis and tendency to hæmorrhage may be accounted for.

As to the nature of the deteriorated condition of the blood, there are also different opinions. Cullen maintained that it consisted in too great retention of saline particles in the circulating mass, and explained this to be the consequence of interrupted secretion. Stevens, on the other hand, from experiments which he adduces as proof, insists that the black colour of the blood is owing to a *deficiency* of its saline compounds. Others explain it to arise from a deficiency of oxygen; or a diminished coherence of its molecules;—or the absorption of crude ill-digested chyle, &c.

The most important result of these hypotheses is, the influence they have had over the principles of treatment. Hence we find acids and fermented liquors extolled by one physician; substances abounding with vital air, and the inhalation of oxygen, by another; abundant supply of nutritious food with tonics, by a third; and the exhibition of saline remedies by a fourth.

The next question is—whether this disease be of a dynamic or adynamic character? On this subject I am not

quite prepared to offer an opinion, not having witnessed the disease throughout its several stages; but I am inclined to believe, that it partakes of the former in sanguine and plethoric habits, and at the commencement of the complaint, when symptoms of congestion, or increased determination to any of the viscera, probably exists; while the feelings of exhaustion, depression of energy and spirits, the bloated countenance, and ecchymosed state of the surfaces, which mark its progress, would seem to indicate its having merged into the latter.

With respect to the pathology of scurvy, as deduced from the morbid appearances observed on dissection, I am not prepared to make any remarks, never having witnessed such an investigation.

It remains for me to give some account of the cases I have had under treatment; and more particularly of such as I have conceived to be of more than usual interest.

The total number of patients I admitted under the head of SCORVUTUS during 1836, amounted to 45; among these were the two cases in the 27th regiment which I have already stated as occurring at this post; but which I now deduct, as I conceive they were not of true scorbutic character.

The remaining 43 were all received from other posts, chiefly within the New Province; and the following table will shew their relative proportion as to the number received in each month, the corps to which the men belonged, and the posts from whence they were transferred.

Corps.	Number admitted in each month.					Name of Post from whence received.						Total.
						To the Westward of Keiskamma River.			In New Province.			
	August.	September.	October.	November.	December.	Fort Wiltshire.	Double Drift.	Fort William's.	King William's Town.	Fort Cox.	Fort White.	
Sappers.....	0	0	0	2	1	0	2	0	1	0	0	3
27th Regiment..	0	0	0	1	0	0	0	1	0	0	0	1
72d Regiment ..	0	1	5	0	0	0	0	0	0	6	0	6
75th Regiment..	2	2	4	4 11	10	0	0	0	21	8	4	33
Total	2	3	9	18	11	0	2	1	22	14	4	34

The other scorbutic cases from King William's Town, Forts Warden, Wellington, and Waterloo, in the New Province, passed to Graham's Town and Bathurst by a different route.

The majority of these 43 men were received by me in an advanced stage of the disease, having been treated in the beginning at the posts where they became affected; others of them had not been subjected to any treatment; and some were convalescent on arrival.

The symptoms, as in other diseases, varied in different subjects, but the following were always observed in the severe cases;—fungoid and discoloured gums, with tendency to bleed; fetid breath; depression of spirits; great prostration of strength, and disinclination to move; pulse variable in frequency, but always feeble; attacks of oppressed breathing, with pain and constriction at the chest; sometimes spasmodic palpitations during sleep; tendency to syncope on sudden exertion, or exposure to cold air; extensive ecchymoses and subcutaneous discoloration on the thighs, hams, and legs; rigidity and induration of the cellular and muscular tissues, and contraction of the knee and ankle joints; sallow and bloated countenance; coldness and dryness of skin; urine scanty, and prone to rapid decomposition, with fetid odour. The state of the chylopoietic organs did not appear to be deranged, the bowels being usually regular; and although the alvine evacuations varied in appearance in different cases, they were not in any instance of unnatural or peculiarly offensive character. In one case epistaxis frequently occurred; in another, the skin was *anserine*, and had a measly appearance, with much itching; in a third, a dark, bloody fungus, called by seamen "*bullock's liver*," arose on the leg; and in a fourth, in which the right leg was considerably enlarged and ecchymosed, small pendulous bodies, resembling purple grapes, sprung from the gums.

In the treatment of all those patients I was guided by the chronic nature of their symptoms, and the evident asthenia, and general cachexy, which they presented: my practice was accordingly conducted on the tonic principle, which was chiefly fulfilled by dietetic means, combined with the use of saline remedies; and I also studied to excite a

moral influence of a cheering nature, by promoting amusing occupations, with moderate exercise.

I must remark that during the period I had the management of these cases I was not acquainted with the antiphlogistic mode of combating this malady, lately recommended; but even if I had. I much question whether I should have considered its adoption prudential in the advanced stages I found them; and whether it would have been attended with those salutary effects which it has been said to produce when employed in the early periods of the disease.

I do not wish to be considered sceptical, or prejudiced against the treatment by depletion with the lancet and purgatives: on the contrary, in the early stages of scurvy, I conceive it to be based on sound pathological reasoning, and I acquiesce in the opinion, that "the lassitude felt at the commencement of the disease arises from oppression of the nervous and vascular systems;" and when symptoms of engorgement or determination to any viscus manifest themselves at the commencement of the attack, early and judicious abstraction of blood would appear to be the means most likely to remove this oppression; but, if the disease be not arrested at this period, and by long protraction has assumed the asthenic character, the patient having become sallow, dejected, feeble, apathetic, and disease of the lymphatic system appearing to predominate, I am of opinion that this practice is contra-indicated, and that other means must be adopted to restore the vitiated condition of the blood.

For the fulfilment of this object I conceive that the removal from the locality where the disease has been contracted, together with a moderate tonic corrective and nutritious diet, and saline medicines, attention to general comfort, and promoting mental amusements and bodily exercise, are the means most to be depended upon.

In my patients the benefit derived from removal from the place of its generation, and the advantages gained by change of air, were remarkable; an evident improvement having usually been observed within a few days after their arrival at this post.

Even in the worst cases the functions

of the stomach did not appear to be in the least deranged; the appetite being unimpaired and free from caprice, and the process of digestion performed with apparent regularity and ease.

The ordinary diet allowed to each of the scorbutic patients daily, consisted of coffee, with sugar and milk, and a quarter of a pound of bread, for breakfast; half a pound of bread, half a pound of fresh meat made into soup, with fresh vegetables and rice, and salt *ad libitum*, for dinner; and boiled rice, with milk and sugar, for supper. In addition to these they got two gills of good Cape Madeira wine, or, when it was not to be had, half a gill of French brandy, with water and sugar, at their dinner; with half a gill of vinegar daily, and on every alternate day an orange, or half a lemon, as they could be procured.

In most cases the use of vegetables and sub-acid fruits moderately relaxed the bowels, and in two instances produced a slight diarrhoea; so that purgative medicines were rarely required; when an aperient was called for, $\mathfrak{z}\text{ij}$. of Epsom salts in bitter infusion, along with some cordial tincture, or given in a state of effervescence, by adding carbonate of soda and acid, answered well.

I employed gargles of solution of chloride of lime for the mouth, and friction, with oily and stimulating liniments, for the limbs.

Under the foregoing treatment, amendment and convalescence proceeded rapidly; in only two cases was recovery protracted beyond a month, and many returned to their duty in a much shorter period.

As improvement took place, the sleep became undisturbed by palpitations of the heart and oppression of breathing; the livid discoloration and indurated swelling of the limbs gradually gave way; the skin became warm and soft; the tendency to fainting ceased; the spongy condition of the gums subsided; and the spirits became cheerful. The symptoms last to disappear were pains across the loins and chest, especially the latter; and for some time after convalescence they were easily re-excited by any unusual exertion.

[To be continued.]

SOME OBSERVATIONS
ON THE
PHYSIOLOGY AND PATHOLOGY
OF THE PORTIO DURA.

To the Editor of the Medical Gazette.

SIR,

THE following case was admitted by Mr. Arnott, and fell under my care during his temporary absence. Some circumstances in it may perhaps be interesting to your readers.

Your obedient servant,

ALEXANDER SHAW,
Assistant-Surgeon, Middlesex Hospital.

Davies' Terrace, Brook-street,
Oct. 15, 1837.

*Injury of the Head, with partial
Paralysis of the Face.*

Geo. Kearsley, æt. 20, was admitted on the evening of September 3d, with a severe injury of the head, received by falling out of one of the carriages of the Birmingham Railroad, when the train was at its most rapid speed. It appears that, being intoxicated, and sitting on the end seat of an open carriage, his hat fell off. He turned sharply round to recover it, when he was suddenly thrown from the vehicle, and alighted a little way from the iron rails. When brought to the hospital he lay comatose, breathing heavily, occasionally vomiting, and when roused, he struggled and became noisy. There was an open contused wound of the scalp, near the tuberosity of the occipital bone, a little to its left. Blood flowed freely from the right ear. His head was shaved, and cold lotions applied.

Sept. 4th.--It is difficult to rouse him; the pupils contract on exposure to the light; he vomited, in the morning, a quantity of bile; his bowels have been freely evacuated by a dose of calomel and jalap, followed by a colocynth enema. He was ordered—

Calomel, gr. ij.; Antimon. Tartariz. gr. $\frac{1}{2}$, every four hours. A poultice to the wound. The cold lotions to be continued.

5th.—Having had some increased heat of skin, attended with flushing of the face, twelve leeches were applied to the head. He lies crouching in bed.

Mustard cataplasms have just been applied to the calves of his legs; and these, together with the leech-bites, appear to make him restless; nevertheless, he is difficult to rouse. The blood still oozes from the tube of his ear.

6th.—The muscles of the right side of the face (except those that move the jaw) are found to be paralysed. On a close examination, these are the only parts similarly affected. The sensibility of the side of the face is not impaired. He protruded his tongue, after much rousing. The nurse informs us that he exhibits signs of being desirous of using the close-stool, and gets out of bed for that purpose with her assistance. The bleeding from the right ear has ceased.

7th.—The symptoms are nearly the same. Although the distortion of the features is more remarkable than it was yesterday, the right eye is closed as in natural sleep; and even after the eyelid is raised, it descends again, so as to cover the eye. A considerable ecchymosis is now seen behind the ear.

The pills to be taken at intervals of six hours. A blister to be applied to the back of his neck.

11th.—As it appeared, from his frequently raising his hand to the right side of his head, that he suffered some pain at that part, six leeches were applied; and it was observed that this was followed by an improvement in the condition of his face.

The pills to be taken three times a day.

13th.—The features are now so near being equally balanced, that the distortion of the face can only be perceived with distinctness when he is roused, and uses the right side of his face.

16th.—All his symptoms are improved, but the amendment in his face has not continued. He was found at the visit with a book in his hand. For the last three days he has had a constant hacking cough, as if he were clearing off mucus from the palate. On inspecting the throat, the uvula was found to be turned obliquely to one side, so that its point was directed towards the right amygdala; yet, when he inspired, the velum was elevated on both its sides. The eyelids were observed to be closed, as before, although their margins did not quite meet; and after elevating the upper eyelid, it descended again when he was asked to wink. But it was no-

ticed that the eyelid did not descend with any degree of force; on the contrary, it presented a looseness and flaccidity which proved that the orbicularis was passive. On narrowly watching this motion, it was seen to depend, in a principal manner, upon the revolving of the globe of the eye, which takes place when we wink; for it was only after the cornea had been elevated, so as to be hid behind the raised eyelid, that the eyelid dropped over the white part of the eye.

He was ordered to use an astringent gargle, and to rub the skin behind and under the ear, twice a day, with mercurial liniment. Simple dressing to be applied to the wound, in place of the poultice. The pills to be discontinued.

23d.—He has been out of bed for some days. The eye is slightly inflamed, and he finds it weaker than the left, especially during candle-light.

A blister was ordered behind the ear.

27th.—Hitherto, owing to the general stupor with which he has been affected, it has been difficult to ascertain to what degree his hearing was injured on the right side. At one time it was our impression that the sense was entirely lost; but he now hears apparently quite well on this side, and only complains of a crackling noise in the ear.

October 6th. — When asleep, his eyelids were noticed to be perfectly closed; and he afterwards mentioned that it was without his consciousness that they were shut. A marked difference, however, is observed in their position when he is awake. A short time ago, when he lay comatose, they were always shut; and even after he recovered, the eye-lids closed when he made the attempt to wink; but now they remain wide apart, and the upper one no longer descends, even if he winks with all his force. Indeed, it appears raised to a higher level than before.

10th.—He has been made an out-patient: a second blister was lately applied behind the ear; he has also continued rubbing the cheek frequently during the day, to excite the muscles. There is a manifest improvement in the appearance of the face, but he cannot close the eye any better.

REMARKS.—The circumstance that gives the principal interest to this case, is the occurrence of partial paralysis of

the face, in connexion with concussion of the brain. Previous to our improved knowledge of the functions of the nerves, such a combination could not have been explained in any satisfactory manner. No reason could have been assigned for certain muscles of the face alone being paralysed, while others immediately adjoining those that were affected retained their power; nor could it have been understood how any of the muscles of the face should have become paralysed without the integuments being deprived of their sensibility. The only way of accounting for this kind of paralysis would have been by supposing that the injury to the brain, occasioned by the fall, had given rise to it. Yet such an explanation would only have increased the obscurity that already characterizes the symptoms resulting from blows on the head; for no lesion of the brain hitherto described has ever been found to produce a loss of power so limited in its nature as that presented in the foregoing case. When it is known, however, that each nerve to the brain is endowed with its appropriate function, it becomes an easy task to distinguish between the effects of a local injury to one of them, in its course from the brain, and the symptoms denoting a general disturbance of this important organ itself. Here, for example, no doubt can be entertained, that independently of the violence of the brain giving rise to the usual symptoms of concussion, the portio dura of the seventh pair has received an injury in some part of its course; and that this has occasioned the partial paralysis of the face. The portio dura, it has been ascertained, alone controls the particular muscles that have been paralysed; it is known, also, that in its passage through the bones of the skull, it pursues a different route from the rest of the nerves; it can easily be understood, therefore, that it alone may have been injured, while the others escaped. Such an explanation will account for the sensibility of the skin, and also for the power over the muscles of the jaws remaining unimpaired, since neither of these properties depends upon the portio dura, but they are both conferred by the fifth pair, which takes a distinct course through the bones from that nerve.

This case is a correct parallel to others that I have witnessed. Indeed, it may be stated that partial paralysis of the

face, of the same kind as was exhibited in this patient, is not an uncommon effect of injuries of the head. It is, at all events, remarkable that from such accidents the portio dura is more liable to be injured than any of the other cerebral nerves. There is evidence of this in the recorded cases of affections of the different nerves of the head. If we refer to the papers on Partial Paralysis*, by Mr. John Shaw (containing the first illustrations submitted to the profession, of the practical benefits to be derived from the discoveries in the nervous system), or to the collection of cases in Sir Charles Bell's work on the nerves†, where a most extensive series of local nervous affections from different causes is given, it will be found that a considerable number of these resulted from blows received on the head. Ten such cases, at least, may be comited. Now in all of these the portio dura was injured; and in one alone, characterized by Mr. Shaw as uncommon in its occurrence, the fifth pair was partially involved in the injury, along with this nerve. Hence it is interesting to inquire, what is remarkable in the anatomy of the portio dura, that should render it so peculiarly subject to have its function destroyed in these cases of blows upon the head. The explanation, as it appears to me, is to be sought for by attending to the course which this nerve takes through the bones of the cranium, and by bearing in mind, at the same time, the principle of *contre-coup*.

It is by a circuitous route, through a canal of some length and complexity, that the portio dura pierces the temporal bone. Now, it is at this very part of the skull that we learn, both by experience and by studying the forms of the bones on the principle referred to, that the vibrations caused by a severe blow on the head are most powerfully felt: it is here that fissures produced by injuries received at distant parts of the skull are most frequently found. We cannot be surprised, therefore, when the portio dura (which, notwithstanding its name, is extremely delicate in its texture) has to pass through the temporal bone, inclosed in a narrow canal, with boundaries of such great density,

* Quarterly Journal of Science, 1821. Medico-Chirurgical Transactions, 1822.

† Bell on the Nerves. Third edition, 1836.

that it should be peculiarly liable to have its function destroyed by the vibrations of the skull. Blows may be inflicted on the head, of every different degree of violence short of producing actual fracture of the bone, which will, at the same time, cause a considerable spurring out of the bone at the temple, and consequently a shock extending through the canal that contains the portio dura; and no one can venture to say what slight degree of concussion communicated to the nerve thus enveloped may not produce the immediate loss of its power; or by occasioning the effusion of blood, or of serum, or of lymph, around it, give rise to the subsequent destruction of its function. Just as hæmorrhage from the ear, which was one of the symptoms in this case, may either indicate a trifling laceration in the tympanum, or a fracture extending through the base, so may paralysis of the muscles of the features indicate either a slight or a formidable injury to the temporal bone. But let this opinion be correct or not, I conceive that I am borne out in saying, that the frequency with which paralysis of the face, hæmorrhage from the ear, and, I may add, effusion of blood at the temple from rupture of the meningeal artery, attend violent blows on the head, is to be accounted for by the tendency which the vibrations have, on the principle of *counter-fissure*, to be concentrated towards the temples.

Without hazarding an opinion as to the exact nature of the injury received by the nerve, in this particular instance, I may be allowed to make a few observations on the subject. When we consider the kind of accident that befell the patient, the state of insensibility in which he lay for more than a week, and that the hæmorrhage from the ear lasted even for three days, we cannot help concluding that there must have been a fracture extending across the temporal bone, lacerating the portio dura in its canal. A fissure through part of this bone might have existed, even although the symptoms were only those of concussion. Nor will the recovery of the patient be offered as an argument against this opinion. I have, in the museum of the school, two interesting specimens of skulls, exhibiting fissures (one complicated with fracture) extending into the temporal bones in both instances; the fissures approach

closely to the canal for containing the portio dura; yet it is obvious, from the signs of reparation, that the individuals from whom they were taken survived the accidents. But if there had been a fracture, in Kearsley, we should have expected to find, along with the hæmorrhage from the ear, some puffiness or discolouration of the integument at the temple. It may also be argued, that had the injury to the bone been of such a severe nature, the paralysis would have immediately succeeded the accident, instead of appearing, for the first time, on the fourth day. Besides, the manifest improvement in the condition of his face, shortly after the first accession of the paralysis, is inconsistent with the idea of the nerve having been torn through. It is to be presumed, likewise, that if the bone had been fractured to any extent, the portio mollis could scarcely have escaped; yet when the patient left the hospital, his hearing was scarcely, if at all, impaired. In a case that occurred, not long ago, in the same ward where Kearsley was lying, a patient had partial paralysis of the face, similar to that with which he was affected, and produced from the same cause, a fall from a height: like him, he had also hæmorrhage from the ear corresponding with the paralysed side; this hæmorrhage continued for some days, when it was succeeded by a copious discharge of clear serum from the ear, which only ceased when the patient died, which was in the course of about ten days. It was found, upon examination, that a fissure extended across this side of the skull, towards the foramen magnum; that the dura mater had been torn where it lines the temporal bone; and that this had permitted the serum, secreted by the membranes of the brain, to distil through the crevices of the bone and escape by the external ear. The preparation of this bone which is in my possession, shows that both the portio dura and the portio mollis have been torn across in the interior of the petrous portion: and it leads us also to reflect on the improbability of these nerves ever recovering their functions after such an accident. It will, perhaps, therefore be more consistent with the facts observed in the case of Kearsley, if we consider that instead of there being a fissure extending completely through the bone, there was only, in consequence of the

shock communicated by the fall, a partial breaking up of the thin partition in the interior of the temporal bone that divides the portio dura from the cavity of the tympanum. Such a fracture would be attended with laceration of the lining membrane of the tympanum, and might have given rise to the hæmorrhage from the ear, without being sufficient of itself to disturb the functions of the portio dura, so long, especially, as the blood continued to escape from the ear. When, however, the blood ceased to flow, it is probable that it accumulated in the tympanum, coagulated and pressed upon the nerve, thereby producing the paralysis. This corresponds with the manner in which the paralysis of the muscles took place; for it did not commence till the hæmorrhage from the ear ceased; and it corresponds also with the partial recovery of the patient, for it is not unlikely that the effused blood would be gradually absorbed in part.

In the narrative, a peculiarity was noticed in the appearance of the uvula, which deserves some attention. I must state, however, that the appearance to which I refer has been observed in such a small number of cases that it is questionable whether I ought to draw any conclusions from it; but perhaps the observations I am about to make will induce others to attend to the subject. On looking into the patient's throat, and directing him to inspire, the uvula was seen to turn obliquely to that side on which the muscles of the face were paralyzed; and this appearance continued to be observed during all the time he was in the hospital. The question therefore arises—had this condition of the uvula any thing to do with the affection of the portio dura? Now this will not appear an improbable supposition, if we recollect that a branch of this nerve may be traced, if not directly to the soft palate and uvula, at least into that plexus of nerves which supplies these parts. A twig derived from the digastric branch of the portio dura joins the pharyngeal plexus; and anatomy, which is a better guide than experiment, in questions relating to the functions of nerves so deeply seated, would lead us to conclude that, as it is through this plexus that the motions of the soft palate are performed, the branch of the portio dura that assists to form it may possess

a certain influence over the muscles which move the palate*.

The fairest and most unexceptionable mode of verifying this observation will be to examine the appearance of the soft palate in numerous cases of disease affecting the portio dura, while it is still contained within the temporal bone—that is, before it has given off the digastric branch. Hitherto I have only observed a similar condition of the palate in one other case; and it was favourable for the observation, as the paralysis of the face arose obviously from the nerve being injured in its course through the temporal bone. The following is the memorandum that I made:—"On looking into his throat, and making him inspire, the uvula became corrugated, but there was an apparent distortion of the arches of the palate; they were contracted on the side corresponding with the sound side of the face, and relaxed on the other." In a thesis by M. Montault, on Hemiplegia of the Face, I find that he likewise has observed an obliquity in the arches of the palate, in a patient who had partial paralysis from disease of the portio dura; but the description that he gives of the case is not sufficiently exact to allow me to draw any conclusion from it. If future observation shall confirm the idea that I have here offered (and I beg to repeat that I propose it with considerable diffidence), that the motions of the velum palati are in some degree, for I do not suppose that they are wholly, subject to the influence of the portio dura, it may then be asked what is the meaning, in the case that I have related, of the point of the uvula having been turned towards the paralytic side? Now this is the very position which we might expect the depending part of the uvula to assume, by the contraction of the muscles of one side of the palate alone. As the levator palati draws the velum nearly directly upwards and backwards, it will not have any considerable influence upon the uvula; but as the circumflexus or

* I performed the experiment of cutting across the portio dura in a dog, with the view of ascertaining whether it would produce any effect on the motions of the palate; but I confess that I could not obtain any satisfactory result. To divide the branch that joins the pharyngeal plexus, it is necessary to cut very deeply; and the injury to the adjoining parts is consequently so extensive that no legitimate conclusion can be drawn from the experiment.

tensor palati stretches the palate laterally towards the hamular process of the sphenoid bone, round which its tendon winds, it will draw the highest part of the uvula that is attached to the centre of the velum to that side, and, by so doing, cause the point of the uvula to turn obliquely to the opposite side. In short, we may expect the same to occur with regard to the uvula that takes place in the tongue: if the muscles of one side of the tongue be paralyzed, and the patient be made to hold it out, the tip will be inclined towards the palsied side; the explanation of which is, that owing to the muscles at the root of the tongue on the sound side operating most powerfully, the root itself is drawn towards the sound side, and the point is consequently directed towards the paralytic side.

I may now be permitted to make a few remarks on the condition of the eye-lids. It was observed, for about a fortnight after there were the most unquestionable signs of the muscles of the forehead and eye-brow, as well as of the cheek, nostrils, and mouth, being paralyzed, that the patient continued to keep his eye shut, and when the eye-lid was elevated it descended again, in correspondence with the left eye. These appearances caused some who witnessed the case to surmise that the orbicularis oculi preserved its power, probably because the nerve had been only partially injured. But this opinion could not be maintained, when the loose flaccid condition of the eye-lid, as it descended to cover the eye, was observed; for it was obvious, as it has been noticed in the case, that although the lid moved, the fibres of the orbicularis did not contract. A question therefore arises, what can cause the eye-lid to close, when the muscle on which its motion directly depends is paralyzed? How does it happen that the levator palpebræ, which is the antagonist of the orbicularis, does not lift the eye-lid and keep it so elevated? Looking at the patient's countenance, we had a striking enough exemplification, in the distortion of the features, of the tendency that is common to all muscles that retain their power to drag those that are paralyzed towards them. In the case of ptosis, resulting from paralysis of the levator palpebræ, we know that the orbicularis, when it preserves its power, keeps the eye-lid incessantly closed. Is it not strange, when the circumstances are reversed,

the orbicularis being paralysed and the levator palpebræ retaining its power, that the eye-lid should still be depressed?

It may assist in solving this question, if we recollect that, during the time referred to, the patient was comatose; and accordingly there was no reason to expect that the levator palpebræ would be brought into active use. But independently of this, we have to remark that this muscle has an action of a peculiar kind, corresponding with its office, and distinguishing it from other muscles. During the third part of the twenty-four hours, while we are asleep, the levator palpebræ is constantly relaxed; and while we are awake, it relaxes, in the act of winking, about once every minute. Hence it is difficult to say whether, if left to itself—that is, if it be unopposed by other muscles—its natural condition will be to remain in a state of contraction or relaxation. In the present case, owing to the paralysis of the orbicularis and the insensibility of the patient, the levator palpebræ was in a continued state of relaxation, and the eye-lid was perfectly passive, as if both its muscles were paralyzed. We can, therefore, understand why the eye-lid fell.

But this view will not throw light upon what has been further stated in the case—that the patient, when he was so far recovered as to understand our wishes, could close the eye after the eye-lid had been previously elevated. Such an effect could not be produced in a mere passive condition of the parts. To explain how it took place, we must turn our attention from the eye-lid to the eye-ball, and observe that the action of winking does not consist in a mere dropping of the eye-lid, but that at the same time that the eye-lid descends, the globe of the eye ascends, and even mounts to a very considerable height—to a greater height than the eye ever rises to when employed in vision. The motion to which I refer was first pointed out by Sir Charles Bell, and proved, by a course of ingenious observations and experiments on living animals, to result from the alternate action of the inferior and superior oblique muscles, and to be independent of the action of the recti. In the patient Kearsley, this revolving motion of the ball of the eye remained unimpaired, because the nerves that regulate it proceed from within the orbit; so that when he winked, although the eye-lid did not perform its share of the

action, the globe of the eye executed its part, and ascended towards the lachrymal gland. Now it is not difficult to understand how the motion of the eye here described should have the effect of communicating a motion to the eye-lid that might be mistaken for the muscular action of the orbicularis. We have, in the first place, to recollect that the cornea forms a considerable prominence in the front of the eye, and that the margin of the lid even rests upon its convex surface. As the eye-lid, in consequence of the paralysis of one of its muscles and the relaxed condition of the other, in the present instance was quite passive, it is obvious that, upon the eye-ball being elevated, it would be carried up along with it. But the eye-ball could not elevate the lid to any considerable extent, without causing it to receive a sudden check when it had reached its highest point of ascent: by this means the elasticity of the ciliary cartilage, contained within the eye-lid, would be brought into operation; it would recoil, and the eye-lid would again slip over the eye. An additional circumstance is to be remarked: when the prominent cornea passed beneath the margin of the stiff eye-lid, it would meet with some obstruction, and have to push the cartilage forwards to some extent. This would necessarily cause a certain degree of tension in the eye-lid; its elasticity would be brought again into play, and, when the tension was relieved, the eye-lid would glide downwards by its own resiliency, and with an impulse resembling the action of the orbicularis muscle.

Before leaving this subject, I may revert to the revolving motion of the eye-ball here referred to. It is not for a moment to be supposed that this action of the eye has any particular connexion with affections of the *portio dura*: the only circumstance that may associate it with such cases, is, that as disease of this nerve causes the eye-lids to remain apart, the rotation of the eye is more easily observed in them than on common occasions. The rolling motion of the eye-ball takes place every time we wink, or when we close the eyes in dropping asleep. It is owing to it, principally, that we experience that aching sense peculiar to drowsiness, when an almost irresistible disposition to turn the eyes upwards, as well as to close them, seizes upon us. But the

motion may be observed likewise in a person fainting; as I have witnessed it, with a more appalling expression, in a patient dying. There are, nevertheless, some authors who profess to believe that no such motion of the eye-ball as that on which I have been dwelling ever takes place. Had these gentlemen seen the eye, in this case, travelling upwards to such a degree that the cornea was completely concealed behind the raised eye-lid, and the white part alone exposed, no doubt they would have attributed it to the injury received by the brain—called it a symptom of effusion—applied the term *strabismus* to it, or perhaps described it as a singular spasmodic disorder of the superior rectus muscle, heretofore unobserved by pathologists.

I beg now to draw attention to the observation, that after the patient had enjoyed the power of closing the eye in the manner that I have endeavoured to explain, he gradually lost it upon recovering from his state of coma, and the eye was in consequence permanently open. That this change did not take place from the power over the muscles of the face becoming diminished, was obvious, because at the very time that it occurred a manifest improvement was observed in their general condition. Two causes, it appears, tended to produce this new feature in the case. First, in consequence of the *levator palpebrae* being unopposed in its action, and the patient resuming the exercise of the eye in vision, it is natural to conceive that this muscle would acquire a power of contracting to a disproportionate degree, from which cause the margin of the eye-lid would be elevated to a higher level than formerly; and consequently it would be removed from the influence of the revolving motion of the globe of the eye, to which I have attached importance as causing its descent. But another circumstance may have contributed to produce the same effect. In the natural condition, the eye is held within the socket, balanced as it were, between two opposing forces; at its back part it has a bed of fat and cellular substance, which tends by its elasticity to thrust the eye forwards, while on the other hand, the eye is supported and confined within the orbit by the *orbicularis oculi* acting on its fore part. It follows that when the last-mentioned muscle is deprived of its power, the eye-ball will

advance to a certain degree, and become more prominent than the other. This is a condition of the eye which was not only noticed in the present case, but has been referred to as generally observed in affections of the portio dura. If it be allowed that this prominence might become gradually greater after the first accession of the paralysis, we may conclude that it formed an additional cause why the patient was unable, towards the end, to close his eyelids, although he had possessed that power at first.

Having thus had occasion to advert to the compression exercised by the orbicularis muscle upon the globe of the eye, I may be allowed to dwell a little longer upon it, as it may lead us to discover the design of a very peculiar structure in the interior of the eye, to which the action of this muscle seems subsidiary. It has to be remarked that the delicate textures of which this organ consists, are liable to be destroyed, independently of the effects of disease, not only by injuries proceeding from without, as blows, or the intrusion of foreign substances, but also by forces operating from within. This has been especially dwelt upon by Sir Charles Bell, in explaining why the orbicularis oculi should be under the control of the portio dura, a respiratory nerve. It will be seen that, although the action of this muscle has the chief share in defending the eye from both the sources of injury referred to, yet there is a beautiful provision in the eye itself for protecting its fine textures, that has hitherto been overlooked.

First let me attend to the action of the orbicularis. At all times the globe of the eye is compressed by this muscle to a certain degree; but at particular periods it is embraced and pressed into the socket much more powerfully than at others. If we watch these occasions, it will be obvious that the violent action of the orbicularis has a relation both to the respiration generally, and to the circulation of the blood within the head. When we cough, sneeze, blow, or strain in any way, we are sensible that, instinctively, or, as it were, spasmodically, we close the eye-lids, and brace the eye firmly in the socket. Now what is the condition of the circulation at these periods? It is familiarly known that the blood contained in the veins flows to the heart with different degrees of freedom, depending on the state of

expansion of the thorax; if the breathing be natural, the blood has an equable motion; if a deep inspiration be made, it is admitted in greater quantity than usual; but if, on the contrary, a full expiration be made, it is suddenly checked. Thus the variety in the capacity of the chest influences the admission of the blood to the heart, as it does the admission of the atmospheric air to the lungs. But as the veins of the neck and head are unprovided with valves, the interruption to the free course of the blood in the acts of respiration will be attended with more remarkable effects in them than in the veins belonging to other parts; the large trunks, for example, will be first over distended, and then a regurgitation will take place, reaching even to their minute branches. In a patient who had attempted to cut his throat, and only wounded the external jugular vein in the middle of the neck, I observed that as he coughed the stream of blood spirted upwards as high as the temple, as if the wounded vessel had been an artery going to the head, in place of a vein descending from it. This proves, what will be the condition of the eye, when we cough or sneeze, or perform any other violent act of expiration. The blood will be thrown back into the minute veins ramifying on the tender tunics, by which its soft humours are sustained; and, if not prevented, there will be the same turgescence in these delicate parts that we see exhibited in the red and suffused skin of the face and neck of an asthmatic. How admirably, then, is it provided, that the orbicularis oculi should act powerfully at this very moment, and press the eye-ball firmly into the socket, thus preventing the back current of blood from gaining an entrance into the interior of the eye. The necessity of this compression will be more strongly evinced when we consider what takes place in the brain; for it is to be remembered, that the veins of the orbit belong to those of the encephalon, inasmuch as the ophthalmic vein, after leaving the orbit, joins the sinuses of the brain; and it will accordingly be subject to the same impulses that are occasionally propagated through the veins of this organ. When a circular piece of the skull has been removed in trephining, or, in other words, when a portion of the brain has been deprived

of its uniform support, we have an opportunity of observing the nature and extent of the force which sometimes operates through the great venous trunks during the irregularities of the respiration. Every time that the patient breathes heavily, or struggles, or coughs, the dura mater will be seen swelling up to the level of the outer surface of the trephined hole, or, if the dura mater has been lacerated, a quantity of cerebral substance will be protruded on each of these occasions. As these phenomena are only to be accounted for by the blood being retarded in its flow, and consequently accumulating in the veins, and thus squeezing out the brain where it was unsupported by the skull, so may we suppose that if the eye were not duly compressed, its vascular membranes would be disorganized, from the over distension of its vessels.

But I have referred to another provision in the interior of the eye for assisting in the same object. It appears to me that the remarkable distribution of the veins in the choroid coat, which has obtained for them the term *vasa vorticosa*, is designed as an additional means of protecting the delicate textures composing this organ from the same dangers to which I have been adverting—that the force of the returning blood will be diffused and broken, by this peculiar arrangement of the veins, before it can reach the soft and easily-injured parts in which their minute extremities ramify. This appears to be the reason why the veins of the eye do not proceed directly backwards, in straight lines, but before uniting to compose a single trunk, describe those circular concentric sweeps so much admired in an injected preparation of the eye. This arrangement must cause the blood conveyed from the different vascular parts in the interior to flow, in the first place, in a contrary direction to the current in the larger vessels, so that it will oppose more effectually the impetus of that which is regurgitating. But the oblique manner in which these vorticeous vessels enter into the larger veins, towards which they converge, will also serve to check the course of the back current. The mechanism is, in effect, nearly the same that we observe in the veins of the brain, as they fall into the longitudinal sinus; and which, we can have no doubt, is provided to prevent these delicate vessels,

which are sustained in the interior only by the soft pulpy substance of the brain, from being gorged with the blood thrown back into the skull during forcible expiration*. In observing the course of these veins, as they ramify on the surface of the hemisphere of the brain, it is seen that they commence at the back part, and then advance forwards in an arched direction, till they fall with oblique openings into the longitudinal sinus. Now, if we exercise a little fancy, and exaggerate in our minds the curves described by the veins of the brain, we may consider the arrangement which they present as not differing greatly from that which exists in reality in the *vasa vorticosa* of the eye. In conclusion, I have to state that the opinion here expressed as to the uses of the *vasa vorticosa* is confirmed, if we observe the difference in the distribution of the veins in the eyes of fishes, as contrasted with the mammalia. The appearance that has given rise to the name is common to the mammalia; and these are all exposed, more or less, to interruptions of the breathing, as in contending for their prey, or uttering their wild cries, &c. They are consequently subject, also, to the irregularities in the circulation of the blood within the eye, such as we have been considering. But the fish, being a voiceless animal, living in a different element, never exhibits, by the expression of its features, or the movements of its body, any indications of its respiration or circulation being disturbed in a similar manner. Its apparatus for breathing and its circulating system, are, in fact, completely different from what we find in the mammalia. Accordingly, as the fish has no eyelids with which to close the eyes in a troubled state of his breathing, so I have not been able to find any appearance in the interior of the eyes corresponding with the *vasa vorticosa*; on the contrary, the veins which ramify on the choroid coat, in place of taking a vorticeous direction, run in straight lines like those of other organs.

* In a case of fracture of the skull, where the longitudinal sinus was wounded, the blood flowed in such torrents each time that the patient struggled or coughed, that I had much difficulty in restraining the bleeding, although it ceased almost entirely when he remained quiet.

REMARKS ON CHIRAYITINE,
QUINA, AND CINCHONAÆ.

To the Editor of the Medical Gazette.

SIR,

IT is the object of the following remarks to expose an imposition on the medical profession and the public: they refer to the professed discovery of a new vegetable alkaloid, called chirayitine, in the Indian plant *Gentiana chirayita*, Roxb. (*Causarra chirayita* Lam. and R. Br.) The pretended new substance has already been prescribed by several physicians, in the form of a sulphate, and the attention of the public in general has been attracted to it by repeated advertisements to the following effect:—"Chirayitine, a new vegetable alkaloid, discovered and procured from the Indian herb *Chirayita*, or *Chirattah*, by G. M. Mowbray. The inventor (!) begs leave to announce to the medical profession, and to whomsoever the above discovery may interest, that after a course of experiments, prosecuted during leisure hours for several years past, he has at length succeeded in discovering the aforesaid alkaloid. He has therefore prepared a quantity of the sulphate in crystals, for their use. Its properties and dose having been experimented on by several of the profession, may be known on application at his establishment."

Of this article I procured a portion from the discoverer. It was labelled as follows:—

"A new Vegetable Salt, perfectly soluble, having for its base the active principle of the *Chirayita* herb. Prepared by the discoverer, G. M. Mowbray. Dose $\frac{1}{8}$ to $\frac{1}{2}$ a grain.

"Brighton, A.D. 1837."

An examination of the substance, to which I proceeded, proved it, as well from its form of crystallization as from the results of the application of various tests (which I shall presently specify), to be the neutral sulphate of quina (commonly called bisulphate of quinine.)

The substance appears in colourless, transparent, rectangular, four-sided prisms; its crystals effloresce, and are soluble in from 11 to 12 parts of cold water; its solution has a purely bitter taste, and reddens litmus paper.

The crystals, when heated upon pla-

tina, melt like wax, and, by increased heat, change from a red to a black fluid, exhaling an empyreumatic odour, and leaving a combustible coal behind—a proof of the presence of an organic substance.

A heavy caseous white precipitate was produced when a recent infusion of gall-nuts was added to a solution of the substance under examination, thus indicating the presence of an alkaloid.

When chlorine and ammonia were added in sufficient quantity to a solution of one grain of the alkaloid in 1000 grains of pure water, with the addition of a few drops of diluted sulphuric acid, the solution assumed a beautiful green colour, peculiar to quinine, indicating the presence of the latter.

Caustic potash produced a white flocculent precipitate, which did not crystallize, and restored the blue colour to reddened litmus paper.

Phosphate of soda added to a concentrated solution of the alkaloid, turned the fluid into gelatine.

Sulphate of barytes gave a white precipitate, not soluble in muriatic acid, proving the presence of muriatic acid.

The application of these tests implies the nature of the substance to be a sulphate of quina, and not a peculiar alkaloid of the *Chirayita* herb, as asserted by the discoverer. These experiments have been carefully repeated, and compared with others referring to sulphate of quina, so as to remove every doubt upon the subject.

Does the herb *Chirayita*, then, contain quina?—a most interesting question, since that alkaloid has only been found, hitherto, in the *Cinchona*.

According to Mr. Mowbray's verbal information, twenty-eight pounds of the herba *chirayitæ* yield three ounces of the sulphate salt; therefore an ounce of the herb will contain about three grains, which quantity is sufficient to ascertain by test the nature of the substance. An ounce of the herb was procured from the discoverer, and submitted to the following process, in order to extract the alkaloid in question. The herb was well bruised, and boiled with eight ounces of spirit of wine, and twenty drops of diluted sulphuric acid; the spirit was pressed from the herb, and filtered, and evaporated in a water-bath, the residue dissolved in water, and diluted ammonia added, until litmus paper remained but slightly reddened.

An ounce of a pale-coloured fluid was obtained, which evinced no trace of an alkaloid by the application of a recent infusion of gall-nuts, a test which will indicate with certainty 1-25,000th grain of sulphate of quina. It is superfluous to add that other tests of quina give a negative indication. A friend, who first called my attention to the above substance, and who is known as a skilful manufacturer of alkaloids, experimented on many pounds of the herb to produce the new alkaloid, and I can affirm that he also could not find a trace of an alkaloid.

The foregoing experiments prove that the pretended new alkaloid is nothing more than the commonly called bi-sulphate of quinine, and that the herba chirayite does not contain a trace of that alkaloid. Thus much for the public good, and for the sake of truth.

It may not be here out of place to notice a few tests for quina, with reference rather to their delicacy and peculiarity than to their novelty. The sensible action of tannin upon alkaloids has been observed by many chemists, and particularly since J. Pelouze published his interesting treatise upon tannin (*Ann. de Chem. et Pharm.*, l. iv. p. 337-365). O. Henry has applied pure tannin as an alkalometer. From my own experiments, I have found that a recently prepared infusion of gall-nuts will indicate with precision the presence of 1-25,000th part of a grain of sulphate of quina, and will produce, after a few hours, a flocculent sediment. Even the 1-50,000th part of a grain of the alkaloid will be indicated, after being in contact for a few minutes with the infusion of gall-nuts, by a turbid appearance of the fluid; but this I may almost call the limit of dilution for the test. It is to be remarked that by such dilution of the alkaloid, the fluid must be kept neutral. To be able to place reliance on the delicacy of this test, it is quite necessary to make use of a recent infusion; for if the infusion is but a few days old, and has been exposed to the atmospheric air, it displays no re-action whatever upon the alkaloid, in consequence of the change of the tannin into ellagic and gallic acids, both of which will be deposited. But though the fluid still contains a portion of unchanged tannin, yet from the presence of gallic acid, a portion of which is held in solution, the formation of a tannate

will be counteracted, or the formed tannate of the alkaloid will be dissolved by it. The proof of the foregoing is, that quina will not be precipitated by gallic acid, and the fresh precipitated tannate of quina will be *readily* dissolved by the addition of gallic acid. Morphia is particularly sensitive in this respect, as a small proportion of gallic acid accompanying the tannin, will be sufficient to prevent the precipitation of the tannate of morphia; the consequence of which has been, that even eminent chemists have asserted that pure morphia could not be precipitated by an infusion of gall-nuts, and have established this apparent fact as a distinction between the above alkaloid and the narcotine which was readily precipitated thereby. When sulphate of quina is dissolved in water, and a few drops of diluted sulphuric acid are added, it is well known that the fluid becomes slightly blue; and I have found that this phenomenon can be clearly observed even when the fluid contains but 1-25000th part of a grain of sulphate of quina. This phenomenon may (conjoined with other tests) be received as evidence of the presence of quina. A remarkable and very peculiar test of quina, is that which was first remarked by Roper (*LONDON MEDICAL GAZETTE*, Dec. 1832, p. 320-321), and afterwards noticed by André (*Journ. de Pharm. Mars 1836*, p. 127, 137). It is the action of chlorine and ammonia upon a solution of quina, producing an emerald green colour peculiar (amongst the alkaloids) to quina alone. As this is one of the most decided tests for quina, which will ascertain 1-5000th part of a grain of sulphate of quina, I will here notice the caution which is necessary in its application, that it may be safe even in the hands of the inexperienced. To an aqueous solution of quina are to be added a few drops of diluted sulphuric acid, until that bluish hue which I before mentioned is produced. Chlorine gas is now to be slowly introduced (from a suitable apparatus containing chloride of lime, to which has been added a sufficient quantity of hydrochloric acid), until the blue circle which appears on the surface of the liquid (and which is plainly perceptible if the surface of the liquid be glanced at in a straight line) is destroyed. Liquid ammonia is yet to be added by drops, until that beautiful green colour is produced.

If this manipulation be carefully attended to, 1-5000th part of a grain of the alkaloid will be detected. If the solution of quina be concentrated, a greyish-green precipitate will be produced. A superfluity of chlorine will alter the quina, and, by the addition of ammonia, a brown colour instead of a green will be produced; which is particularly the case with a minute portion of the alkaloid. If the solution of quina contains colouring or extractive matter, the peculiar re-action cannot be produced unless it be freed from those substances; which cannot be done by chlorine, without partially or wholly altering the quina. To free it from those substances in some other way, according to their respective character, is therefore indispensable.

To ascertain an admixture of cinchonia in the quina salt, I found the following procedure satisfactory:—Two grains of quina are to be shaken in a phial, with one drachm of pure ether and one drachm of liquid ammonia of the usual strength; if the quina contain but 0.1 of a grain of cinchonia, the latter will be seen separated between the surfaces of the ether and the liquid ammonia, while the quina will be kept in solution by the ether. The separation of the cinchonia is caused by its difficult solubility in pure ether.

G. SCHWEITZER.

Brighton, Oct. 1837.

ON INJURIES OF THE EYE BY PERCUSSION CAPS.

By SAMUEL CROMPTON, Esq.

THE following observations were made in the practice of Mr. Barton, surgeon to the Manchester Eye Institution, to whom I am indebted for permission to publish the illustrative cases, and his method of treating them.

One of the fragments, into which a percussion cap breaks when it is exploded, sometimes enters the eye. The accident generally occurs in shooting with, or in discharging, percussion caps with a hammer. I have seen many instances of it, and have preserved notes of seven cases, in each of which the injured eye was destroyed. In one of these cases the vision of the other eye, also,

was nearly lost, from sympathetic inflammation; and it is most likely that there would have been a similar termination of the rest, if that treatment, which I shall presently describe, had not been adopted. The peculiarities and importance of these injuries, and the consideration that they are unnoticed by systematic writers on the eye, have induced me to detail, more minutely than would have otherwise seemed necessary, the most remarkable circumstances relating to them.

In every case the fragment of cap was driven into the posterior chamber of the eye; but immediately after the accident, the changes produced in the eye, and the symptoms, were so like those observable in penetrating wounds of that organ, when no foreign body remains in it, that it was impossible to ascertain, at first, whether the cap was in the eye or not. The wound made by the entrance of the fragment of cap into the eyeball was generally a clean incised one, and healed readily. The vision was not destroyed immediately in those cases in which the cap went through the sclerotica, and did not injure the transparent parts of the eye. For a length of time, varying from a few days to a month after the accident, the patients appeared to be in a fair way for immediate recovery; but at the expiration of that time they were suddenly seized with most acute pain in the eye, attended with extensive chemosis, and with haziness of the cornea in some of the cases. Suppuration never happened. The pain subsided entirely for a while, or was greatly mitigated in a day or two after its commencement; but this cessation was only temporary, for it always recurred and subsided at uncertain periods, until the vision with the injured eye was entirely destroyed, the eyeball in a state of painful chronic inflammation, and the health of the patients much injured by the irritation occasioned by the injury, and from anxiety for their sight; for the vision of the other eye became affected at this stage of the disease, by the inflammation extending to it by sympathy. The first indications of its commencement there were a slight redness of the conjunctiva, and an inability to see so well as formerly with the eye, or to bear the ordinary light of a room without pain and confusion of vision.

In Case I. the effects of the symp-

thetic inflammation were, a dull yellow colour of the sclerotica; a change in the colour of the iris, and adhesion of it to the capsule of the lens; and a very irregular and small pupil, filled with a dot of opaque capsule.

The fragments of caps taken from the eye, after being within it for months, were only tarnished; they bore no appearances of undergoing changes similar to those which take place in pieces of steel during their exposure to the humours of the eye; they were always of a considerable size, and their angles were very sharp. Mr. Barton believes that the sympathetic inflammation in these cases is occasioned by the presence of a fragment of cap in the other eye, and that the only means of preventing it, or of allaying it when it has arisen, is the removal of that fragment from the eye. He has treated many cases on this principle; of seven of which I have preserved the following notes.

CASE I.—W. —, Esq., about forty years of age, and of very intemperate habits, was shooting on the moors in August 1832, when, on discharging his gun, something cut his right eye. He lived far from Manchester, and was under the care of his usual medical attendant, who used active measures to subdue the pain and inflammation which occurred in a few days after the accident. He consulted Mr. Barton, for the first time, on the 29th October, 1833, when he had continued pain in his right eye, occasionally so severe as to prevent him sleeping for successive nights; his vision with it was destroyed; the left eye also was inflamed sympathetically, and vision with it so much impaired that he could not find his way. His health had suffered greatly from the effects of the disease, and his anxiety for the recovery of his sight. Mr. Barton told him that it was very probable that something had entered the eye and occasioned his sufferings; but, in compliance with the request of the patient, who was unwilling to submit to an operation for its removal, various plans of medical treatment were tried until the 3d of November, without the least benefit being derived from them. On this day a large piece of the cornea of the left eye was cut away, in order to remove the foreign body; but the eye was so exquisitely sensitive,

that attempts were not made to find it. A large poultice was applied to the lids. In a few days afterwards a large fragment of a percussion-cap was removed from the coagulum, which filled up the opening that had been made in the globe of the eye; it was merely tarnished, and its angles and margins were as sharp as if it had been just broken. The patient was permanently relieved, but the sympathetic inflammation had produced so great changes in the other eye, that it was necessary to perform an operation for artificial pupil upon it.

CASE II.—George Ankers, of Staly-bridge, about 28 years of age, on the 29th January was sitting near a man who discharged a gun, when a portion of cap entered his right eye. On the following day he could see across a room with that eye, but in the course of a month vision with it was quite lost. On the 8th April the conjunctiva was very vascular, and the eye occasionally very painful. He could not bear the ordinary light of a room without placing his hand over the other eye (the left), neither could he read more than one, two, or three lines with it, before the letters became indistinct, and the eye painful. A flap of the cornea of the right eye was removed, and a poultice was applied to the eyelids. On the 16th of June, a friend removed the fragment of cap from the cicatrix in the front of the eye-ball. He has been easy since, and the powers of the other eye are quite restored.

CASE III.—Master R., of Stockport, about six years of age, was playing, on the 25th of July, 1836, with a boy who was exploding percussion caps with a hammer, when a fragment of one cut his eye. This eye was so free from pain and inflammation for several weeks, that it was hoped that the cap had not entered it; but by the 21st of September it had assumed the appearances indicative of its presence there. A portion of the front of the eye-ball was cut away. On the following morning the fragment of cap was found in the poultice which had been applied to the eye-lids; it had only become of a darker colour by being in the eye.

The following cases were treated as the above:—

CASE IV.—W. Williamson, of Stockport, injured in shooting.

CASE V.—Adam Chamley, of Hebden Bridge, Yorkshire, injured in shooting.

CASE VI.—Mr T., of Cumberland, injured in shooting.

CASE VII.—John Taylor, of Manchester, injured whilst standing near a man exploding a percussion cap with a hammer.

The details of the four last cases are so like those of the three first, that it seems unnecessary to give them. In all, however, the object of the operation was gained; the sympathetic inflammation being suspended in the first case, and the symptoms which were thought to indicate its approach being removed in the others. The operation is thus performed:—The patient being placed in a convenient position, the operator forms, by means of Beer's knife, a large flap of the cornea, which he seizes with the forceps and cuts away with a pair of curved scissors. A dose of laudanum is then administered to the patient, and a linseed-meal poultice applied to his eye-lids. The operation always gives great pain, and should be performed as rapidly as possible. The eye is so exceedingly sensitive, that attempts to find the fragment of cap cannot be endured. In all the cases of Mr. Barton, the cap was found in the poultice, or in the coagulum which closed the opening into the eye, in a day or two, or at a longer period after the operation.

I have made diligent inquiries as to whether the caps which inflicted the injury were grooved or smooth. I believe that they were generally smooth and of an inferior kind, called French caps; but it is very likely that both kinds are very dangerous when exploded between two flat surfaces on a level with the eye; an amusement with children which is very common in this part of the kingdom.

Manchester, Oct. 21, 1837.

POISONING WITH CREAM OF TARTAR.

To the Editor of the Medical Gazette.

SIR,

SHOULD you think the following account of a case of poisoning, from taking a large quantity of cream of tartar, worthy to be recorded, I would beg the

517.—XXI.

favour of its insertion in an early number of the *MEDICAL GAZETTE*.

I remain, sir,

Your obedient servant,

WM. T. TYSON,

M.R.C.S. L.

57, Ossulston-street, Somers Town,
October 22, 1837.

John Hudson, æt. 37, residing at 24, Southampton-street, Euston Square, employed by the notorious and self-styled *Dr. Morison*, at his pill establishment in the New Road, King's Cross.

On Tuesday evening, Oct. 10, he returned home extremely weak, and scarcely able to walk. He stated that he had been severely purged, had at least had twenty-five motions, attended with constant vomiting. On the previous day (Monday) he was drunk. The nurse told me, that one of Morison's men said he was sure Hudson had taken nearly a quarter of a pound of cream of tartar at one time, and that during the day he was continually putting small lumps into his mouth, to cool his stomach. The nurse also noticed a quantity of white substance at the bottom of the chamber utensil; she then asked him if he had been taking any salt. It was unfortunately thrown away.

11th.—At twelve o'clock he was seen by a medical gentleman, who found that during the night he had been repeatedly purged, and suffered greatly from constant vomiting. He complained of pain in the umbilical region, and of great thirst. Tongue brown and dry; pulse feeble; pain in the loins. The thighs and legs appeared paralysed. The fluid vomited was of a blackish green colour, and the motions of the colour of coffee-grounds. He stated that he had taken four or five table spoonfuls of cream of tartar, which is a principal ingredient in Morison's cooling powders. An opiate was given to him, which afforded slight relief; but the symptoms returned, and he died on Thursday at noon.

14th.—I was requested to assist in making a post-mortem examination of the body. He was a strongly-built, well-formed man, stout and muscular; no spots or bruises upon the body. The fat covering the abdominal muscles about half an inch thick.

Stomach.—This was distended with gas, and contained about three ounces

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of a thick brown fluid, coloured apparently with bile. Near the pylorus there were several red patches. The cardiac end of the stomach was very much inflamed, the mucous membrane being of a deep red colour, with three or four spots of a blackish red, as if from rupture of some of the minute blood-vessels. The mucous membrane of the duodenum was also of a red colour in many places, but not in so great a degree as that of the cardiac end of the stomach; it contained, apparently, the same kind of fluid as the stomach. A portion of the small intestines, as also the colon, had the mucous membrane reddened. The mucous membrane of the rectum was injected in small streaky red patches; where there was no redness, the membrane was of a white colour. The intestines contained a thick brownish kind of mucus; no fecal matter was observable throughout their whole extent. A great deal of fat was attached to the large intestines, and the omentum contained a large quantity.

Chest.—The lungs might be termed healthy, though there were adhesions at the posterior part of the lobes of the left side, and at the anterior part of the lobes of the right, evidently of an old formation.

Heart.—A great quantity of fat about the pericardium. The heart was unnaturally large and flabby; the lining membrane of the left auricle was of a deep red colour, as also the lining membrane of the aorta and semilunar valves. It weighed 13 oz.

Liver.—Large and very firm. On cutting it an oily appearance was left on the knife, and oil could be scraped from the cut surface. It weighed 4 lb. 11 oz.

Spleen healthy. Kidneys large, and embedded in fat.

LARGE EXOSTOSIS OF THE FOOT.

To the Editor of the Medical Gazette.

SIR,

By inserting the following case of exostosis of the foot, you would much oblige,

Your very obedient servant,

THOMAS FALCON.

Bradford, York,
Oct. 23, 1837.

Samuel Bradley, by trade a weaver, 32 years of age, of a leuco-phlegmatic temperament, says that the above exostosis first made its appearance about fifteen years ago, and has gradually increased to its present enormous size.

The tumor completely envelops the whole of the foot, both on its dorsal and plantar surface, its circumference being about eighteen inches. That portion of the tumor situate at the internal border of the foot is slightly moveable on its base, and apparently cartilaginous or gelatinous in texture; but in the middle and outer part of the foot it is immovable and osseous. There are two ulcerations on its dorsal aspect, from which spiculae of bone have been extracted. The patient has never experienced pain, nor had any constitutional symptom indicative of the tumor being of a malignant nature.

He had previously consulted three surgeons, who declined to remove the tumor, and said that amputation must be had recourse to. The patient objected to lose his leg, but was willing to submit to the removal of the tumor. From his history of the case I was led to infer that the growth of the tumor first commenced either from the cuboid or external cuneiform bones, or from the heads of the three outer metatarsal bones.

I gave it as my opinion, that the tumor was attached by bone (and inseparable) only at the outer part of the foot, and that there would be a portion of the foot remaining, after the operation, sufficient for the support of the body and progressive motion. Having obtained the man's consent, I first removed the tumor in the manner hereafter described, and then a portion of the foot, by the method described by Mr. Wharton before the Medical Association at Dublin.

Operation.—The first incision commenced at the intermediate space between the second and third toes, and a little above their junction with the metatarsal bones (viz. from the point marked *a*, in the drawing, in a straight and longitudinal direction to the point *b*.) From these last points two incisions were made at right angles, viz. from *b* to *c*, and from *a* to *d*, and the integument reflected inwards, so as to be enabled to obtain a covering for the two

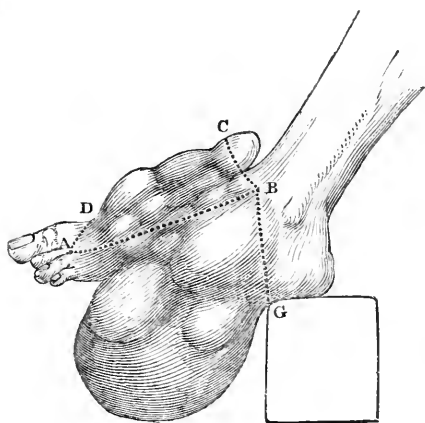


FIG. 1.



FIG. 2.

first metatarsal bones (on their dorsal aspect.) Another incision was then carried from *b* to the anterior and inferior border of the os calcis, viz. at *g*. The portion of the tumor included in the quadrilateral space marked *a b c d*, being cartilaginous, was separated from the tendons of the tibialis anticus, extensor proprius pollicis, and that portion of the tendon of the extensor longus digitorum going to the second toe, and removed by a strong knife. Another incision was then made, commencing at *a*, and extending to *g*, on the under surface of the projecting part of the tumor. The portion of the tumor situate in the triangular space *a b g*, was then separated from the three outer metatarsal bones, the cuboid, and the external cuneiform bone, by Hey's saw. The part of the tumor extending into the sole (which was cartilaginous) was then carefully dissected outwards, and separated from the abductor pollicis pedis, flexor brevis digitorum pedis, and the internal plantar artery and nerves. A catlin was then passed between the second and third metatarsal bones, and thence forwards between the external and middle cuneiform bones. Laying hold of the three outer metatarsal bones with the left hand, and placing his foot on my knee, I used it as a fulcrum to dislocate the cuboid and external cuneiform bones from their articulatory surfaces with the os calcis and scaphoid. Then, by dividing the calcareo-cuboid, ligamentum longum plantæ, and the

deep-seated calcareo-cuboid ligament, completed the operation.

A considerable portion of the flaps united by the first intention; the remainder granulated, and cicatrized favourably.

Fig. 2 represents the foot after the operation, and the numerals 1, 2, 3, the line of the cicatrix.

At the present time (three months since the performance of the operation) he is able to walk upon the limb. The tumor, after its removal, weighed five pounds.

MEDICAL GAZETTE.

Saturday, October 28, 1837.

"Licet omnibus, licet etiam mihi, dignitatem
Artis Medicæ tueri; potestas modo veniendi in
publicum -it, dicendi periculum non recuso."
CICERO.

ON THE SUPPLY OF SUBJECTS FOR DISSECTION.

THE medical session has scarcely commenced, and even now there is a complaint of the inadequacy of the supply of subjects for prosecuting anatomical studies—already the inexpediency of the system at present adopted under the Anatomy Act has become evident. This is only in accordance with the experience of past

years, which has clearly shown that the only advantage (as far as the student is concerned) of the plan now pursued for obtaining bodies, is their greater cheapness; an advantage which is, of course, more than outweighed by their scarcity. Under the old system there was never any difficulty in obtaining as many subjects as the pupils would pay a large price for; whereas now that no amount of expense or trouble is of any avail, the supply falls miserably short.

But we do not believe that this deficiency is the necessary result of the Anatomy Bill *per se*. Had it been left to be worked out in strict accordance with its letter, it would have continued to afford still, as it did at first, a supply fully sufficient; but interfered with as it has been, by the introduction of fresh powers and of artificial expedients of distribution, it has been made useless, if not mischievous. The original cause of the failure of the last four years is probably in the recollection of many of our readers. In 1833, previously to which there had been a plentiful supply under the act, complaints were raised by some of the teachers that there was an unequal distribution among the different schools, and that this would necessarily, in the end, be injurious to those least favoured by fortune or by influence. The fact was, that many of the teachers had been using all the interest they possessed in different parishes to persuade the authorities to assign the unclaimed dead for dissection, and had in many instances succeeded in inducing them to grant that they should be so, provided they were sent to, and dissected at, certain schools in whose managers they (the authorities) could confide. By these exertions many of the schools obtained a sufficient supply; but no sooner did the others who had less influence, and received only their share from the general stock consigned to the Inspec-

tor, find themselves at all deficient in their supply, than they cried out for an equal distribution of all the bodies, to whomsoever consigned, according to the number of pupils in each school requiring to dissect. The immediate and very natural effect of this was, that in some of the parishes the authorities refused to give up the bodies, and preferred to have them buried at the parochial expense, rather than risk their not being buried at all. Others became indifferent, when they had not the power of obliging their own friends; and the exertions of the teachers, which were well repaid when all the subjects they obtained were sent to their own or only a few schools, were discontinued now that the same number of subjects were divided among twenty different establishments.

Ever since, the supply has been smaller in all the schools than it was in any of them previously to this interference. In 1836, the occurrence of the influenza, and the consequent number of deaths in the large hospitals, gave the schools attached to them a temporary surfeit; and accidental causes have at other times, for a short period, favoured them; but, with these exceptions, the deficiency has been universal, and the prospects for the ensuing season are worse than ever.

The chief cause of all the difficulties is evidently this imagined necessity for equal distribution, which, even if it existed, would be no compensation for the loss it occasions; for surely it were better that some should be served well, and others moderately, than that all should fare intolerably ill. In the first place, how is this equal distribution effected? Why, from each school there is to be sent to the Inspector a written account of the number of pupils intending to dissect there, and according to the proportion of the members thus ascer-

tained, the bodies are distributed. If, for example, from one school a return of twenty, from another of thirty, from a third of forty pupils were obtained, they would severally receive in return, in the proportions of two, three, and four, from the whole stock of subjects obtained. The objections to this method are palpable enough. From some schools, lists, all but fictitious, are sent in: pupils who have long since received their diplomata; pupils who do not intend to remain in London more than a few weeks; all that can be pressed into the service, subscribe their names to swell the list of those who it is pretended intend to dissect; and the greater the degree in which this dishonesty is practised, the greater is the reward. If, on the other hand, a teacher sends in a fair list, or if (as is often the case in large schools) he cannot easily obtain the signatures of all his pupils, and at all neglects to make out his fullest number, he suffers for it by a proportionate loss. Besides, this plan applies only to the pupils, not to the lecturers, who need as many subjects for a small as for a large class; and according to it, if the teacher of 120 obtained six subjects for his lectures and demonstrations (which are not more than is necessary,) the teacher of twenty would have but one for both courses for the whole season.

Nor are these the only objections that must always mar the present system of distribution. Of all schemes in political economy, none have more invariably met with signal failures than those of attempting to regulate supplies by imagined wants; and the present will only add another to the number experience has already seen fail, even when undertaken on far better data than are at present attainable. To say nothing of the somewhat inquisitorial plan of demanding from each teacher the number of his pupils, and

so of holding up to the public the degrees of success or failure of each school, this mode of supplying by number, without regard to condition or want, is utterly absurd. An inspector might as well attempt to determine by the number of customers which each butcher or baker had, at the beginning the year, how many oxen the one should be allowed to buy at Smithfield, or how much flour should be supplied to the other at Mark-lane, and restrict each to what he deemed sufficient, till all his brethren were served with what seemed a fair quantity and quality, according to their lists of customers. Our authorities may depend on it, neither teachers nor tradesmen will ever procure more of their commodities than they can fairly dispose of; and they are themselves both the best judges of what they want, and the best able to obtain it: besides, the perishable nature of the commodity here considered, is sufficient to prevent any attempt at monopoly. When Government appoint persons to do that for others which they can do sufficiently well for themselves, dissatisfaction is sure to arise, and failure must follow; and this is just the course which the system, employed under the Anatomy Act, is now taking.

At present, however, no exertions seem to be used by any one, and the parishes and other sources from which the principal supply should come, are left to give their unclaimed dead or not, as they like. The authorities in several places still dislike disposing of their fellow-creatures for such purposes; some fear popular clamour; others think the small pecuniary advantage of saving the burial fees insufficient to compensate for the trouble; and all are cold and indifferent to that in the advancement of which they have no immediate interest*.

* There is a man of the name of Roberts, who, disappointed at finding no patronage for

The Poor-Law Bill, too, thins the number of living in the workhouses, and so indirectly affects the dead. But all these difficulties might be overcome by private exertion, by any one who would act in one case on the common sense, in another on the interest, in another on the friendship of the parties concerned; here asking favours, there promising benefits in return, and always assuring the fulfilment of all the Act requires. These, and these only, are the means which, actively employed by private individuals, will secure a sufficient supply of subjects of dissection; and if they be not permitted, we can scarcely expect but that many of the abominations of the system that existed before the Act was passed, will return.

But what encouragement is there for those teachers who, previous to 1833, shewed, by the supply they obtained, how great influence they possessed, to continue their exertions, when, after all the trouble and expense they have incurred, they see that supply which, in the mass, would have been valuable for one, broken into twenty morsels, in a vain pretence of satisfying a multitude, among whom are all their own opponents? Even exertions made in private practice are now useless; for where the friends of a deceased person would give up the body to any teacher whom they were on terms of intimacy with, it is not probable they would let it be carried they know not whither, or to what destiny. In short, individual private exertion is totally disabled, and all have given up attempting to employ any; and to expect a co-operation of such conflicting interests as those in the various schools, is a chimera

what he absurdly thinks a valuable discovery for preventing putrefaction, endeavours to prevent the schools being supplied, by addressing to the guardians of the poor the most ridiculous misrepresentations "of the disgraceful manner in which the bodies are treated," and who placards the streets with warnings against Dr. James Somerville, and the anatomical teachers.

to demonstrate the fallacy of which the angry feelings excited in 1833 were not needed.

Had the present difficulties been foreseen when the bill was being framed, a clause might have been introduced to appoint some person, or means that should have constantly acted, to insure the greatest possible supply from all the sources open; for it was the professed object of the bill not only to prevent the repetition of the indecent outrages and crimes before perpetrated, but to afford all necessary legal assistance to the study of anatomy, for which previous statutes had been found insufficient. Had all the government and other influence been thus fully secured, it might have been the rational duty of an inspector to distribute the full supply that would then have been procured; but in the present case this power given to the inspector cripples private exertion, and brings with it nothing as a compensation. We know that many think it is the duty of the present inspector, not only to see all the requirements of the act fulfilled, and to effect a just division of the bodies, but also to procure a sufficient supply for such distribution. But in the Act no such power is given him; neither is he mentioned as the proper person to do this; nor is any mode of obtaining a supply pointed out. To add this to his other labours is quite absurd. If those who made themselves dependent on his bounty thought that they had placed themselves in the power of one who could pay them well, and had riches both in hand and in prospect, they were much mistaken. He is in no way obliged to procure a single body for dissection—nay, even his right to distribute them is rather questionable; and the only duties actually required of him by the Bill, are to see that all the matters relating to the receiving and burying of bodies are accomplished according as it ordains.

He complains, we understand (and we have no doubt with justice), that his situation is already most painful and most arduous, assailed as he is every day by importunate lecturers who have no *subject* to descant upon. This distributing part of his functions, too, is doubtless the more annoying, as in it he exercises a power which he did not originally possess, or at least exert, but which was almost forced upon him, in 1833, when, in the hope of assuaging the disputes, he undertook to manage this division of the bodies among the several schools. We can easily imagine that at that time he little imagined what an unpleasant task he had undertaken, and that he little knew how impossible it would be ever to please all those whom he had made the beadsmen of his bounty. Had he all the living as well as the dead to dispose of, he would scarcely satisfy them of his justice, though he might of his liberality; but in his poverty of resources, we can scarcely conceive any man more miserably pestered than he must be every day of his life. We would strongly recommend him to give up the little authority his power affords him, and which is dearly bought at the expense of being subjected to such perpetual and irremediable annoyances, and to let his office lapse back again into the comparative sinecure of receiving and arranging confidential returns and burial certificates.

If the clog of distribution were removed from them, we feel assured the teachers would each find for himself a sufficient supply; for, as we have said, exertions which never will be employed for the general good would soon be actively made for private advantage.

The only ill effect which has ever been feared from this return to the old plan (but that it would follow has never been proved) is, that the small school's

would suffer in consequence of the whole supply being absorbed by the larger ones; and that some of those possessing least influence must be altogether "swamped." It is not for us to fix the value of some of these institutions, nor to say how much the public or the profession would suffer, if some of the lowest on the list were altogether expunged, but assuredly, if in order to serve them the whole body of students are to be prevented from learning anatomy efficiently, the sooner such schools are closed the better. But we do not believe such would ever be the result; there cannot be a teacher without sufficient interest in some parish to obtain a share of the bodies, and there will always be a certain number consigned to the inspector, which it would be his especial business to distribute among the worst supplied schools. But further—can they be worse off than they are now? The dissecting rooms of some are empty and inodorous; while those of others are so odoriferous that they had better be emptied as soon as possible; demonstrations are being given without subjects; and all real anatomy is at a stand-still.

In short, the present system being irksome to both teachers and inspector—injurious to the large schools—ruinous to the small ones—and an insuperable obstacle to the study of anatomy in London—it will be for the advantage of all to give it up. *Let, then, the teachers memorialize the government*, stating their ability if left to themselves to relieve their own wants, and the impossibility of any but private exertions effecting the object: let this be done, and supported by the inspector—as we scarcely doubt they would be—they may almost rely on a speedy amelioration of their condition.

CLINICAL LECTURE

ON

VARICOSE VEINS AND ULCERS
OF THE LEGS;*Delivered at St. George's Hospital, Oct. 17, 1837,*

BY SIR B. C. BRODIE, BART.

Ulcers of the Leg depending on Varicose Veins—Importance of such Cases—Different Situations in which Varicose Veins occur—The Varicose state sometimes dependent on Pressure, but often independent of this, and probably arising from Weakness of the Coats of the Veins—Effect of Pressure and of the Valves—Progress of Varix—Symptoms which result—Inflammation of Varicose Veins—Peculiar appearance of Ulcers from this cause—Treatment of such Cases—Bandages—Plasters—Laced Stockings, &c.—Rest—Cold Lotions—Purgatives.

"MARY ANN RICHARDSON, 48 years of age, was admitted on the 11th of October, with ulcers on the legs. She has a group of three ulcers, each about the size of a shilling, situated in the middle of the left leg, on the outer side, with livid edges and flabby granulations, slightly elevated above the level of the skin, and discharging pus tinged with blood. There is another ulcer, of the same size and description, but with small indolent granulations, above the inner malleolus. These ulcers are reported to have commenced each with a small pimple, followed by a superficial abrasion of the surface, discharging a watery fluid, and afterwards extending into an ulcer. There are cicatrices from previous ulcers on the leg, and also the appearance of some of them in the first stage of their formation. She has suffered from these ulcers for nine months, and for some years from varicose veins, of which there are clusters on both sides, and smaller ramifications of them over the limb. The skin of the lower two-thirds of the leg has a dark stained appearance, with a defined margin."

This is a case in which there is no question about the patient's life or death, and I think it very probable that many among you may pass by the bed-side of such a patient without thinking it worthy of attention. But I am not disposed to regard it in this manner. Although the patient will not probably die of this disease, yet, without great care, it may render her miserable. The disease may be very much relieved by art, and it is one of very common occurrence. You examine carefully a case of aneurysm, a case of stone in the bladder, and so on: but these are

things comparatively of rare occurrence, and which, at any rate, will not fall under your treatment in the beginning of your professional lives. But here is a case of a very distressing nature, and such an one as may meet you at every turn of your practice; and your reputation in early life will depend more upon understanding a case of this kind, than upon your knowledge of one of more rare occurrence.

I have no doubt that the ulcers of the leg in this case depend upon the varicose veins. These are what we call *varicose ulcers*. In order that you may understand this particular case, I must make some general observations on varicose veins, and the ulcers to which they give rise. I shall first speak of the disease, then of the treatment required; and I shall leave you to apply the observations which I have made to the case now before you.

By a varicose vein, I mean a vein which is unnaturally dilated. When there is increased growth of any part, the arteries increase in size to take the blood to it, and the veins increase in size to take the blood from it. This is a healthy increase of the veins, and we do not call these veins varicose. But by a varicose vein I mean a vein unnaturally enlarged, without the dilatation being instituted to answer any good purpose in the animal economy.

Varicose veins occur principally in three situations: in the legs: in the spermatic cord, where the disease is called *varicocele*, or *circoscele*; in the rectum, and about the anus, where the disease takes the name of *piles*, or *hemorrhoids*. I will explain to you, by-and-by, why they occur in some situations more than in others. But varicose veins occasionally occur in other parts of the body. I have seen varicose veins of the forearm to a considerable extent. In the case to which I allude, there had been inflammation of the median cephalic and cephalic vein. These veins had become obliterated, and, in consequence of their obliteration, the blood did not easily return from the forearm; so that the veins became varicose.

A man was admitted into the hospital who had varicose veins all down the right arm, and to a considerable extent down the right side of the chest. He had difficulty of breathing, and cough. One day he felt as if he had received a blow on one side of the chest, and immediately a large abscess presented itself, as big as an orange externally, which had evidently made its way from the inside of the chest through one of the intercostal spaces. Immediately upon the appearance of this swelling, the varicose veins in a great measure subsided. The man died, and on examining the body after death, it was

found that there was disease in the bronchial glands; suppuration had taken place in them, and a large abscess had been confined in the inside of the chest, which pressed on the right subclavian vein, and this caused the blood to stagnate in the veins in which it had its origin, and which had in consequence become varicose. So, under corresponding circumstances, you may find the veins become varicose in any part of the body.

In the first of the cases which I have mentioned, the varicose disease was the consequence of disease and obliteration of the venous trunks; and such is sometimes the cause of varicose veins in the legs. There was a man in the hospital with very bad varicose veins of the legs, one of the worst cases of the kind that I ever met with. The man, however, was admitted into the hospital on account of another disease, of which he died. I examined the body after death, and found an obliteration of the external iliac vein. This vein had been inflamed at some former period, and had become converted into a thick hard cord. The blood could not flow to the heart through this great venous trunk, and so the branches below became varicose. In the other case which I have mentioned, pressure on the venous trunk was the cause of the varicose disease. And so pressure on a venous trunk in the abdomen may produce varicose disease in the legs. You have a very frequent example of this in child-bearing women. The pressure of the gravid uterus will produce varicose veins of the leg. The woman is brought to bed, the pressure is taken off, and the varicose veins in a great measure disappear. Then she becomes pregnant again; the varicose veins recur; she brings forth another child, and the veins in a great measure subside, but not so completely as before. Every time she is pregnant the varicose disease of the veins becomes aggravated, till at last it exists to a great extent in both legs.

There are a few cases in which we may trace varicose veins of the lower extremity to pressure or obliteration of the venous trunks; but in the majority of cases it must be acknowledged that we cannot trace the disease to these sources. It appears, in these cases, to be a mere weakness in the coats of the veins, rendering them incapable of supporting the weight of the body. There is, of course, always a column of blood pressing downwards when the patient stands erect; and if the coats of the veins are weak, this is sufficient to render them varicose. You will understand, then, why, when the coats of the veins are weak, persons of particular habits, or of a particular physical construction, are more liable

to varicose veins of the legs than others. A person who is always upon his legs, always standing or walking, is much more liable to have varicose veins of the legs than one who leads a more sedentary life, because there is here a column of blood almost always pressing on the veins below.

Sir Everard Home has observed, that in the army the grenadier companies are especially subject to varicose veins, they being taller men than the other soldiers. Cooks are very subject to varicose veins. Why? If you put one hand into warm water, and the other into cold, you know that the veins of the former will become dilated, and that those of the latter will contract.

But where the disposition to the disease exists, do all the veins become dilated? By no means. The deep-seated veins never become varicose, because there is the pressure of the muscles upon them on every side, which prevents their dilatation. It is only the superficial veins that become varicose. The branches of the vena saphena major, and sometimes of the vena saphena posterior, become dilated. But the valves do not increase with the dilatation of the vein; they remain of their original size; and what must be the consequence? Why, the valves do not protect the venous branches below from the pressure of the column of blood above; they do not answer the purpose of valves any longer; and the want of action in the valves tends, of course, to aggravate the disease. By and by the valves seem to become changed in structure; they shrivel up, and become at last good for nothing, not even looking like valves. This is in conformity with a general law of the animal economy: a part not used wastes. If you were to tie up one eye, and cover it from the light for many years, you would find at last that you could not see with it. Muscles not used will waste. So it is with the testicles and other organs. When valves become useless, nature does not seem to think them worth keeping, and they waste or shrivel.

In a few instances varicose dilatation of the veins comes on rather suddenly; I have known cases in which the veins in both legs became varicose immediately after very hard walking. But, in general, the disease comes on slowly, and increases gradually. At first one or two veins are a little dilated, and you see the dark blood looking of a blue colour through the skin. Then other veins assume the same appearance, and by and by you find clusters of varicose veins in different parts of the leg. The skin is elevated by the clusters underneath; and it is when the skin is strained and rendered thin that you see the dark

colour of the blood through it. These clusters are more frequently situated about the inner ankle, and the inner side of the leg, than any where else; but they may occur any where else, at the back or outside of the limb. Then, as the disease proceeds, it extends to the trunk of the vena saphena major, and this becomes dilated all the way up to the groin. Sometimes the saphena major looks as large as your finger, assuming a knotted appearance. What is the explanation of this? It would seem that the vein is tortuous. Varicose veins are not only increased in diameter, but in length, and of course must then be made tortuous; and where the saphena vein is twisted, as it were, upon itself, it assumes the appearance which I have mentioned. The dilatation of the vein is perceptible when the patient stands erect; but when he lies down the varicose appearance vanishes, because then the veins become emptied of their blood.

While these changes take place in the condition of the veins, the patient experiences more or less inconvenience. Sometimes he suffers from a sense of itching and weight about the inner ankle. The sense of weight and fullness becomes more troublesome when he takes a long walk, so as to be very distressing. Where there is a varicose cluster, the patient in a few instances experiences extraordinary pain, and this, as I imagine, arises from there being some nervous filament pressed on by the tumor. Sometimes the patient complains of being subject to cramp in the muscles of the leg, especially after a long walk. Varicose clusters occasionally burst and bleed. I said, in the commencement of the lecture, that the disease is not dangerous, but that is not absolutely and universally correct. There are a few cases in which a patient may be in danger from hæmorrhage. A varicose cluster becomes larger and larger; the skin over it becomes more attenuated, at last it gives way, and there is a great discharge of blood. I have heard of patients actually dying from this hæmorrhage, where assistance could not be procured. I have known a great many cases in which patients have lost a very large quantity of blood from such an occurrence; and I have heard of others in which death was the consequence.

Varicose clusters of veins sometimes become inflamed. They are then tender to the touch. Frequently the inflammation is preceded by a rigor, or by an attack of fever. In some instances the inflammation extends to the skin over the cluster, the skin becomes red, and if the patient stands up, he suffers great pain in the inflamed varix, but if he lies down,

the pain is in some measure relieved, though not entirely. The great pain in the erect posture is explained by the weight of the column of blood pressing on the tender parts.

In some cases inflammation of an inflamed varicose cluster will end in suppuration, and in an ulcer, but that is not the way in which ulcers connected with varicose veins generally begin. For the most part, the effect of inflammation of a varicose cluster is not to produce either abscess or ulcer. It is very remarkable that the blood in inflamed varicose veins coagulates, and the vein becomes choked up with the coagulum. There seems to be something in an inflamed vein that is unfavourable to the fluidity of the blood which it contains. You observe this not only when varicose veins are inflamed, but when veins are inflamed under other circumstances. You find this frequently in cases of piles. A patient comes to you with an external pile, which is large, and very tender—it is inflamed. At first it contains fluid blood, but in a day or two it becomes filled with solid matter; and if you slit open such an inflamed pile, you find a solid lump of dark coloured fibrine. If you slit open an inflamed varicose cluster in the leg, under these circumstances, you will find that the cavity is filled up in like manner with coagulated blood. I mention this, that you may recollect what takes place in these inflamed veins, not recommending the practice, which is quite wrong, as I shall explain by-and-by. The effect of such inflammation is to give the patient a good deal of pain at the time, but he is benefitted by it afterwards. The coagulum fills up the vein, the vein becomes obliterated, and the varicose cluster is cured: others may form, but this one is cured. So in an inflamed pile, other piles may form, but the first is cured, and never troubles the patient afterwards. By degrees the inflammation subsides, the coagulum becomes gradually absorbed; as the absorption proceeds, the sides of the vein approximate, and the cavity is obliterated.

In old cases of varicose veins, you will frequently find the skin become inflamed—that is, it will look red, and be very irritable and tender. Sometimes you find the cuticle as it were abraded, and an ichorous discharge takes place from the red cutis. In some cases the whole of the skin of the leg is in this condition. In others there is a chronic inflammation of the cellular membrane. There is effusion of serum into it, and the limb becomes cedematous. When there is disease of the heart, preventing the due passage of the blood through its cavities, the fluid part of the blood is liable to escape from the capillary vessels, and thus you have anasarca of

the legs. But the swelling which takes place in varicose veins does not exactly correspond to anasarca connected with disease of the heart. It is the result of an inflammatory action in the cellular membrane; the fluid has a more distinctly serous character. If you puncture the parts with a needle, the fluid being of greater consistence, does not flow out so rapidly as the thinner fluid escapes after puncture in the case of anasarca.

The inflammation of the skin, and the inflammation of the cellular membrane, in these cases correspond with each other. There is an exudation of serum in one case from the surface of the skin, and in the other from that of the inflamed cellular membrane. These inflammations seem to correspond with those which we meet with in other cases of venous congestion.

But in some instances you find inflammation taking place of a different kind, in the cellular membrane, immediately surrounding the varicose cluster. The cellular membrane becomes infiltrated with coagulated lymph, and then the varicose cluster is, as it were, imbedded in a considerable mass of indurated substance. At first you would suppose that the veins there are obliterated, but they are not. You have a deposit of lymph on the outside, and the blood remains quite fluid. If you put your finger on the hard lump, the course of the vein is readily distinguished by the fluidity of the blood. You feel the fluid blood passing in an open channel, as it were, through a hard or gristly mass. Where there is this deposit of lymph in the cellular membrane round the vein, the skin becomes inflamed, and it may give rise to a troublesome ulcer.

But still, a varicose ulcer does not generally begin in this manner. Usually, the skin being distended at some point, a scab forms upon it. Then the scab comes off, there is an ulcer, and the ulcer spreads. The varicose ulcer, in most instances, begins about the inner ankle; but it may occur, as in the patient whose case is now before us, in other parts of the leg.

Varicose ulcers, in most cases, have a well-marked character, for which, however, you are not at this time to look in this patient, who has been confined to her bed for nearly a week. For the true character of varicose ulcers, you must look at a patient's legs who has been walking about up the time of your seeing her. Varicose ulcers are inclined to assume an oval form, the long diameter of the oval extending in the course of the vein upwards and downwards. These ulcers are generally nearly on a level with the surface of the surrounding skin. The surface of them is dark coloured when the patient is erect, and when the small veins are filled with

blood; but when the patient lies down, the surface becomes florid. The change takes place very speedily from dark to florid, and from florid to dark. The skin, and the margin of the ulcer, are generally of a dingy-red colour, and partly deprived of the cuticle, so that it is difficult to say where the latter terminates and the ulcer begins. These ulcers are generally very irritable and painful, and sometimes are disposed to bleed.

These are the principal circumstances that I have to notice respecting the history of varicose veins of the legs; and now I shall offer to you some observations respecting the treatment to be employed.

Why is it that the superficial veins enlarge, and not the others? Because, as I have already explained, the deep-seated veins have pressure made upon them on every side, but the superficial veins have not. The first thing for you to consider in the treatment is, whether you cannot put the superficial veins, which are dilated and varicose, under the same circumstances with the deep-seated veins which are uniformly supported. This may be accomplished by applying a bandage to the leg. And what kind of bandage? In many cases you may apply merely a partial bandage of adhesive plaster, which will answer the purpose perfectly, giving the patient scarcely any inconvenience. Where the disease is of limited extent—where, for instance, there are only two or three varicose clusters, of small size—you need not trouble the patient with a complete bandage for the whole leg. Have some stripes of adhesive plaster, three or four inches long, according to circumstances, and one inch or an inch and a half wide. First of all, let the patient stand erect, that you may ascertain exactly where the varicose clusters are situated. Having marked the place, let the patient recline and let the foot be raised, so that the blood may run down, and the varix become completely empty. Observe, that the heel ought to be the highest part of the whole person. Then you put on one of the pieces of adhesive plaster across the varicose vessels, and afterwards apply the others in the same manner, drawing up the skin under them, and taking care that the plaster is not thrown into wrinkles or folds. These plasters being applied when the veins are empty, and being strained on the skin beneath, when the patient stands the veins are prevented swelling. In a great many cases you will find that this is sufficient to give all the support required, and perhaps this is all that the patient needs for the whole of his life. A lady consulted me, some years ago, with two or three varicose clusters on the inner ankle and on the back of the leg, but with no varicose veins of any consequence elsewhere. I

put on some pieces of plaster in the manner which I have described. I mention this case only for this reason—that I recommended the treatment seven or eight years ago, and that lately, when she came to London to consult me on another disease, she told me that she had worn the plaster up to this time, and that it had given her complete relief; she had never had occasion for any thing else. But when the veins of the leg are extensively varicose, this compression will not be sufficient, and then you must apply a bandage for the whole leg. There are different kinds of bandages, and sometimes one sort will answer best, and sometimes another. You may use a common roller of coarse unbleached calico, such as we use in the hospital. In some persons you will find a flannel roller more convenient; at any rate, the patient can apply it better for himself. In private practice I frequently recommend a bandage which is made of stocking web. This is a very nice bandage, and very convenient, as the patient can more easily apply it for himself: there is not the dexterity necessary which is required in the application of a common roller. But it will not do for hospital practice, because the bandage is good for nothing after it has been three or four times washed, and because it is too expensive for the lower class of persons.

I must here make a few observations respecting the application of a roller. A bandage should be applied in the morning before a patient goes about, but it need not be worn in the night when the patient lies down. The bandage should begin at the toe, and go up the leg; and you should take care so to apply it as to support the heel. It should be so adapted to the limb as to make uniform and moderate pressure. The pressure should be as nearly as possible equal throughout. Especially it ought not to be tighter above than it is below, for in that case the veins below, where the pressure is least, must necessarily swell. A tight garter increases varicose veins; and the patient ought to be told not to wear a garter at all, but to loop up his stocking. A bandage which is tighter above than below corresponds to a tight garter. But some persons cannot well apply a bandage for themselves, and for them you may prescribe a laced stocking, which is in many respects very convenient. Patients who are awkward in applying a bandage may manage the laced stocking very well for themselves. Laced stockings are made of various materials. The Chinese manufacture a calico called *nauquin*, which is a very good material for the purpose. Some laced stockings are now made partly of Indian rubber cloth, so that they are elastic. An ingenious artist in Jermyn-street makes a laced stocking of

spiral wire, like the springs of braces, but of very fine texture, included within folds of leather or something else. Whether you use spiral wire, or Indian rubber, it is not necessary that the whole of the stocking should be made of the elastic substance; you only want elasticity in a part of the circumference. In most cases I find the Indian rubber cloth to be the best of these elastic materials. Patients complain of the elastic wire cloth being very hot, and besides, if any thing, it makes rather too much pressure. Indian rubber cloth, however, is not very well adapted for hot weather, as the cloth gives way so, that there is not a sufficient support, and hence it does not answer so well as common calico or nauquin in hot climates. However, you will find that each kind of laced stocking has its advantages in particular cases.

So much as to the general treatment of varicose veins; but now we are to consider their treatment under peculiar circumstances. Let us suppose, then, that you are called to a patient in whom there is a varicose cluster of veins in a state of inflammation. There is a great deal of tenderness in the part, and perhaps some fever. The first thing you have to do is to keep the patient in bed, in the horizontal posture, so as to keep the veins emptied of their blood. Then, if there be much inflammation, and the patient suffers a good deal of pain, you may apply leeches; but do not apply them immediately over the veins: they should be applied higher up on the leg, on the sound skin. The biting of a leech over an inflamed vein will give the patient a good deal of pain, and the bite will be difficult to heal. If you apply it on the sound skin in the thigh, or the upper part of the leg, you will relieve the varicose veins just as much as if you had applied it upon them, without giving the patient pain at the time or any trouble afterwards. You may then apply to the inflamed varix a compress wet with spirituous lotion, unless the pain be very great, and then you may use poultices and fomentations instead.

When inflamed varicose veins are distended with coagulum, it used to be the practice formerly to slit open the vein, and turn out the coagulum, but it is not the practice that I should recommend. It is, in fact, very bad practice, and in order to impress this observation the more upon your minds, I will mention a particular case, which I found this morning in looking over one of my old case books. It occurred upwards of twenty years ago. A patient was admitted into the hospital with two or three large clusters of varicose veins. They were all in a state of inflammation; the upper one was the most inflamed.

The patient said that she had had the disease for some years, but that about a week before her admission she had stood for a long time upon a cold stone floor, on a cold damp day. She went to bed, and had a shivering, which was followed by fever, and then this attack of inflammation of the veins took place. I could feel that the blood had become coagulated. I opened the upper varix and let out the coagulum; but the varices below were treated with cold lotion, or in some other simple way. Under this treatment the inflammation very soon subsided in the varicose clusters below, the absorption of coagulated blood began to take place, and the clusters were cured. But observe what happened in the cluster that I had punctured. The puncture became an ulcer, which would not heal, but became very troublesome. At the end of six weeks when the other clusters were well, there was a nasty sore here. I was obliged to make a slough with caustic potash, which I suppose destroyed the remains of the vein which had been opened. The slough came away, the sore assumed a healthy character, and got well, but certainly the patient would have been well some six or eight weeks sooner, if I had pursued the same practice with the upper varicose cluster which I adopted with the lower ones.

The treatment of these clusters of inflamed varicose veins should be just this:—lay the patient in bed; put a cold lotion on the part, or fomentation and poultices if you find these to be more comfortable to the patient; administer purgatives according to circumstances; and if there be much inflammation, but not otherwise, apply leeches to the sound parts above. The result will be, that the veins of the inflamed varix will become obliterated, and the varix will be cured.

WESTMINSTER MEDICAL SOCIETY.

Saturday, Oct. 22, 1837.

R. QUAIN, ESQ., IN THE CHAIR.

Rare Case of Fracture of the Humerus—Alum to Wounds received in Dissection.

THIS was the first meeting of the Society for the session 1837-38. After the usual routine business of nominating members to fill the offices of President, Vice-President, and Committee, for the ensuing year,

Mr. HALE THOMPSON related the following case, which had recently occurred

at the Westminster Hospital. A man was admitted, having received a severe blow on the humerus; and on examination there was every evidence of dislocation having taken place downwards and forwards, under the pectoralis major. It was apparently reduced; but on his being seen the next day, all the original marks of dislocation were present; and on a more careful examination it was discovered that the humerus was split from its head down one-third of its shaft, the split portions resembling the letter V: one portion remaining in the glenoid cavity, the other portion being under the tendon of the pectoral muscle, external to the joint. All attempts at retaining the two divisions of the bone in the glenoid cavity had failed, and Mr. Thompson gave it as his opinion that the man would still have a useful joint, unaccompanied, however, with that perfect freedom of motion which is present in its normal condition.

Mr. COSTELLO remarked that the case was an exceedingly rare one; and a long discussion ensued as to the structure which would fill up the triangular space between the split portions of bone. It was generally considered that it would be osseous in its texture.

Dr. JAMES JOHNSON observed, that as the dissecting season had now commenced, he would draw the attention of the Society to a circumstance mentioned by Dr. Macartney, of Dublin, at the recent meeting at Liverpool. It was to the effect, that a saturated solution of alum, applied for two or three days to a dissection-wound, would prevent any abnormal or dangerous effects occurring from it. He (Dr. M.) had never suffered since he had employed the remedy himself, nor had any of his pupils. He supposed that the intensely astringent power of the remedy constricted the absorbents and vessels of the part, and prevented their taking up any of the poisonous matter into the system.

Mr. HALE THOMPSON remarked, that the late Mr. Brookes always employed a similar preparation himself after receiving a wound in dissection.

Mr. JOHN GREGORY SMITH, after observing that wounds received from dissecting those who had recently died of puerperal peritonitis were generally the most severe, remarked that Mr. Dermott supposed that the injection of saline fluids into the vessels prevented many of the dangerous consequences occurring after receiving a dissecting-wound, as he had frequently seen among his own pupils.

The usual hour having arrived, the Society adjourned.

PHYSICAL SOCIETY, GUY'S
HOSPITAL,

October 21, 1837.

MR. CHAPMAN IN THE CHAIR.

Effect of Posture on the Pulse.

DR. GUY read some short observations on the effect produced by change of posture on the pulse. After stating that the researches hitherto made were incomplete, and that much scope was still left to inquiry, Dr. Guy proceeded to detail the results established by more numerous and more varied experiments than had yet been made. The following short propositions give a connected view of the results established by former observers, and those to which Dr. G.'s own experiments have conducted him:—

1. The effect produced by change of posture on the pulse, is, in both sexes and at all ages, considerable.

2. The difference between the erect and sitting postures is much greater than between the sitting and recumbent postures; being in men more than twice as great, and in females more than ten times as great.

3. The exceptions to the general rule are numerous; and assuming the pulse of healthy males as the standard to which that of females is to be compared, there are thirty-eight exceptions in a hundred in males, and sixty in a hundred in females.

4. The extremes are very remote from the mean results. Thus the difference between standing and sitting may be as high as a third and as low as $\frac{1}{15}$ of the number of pulses in the erect position; between sitting and lying, as high as a fourth and as low as $\frac{1}{3}$ of the number of pulses in the sitting posture; and between standing and lying, the difference may be little less than $\frac{1}{2}$, and as little as $\frac{1}{15}$ of the number of pulses in the erect position.

The following propositions relate to the difference between the pulse of males and that of females:—

1. The pulse of females is much more frequent than that of males.

2. Change of posture has much less effect on the pulse of females than on that of males.

3. The exceptions to the general rule are much more numerous in females than in males.

4. Of these exceptions, the most remarkable are those in which the pulse is more frequent in the recumbent than in the sitting posture.

The following general law, now stated for the first time, is important:—

In males (and probably in females too), the effect of change of posture varies directly as the frequency of the pulse.

The next proposition refers to the cause

of the effect produced by change of posture.

The effect produced by change of posture on the pulse is due to muscular contraction.

Dr. GUY concluded his observations by stating the following general law:—

Muscular contraction, whether employed to change the position of the body or to maintain it in the same position, accelerates the pulse; and the effects of change of posture form merely a particular case of this general law.

The number of experiments from which the above averages are formed, is 160 experiments on healthy males, whose average age was twenty-four years, and 50 on healthy females, whose average was twenty-eight years. All the persons experimented on were in a state of rest, unexcited either by food or exercise.

At the conclusion of Dr. GUY's paper, Mr. GORHAM stated the results of some experiments made by himself on the pulses of infants. These remarks, though irrelevant to the paper, were valuable, inasmuch as they showed the average amount of pulses in children at different ages, which were as follows:—

	Mean Pulses.
From birth to one day old	123
From one day to one week old . .	128
" one week to one month . .	135
" one month to five months, .	149
" five months to two years . .	130
" two years to four years . .	112
" four years to ten years . .	108

The discussion on Dr. GUY's paper was ably sustained by Messrs. Dendy, Iliff, Blenkarn, Hilton, Gaslee, &c.

Dr. GUY, in reply, stated that his observations did not yet admit of practical application, but must be considered as preparatory to the investigation of diseased conditions. Statistics formed the closest approximation to certainty, of which the science of medicine was capable. They did for probability what mathematics did for demonstration; and the objections urged against them affected their application only—their intrinsic value remained the same. In the course of the discussion, Dr. G. vindicated his claim to originality, by comparing his experiments with those of Drs. Knox and Graves; and stated that others had only surmised, while he had proved, that muscular contraction was the cause of the difference in pulses in different positions.

The thanks of the Society were voted to Dr. GUY, for his valuable communication, and the meeting adjourned.

At the next meeting, November 4th, Dr. HUGHES in the chair, Dr. ASHWELL will read a paper on Haemorrhage from the unimpregnated Uterus, associated with hard Tumors.

OF

DRUGS ON SALE IN THE ENGLISH MARKET,

With their Prices and several Duties.

(From the Official Returns up to Tuesday, October 22, 1837.)

	PRICE.			DUTY.			DUTY PAID	
	£	s.	d.	£	s.	d.	In 1837 to last week	Same time last year.
Aloes, Barbadoes, D.P. c	12	0	0	to 30	0	0	{ B.P. lb 0 2 } { F. lb 0 8 }	85,785
Hepatic (dry) BD. c	5	0	0	14	0	0		
Cape, BD. c	1	10	0	1	16	0		
Aniseed, Oil of, German, D.P. lb	0	9	6	0	9	6	F. lb 1 4	251
E. I. lb	0	7	0	0	7	6	E. I. 1 4	1,687
Assafoetida, B.D. c	0	2	10	0	5	0	c 6 0	25
Balsam, Canada, D.P. lb	0	1	3	0	1	4	lb 0 1	1,350
Copaiba, BD. lb	0	3	9	—	—	—	c 4 0	206
Peru, BD. lb	0	4	3	—	—	—	lb 1 0	666
Benzoin (best) BD. c	25	0	0	50	0	0	c 4 0	117
Camphor, unrefined, BD. c	9	0	0	—	—	—	c 1 0	441
Cantharides, D.P. lb	0	5	0	—	—	—	lb 1 0	21,290
Carraway, Oil of, D.P. lb	0	9	0	—	—	—	lb 4 0	3,483
Cascarilla or Eleutheria Bark, D.P.C. lb	1	15	0	—	—	—	lb 0 1	3,483
Cassia, Oil of, BD. lb	0	9	0	—	—	—	lb 1 4	3,046
Castor Oil, East India, BD. lb	0	0	6	0	0	10	c 1 3	5,512
West I. (bottle) D.P. 1½ lb	0	2	3	—	—	—		4,581
Castoreum, American lb	1	15	0	—	—	—		
D.P. Hudson's Bay lb	1	0	0	1	4	0	{ lb 0 6	203
Russian lb	—	—	—	none	—	—		966
Catechu, BD. c	1	2	0	—	—	—	c 1 0	23,175
Cinchona Bark, Pale (Crown) lb	0	2	0	0	3	6		
BD. Red lb	0	3	0	0	6	0	{ lb 0 1	111,492
Yellow lb	0	2	0	—	—	—		98,815
Colocynth, Turkey lb	0	2	6	0	4	0	{ lb 0 2	7,372
D.P. Mogadore lb	0	3	0	—	—	—		13,084
Calumba Root, BD. c	1	4	0	2	5	0	lb 0 2	11,033
Cubebs, BD. c	3	0	0	—	—	—	lb 0 6	31,657
Gamboge, BD. c	5	0	0	15	0	0	c 4 0	78
Gentian, D.P. c	1	4	0	—	—	—	c 4 0	430
Guaiacum, D.P. lb	0	1	0	0	1	8	c 6 0	54
Gum Arabic, Turkey, fine, D.P. c	8	0	0	9	0	0		9
Do. seconds, D.P. c	5	0	0	7	0	0		
Barbary, brown, BD. c	3	0	0	3	3	0	{ c 6 0	3,760
Do. white, D.P. c	4	15	0	—	—	—		9,320
E. I. fine yellow, BD. c	3	0	0	3	10	0	{ c 6 0	1,949
Do. dark brown, B.D. c	1	15	0	2	5	0		2,594
Senegal garblings, D.P. c	4	15	0	5	0	0	c 6 0	3,880
Tragacanth, D.P. c	8	0	0	12	0	0	c 6 0	321
Iceland Moss (Lichen), D.P. lb	0	0	2½	0	0	3	lb 0 1	12,243
Ipecacuanha Root, B.D. lb	0	2	6	0	2	9	lb 1 0	8,727
Jalap, BD. lb	0	1	10	—	—	—	lb 0 6	44,711
Mauna, flaky, BD. lb	0	4	0	—	—	—		41,757
Sicilian, BD. lb	0	1	7	—	—	—	{ lb 0 3	16,206
Musk, China, BD. oz	1	0	0	1	8	0	oz 6 0	1,877
Myrrh, East India, BD. c	5	0	0	14	0	0		1,245
Turkey, BD. c	2	0	0	11	10	0	{ c 6 0	115
Nux Vomica, BD. lb	0	8	0	0	9	0	lb 2 6	1,120
Opium, Turkey, BD. lb	0	12	6	0	13	0	lb 1 0	1,789
Peppermint, Oil of, F. BD. lb	1	1	0	—	—	—	lb 4 0	33,795
Quicksilver, BD. lb	0	3	6	—	—	—	lb 0 1	1,015
Rhubarb, East India, BD. lb	0	2	0	0	3	0	lb 1 0	235,429
Dutch, trimmed, D.P. lb	0	3	6	0	4	0		237,989
Russian, BD. lb	0	8	3	—	—	—	{ F. lb 1 0	35,843
Saffron, French, BD. lb	0	18	0	0	19	0		5,873
Spanish lb	1	1	0	—	—	—	{ lb 1 0	4,321
Sarsaparilla, Honduras, BD. lb	0	1	0	0	1	9	lb 0 6	3,994
Lisbon, BD. lb	0	2	0	—	—	—		88,054
Scammony, Smyrna, D.P. lb	—	—	—	—	—	—	{ lb 2 6	96,900
Aleppo lb	0	12	0	0	15	0		6,252
Senna, East India, BD. lb	0	0	3	0	0	4	{ E.I. lb 0 6	83,593
Alexandria, D.P. lb	0	1	6	—	—	—		72,648
Smyrna, D.P. lb	0	1	0	0	1	3	{ Other sorts 0 6	48,016
Tripoli, D.P. lb	0	1	0	0	1	3		53,430

‡‡‡ BD. In Bond. — c. Cwt. — B. P. British Possessions. — F. Foreign. — D. P. Duty paid.

ACCIDENTS AT THE LONDON HOSPITAL.

Mr. ANDREWS, Surgeon.

Mr. ADAMS, Assistant-Surgeon.

Oct.	Sex.	Age.	Case.
10.	M.	17	Fractured humerus.
	F.	8	Scald.
	M.	5	Scald.
11.	M.	30	Contused shoulder.
	M.	45	Retention of urine from paralysis of bladder succeeding fever.
	M.	70	Fractured femur.
	M.	52	Compound fracture of tibia.
	M.	18	Fractured thigh.
12.	M.	21	Compound fracture of tibia.
13.	M.	48	Cut head.
	F.	6	Fractured femur.
	M.	5	Fractured tibia and fibula.
14.	F.	7	Cut head.
	M.	12	Hydrophobia two months after the injury.
15.	M.	22	Contused side, supposed fractured ribs.
16.	M.	46	Cut head.
	M.	41	Fractured leg.
	M.	35	Fractured leg.
	M.	32	Sprained ankle.
	M.	20	Injured eye.
	M.	58	Wounded arm.
	F.	14	Cut head.
17.	F.	27	Contused side.
			In-patients 23
			Out-patients 53
			<hr/>
			Total 76

Hydrophobia.

In the case of hydrophobia, the boy had been bitten about two months prior to the occurrence of the disease, by a dog; and it appears that the animal was killed soon after. The boy was taken to a druggist's, and the part, according to the statement of the mother, was freely rubbed with nitric acid. The mother brought the child to the hospital two days after, and, as there appeared some doubt as to the condition of the dog, and as the acid had been freely employed, the parts being in a state of inflammation, it was deemed prudent not to meddle with it further. A poultice was ordered, and the child speedily recovered. The disease, in its access and progress, presented nothing unusual. Immediately on his admission the parts were excised, not with any view of arresting the disease by the mere excision, but in order that a free absorbing surface might be presented for the introduction of remedies. The treatment consisted in the application of the extract of belladonna to the arm; and one grain of the same remedy, with three of calomel, were administered by the mouth every hour. The symptoms ap-

peared alleviated at first; but in about six hours he became rapidly worse, and died in about thirty hours after the establishment of the disease.

The post-mortem examination discovered nothing more than is ordinarily met with in such cases.

LITERARY NOTICE.

NEW WORK ON MATERIA MEDICA.

Mr. Pereira has in the press, and will speedily publish, "Elements of Materia Medica and Therapeutics."

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, Oct. 26, 1837.

George Mass, Whitby, Lincolnshire.—Augustus Paul, Exeter.—Thos. Harper, Plymouth.—Henry Letheby, Plymouth.—Thomas Griffiths.—Charles Chapman.—James George Atkinson, Wakefield, Yorkshire.—Alfred Maeta, Norwich.—Jas. Pearson, Langshaw, Lancaster.—Robert Brookes, London.—Peter Hulme Edge, Salford.—James Barrington Prowse, Bristol.—John Bubbers Mather, London.—William Thomas Bowler, Warsop.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, Oct. 24, 1837.

Age and Debility	17	Fever, Typhus	2
Apoplexy	2	Heart, diseased	1
Asthma	6	Hooping Cough	2
Cancer	1	Inflammation	4
Childbirth	2	Bowels & Stomach	2
Consumption	33	Brain	4
Convulsions	16	Lungs and Pleura	2
Croup	1	Measles	9
Dentition or Teething	2	Miscarriage	1
Diarrhœa	2	Mortification	1
Dropsy	7	Paralysis	1
Dropsy in the Brain	5	Small-pox	3
Epilepsy	1	Thrush	2
Erysipelas	1	Unknown Causes	13
Fever	11		
Fever, Scarlet	1	Casualties	4

Decrease of Burials, as compared with } 58
the preceding week }

METEOROLOGICAL JOURNAL.

Oct.	Thermometer.	Barometer.
Thursday . . . 19	from 32 to 57	30.22 to 30.31
Friday 20	40 63	30.35 30.55
Saturday . . . 21	39 59	30.48 30.44
Sunday 22	38 59	30.36 30.24
Monday 23	49 59	30.00 29.75
Tuesday . . . 24	47 54	29.56 29.49
Wednesday . . 25	32 47	29.53 29.92

Prevailing wind, S.W.

Except the 23d, 24th, and morning of the 25th, generally clear; with rain.

Rain fallen, .55 of an Inch.

CHARLES HENRY ADAMS.

NOTICE.

Dr. Hawkins's case of hydrophobia came too late for the present number.

WILSON & SON, Printers, 57, Skinner-st., London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, NOVEMBER 4, 1837.

LECTURES

ON THE

PHYSIOLOGY AND DISEASES OF THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL
AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of
1836 and 1837,

At the Anatomical School, Kinnerton-Street,
near St George's Hospital,

By CHARLES J. B. WILLIAMS, M.D. F.R.S.

LECTURE VI.

Physical Examination of the Chest (continued)

—Acoustic properties of the Chest tested by Percussion—Causes of the sound of Percussion—Modes of Percussion; immediate and mediate—Varieties of the Sound in different Regions; in different states of Respiration—Practice of Percussion—Acoustic properties of the Lungs, manifested by the motions of Respiration—Sounds of Respiration, Inspiration, Expiration—Tracheal, Bronchial, and Vesicular Sounds—Varieties of the Sounds; Puerile Respiration, &c.—Duration of the sound of Respiration, &c.

WE have now, then, to consider the acoustic properties of the chest and its organs. We have explored these parts in their relations to sight and touch, and we have next to examine them in relation to sound, which we now know to be merely a kind of resisted motion.

Well, as we tried the sonorous properties of metal and of wood, by striking them, and observing the character of the resulting sound, so we strike the chest to judge of the nature and condition of its materials. The practice of percussion, as a mode of diagnosis, we owe to Avenbrug-

ger. We will first inquire into the principles of this process. You hear that the chest, when struck abruptly with the ends of the fingers, gives a rather deep, and not very short sound; which implies that the vibrations are not quick, and that they do not instantly cease. When I strike in the same manner on the thigh, there is a very different sound—a short dull tap, implying that the vibrations do not continue. I get the same dead tap when I strike that part of the chest in which the liver lies; but you may observe, by referring to the diagrams, that those parts of the chest under which the lungs are, all yield more or less of the deep hollow sound.

Let us examine a little more into the seat of this sound. Is it in the air, or in the solids of the chest? We will first try the properties of sounds produced in the air of hollow bodies. Here is an open India rubber bottle, which gives, when I tap on it, a hollow sound. But if I close its mouth with my finger, you hear how the sound is changed. Try this with the chest: I close the glottis whilst I strike my chest; it does not sensibly alter the note. Again, the sound in the air of hollow bodies is deep in proportion to their size: remark how much deeper the tone of this large bottle is than that of the little one. Try this with the chest: by enlarging and diminishing its hollow by inspiration and expiration, the sound on percussion remains much the same, except that the extremes of inspiration and expiration slightly raise it; whereas, were the sound produced in the air within, the increase of this air should uniformly lower the sound, and the diminution raise it.

As it is not, then, the air that sounds on percussion, it must be the solids; and observe how the solid construction of the chest favours its vibrations. Here are a set of membranes, thin muscles, and integuments, strained on an elastic framework of bone and cartilage, and free to

vibrate so long as the organs within do not check their motions. Let us find something familiar to illustrate this; my hat will do very well. The crown of this hat is free to vibrate, and gives a good hollow sound when struck. I put into it this handkerchief, which, when loose, may serve to represent the air filled to sue of the lung. It still sounds well, because the handkerchief does not check the vibrations. But when I bring my hand in contact with the crown inside, and then strike on the outside, you perceive the sound is rendered short and dull, because the vibrations are stopped by the solid within. Suppose I were to fill my hat with water (for I am not going to do it; I won't try its *water-proof* so far); this, too, would check the vibrations, and you would have no sound on percussion but a short dead tap.

It is just so with the chest. It is in itself more or less free to vibrate; but it receives the character of its vibrations from the organs underneath, which return them, or check them, according to the density of these organs. Thus you find, where the lungs lie, the chest sounds deep and clear; but here, below the sixth rib on the right side, where we come over the liver, or here, to the left of the lower part of the sternum, over the heart, the sound is short and dull, the vibrations being checked by these soft solid organs beneath. So also you can at once perceive that morbid changes of the organs, such as condensation of the lung by disease, or the pouring out of serum into the pleura, would in a similar way arrest the vibrations, and render the sound dull in those parts of the chest where these changes occur. On the other hand, suppose changes of an opposite kind to take place, such as dilatation of the air-cells of the lung, or an effusion of air into the pleural sac, the walls of the chest will then have under them an increased spring, which will make them vibrate on percussion even more strongly than usual, and yield a clearer sound.

These illustrations are enough to shew the general principles of the acoustic examination of the chest by percussion. It is a test of the density and elasticity of the materials within the chest: as diseases alter these qualities, so will they alter the sound on percussion, which may thus announce their presence. A few more considerations will help us to some useful practical applications of these principles.

As we have seen that the walls of the chest give the sound which we hear on striking the chest, so it is plain that they must be sufficiently tense and elastic to vibrate on being struck. The chests of some persons are so loosely put together, and so flaccid, that they give but little sound, although the organs within are

quite healthy. In others, again, there is such a mass of fat and loose integument on the chest, that the walls are completely muffled by it, and they sound but little on percussion. The same difficulty occurs in most cases in certain regions, where muscles of considerable thickness, or the mammae in females, lie on the walls. In other cases, again, the walls of the chest are so drawn in by contracted adhesions, that they are not free to vibrate, and give a dull sound, although the lungs within them may be comparatively healthy. In all these cases we must give to the part struck the equal tension which is wanting, by pressing on it a small piece of some firmly elastic body, such as wood, ivory, stiff India rubber, or the like. You can get plenty of sound by striking this; and if it be firmly applied to the chest, the density of the contents within will modify this sound, just as it modifies that of percussion of the naked walls of the chest. You perceive that the sound which I get on striking on this little plate of ivory pressed on my chest, is the same in character as that which I get by striking the chest itself, only it is louder; and as percussion on it gives no pain, I can strike with force sufficient to make the vibrations reach the interior, through any thickness of fat or muscle. By this means we can get at the sonorous qualities of the thoracic viscera through the scapulae and muscles of the back, and through fat or oedematous integuments of any thickness. In this way, too, we can get the resonance, or sonorous quality, of any part of the abdomen. In some regions, as in the right iliac and left hypochondriac, the gaseous distension of the colon and stomach often stretches the solids over them enough to make these solids yield a drum-like sound when struck; but the pressure of a solid plate on them will always give these and other regions such a tension, that a stroke on its surface will be modified by what lies underneath.

We owe this method of *mediate* percussion to M. Piorry, who calls this percussion-plate a *pleximeter*; and it is so much better than immediate percussion, that it is now generally preferred. There is, however, an improvement on it, which is, I believe, due to an Englishman, Dr. Skerrett; this is, to substitute for a pleximeter the fingers of the left hand. I need say little to recommend this method of percussion; if you do not already know it, you will soon find out how convenient and *handy* it is, how nicely you can fit the fingers to the inequalities of the chest, sometimes singly, sometimes together; sometimes with their palmar surface outermost to strike on, but generally with this applied to the chest and their backs to strike on, and so forth, as I will shew you at a future

lecture, when I will give you practical demonstrations of all the different methods of examination.

Now that we know how to percuss the chest so as to make it sound, let us see how this sound in different parts ought to be affected by the organs within; and here, again, you see we have need of the topographical knowledge that I said was necessary for a correct physical examination of the chest. These diagrams will assist us; and for the different sounds on percussion in different regions, I must refer you to the table and plates at the end of my work on Diseases of the Chest.

You see here, coloured light pink, the shape of the lungs seen through the parietes; and over the whole of that you get, more or less, the resonant pulmonary sound on percussion. In all the upper parts of the chest, before, behind, and at the sides, this sound is pure and unmixed, and equal on both sides, because there is nothing but lung underneath, as you see represented in the drawing by the unmixed pink. But between the fourth and fifth rib on the right side, you see this pink begins to get a shade of purple from the liver, that here rises into the central part of the chest beyond the lung; and this purple tint becomes darker and darker as the lung is thinner, and the liver approaches the surface, until we come to the margin of the lungs, where the light pink of the lung quite ceases, and the unveiled purple of the liver denotes that this organ is in contact with the walls of the chest. Now, is there any mixture of the lung and liver sounds where we see this blending of their tints in the diagram? I say yes; and I will give you the proof of it when we have a living subject before us. As soon as we get down to the upper level of the liver there is a very slight deadening of the pulmonary sound on percussion; and this deadening increases down to the margin of the lungs, below which it is entirely changed into the dead hepatic sound. You see by this that the stroke of percussion reaches a considerable depth, to organs an inch or more from the walls; and whatever it reaches may modify its sound. This suggests to us, that by varying the force of the stroke, we may make the impulse of percussion reach to different depths, and derive the character of its sound from the superficial or the deep-seated organs, as we will. So, after some practice, you will find that where the lung overlaps the liver, *strong* percussion will give you a shorter, deader sound, than *gentle* percussion. Strong percussion receives the character of its sound from the liver as well as the lung; whilst gentle percussion, as by *filliping* with the finger and thumb, does not pass beyond the thin

layer of the lung, and gives still the pulmonary sound.

Let us look to the left side of the chest. Here, at the sternum, about its junction with the third rib, we see the light pink of the lungs taking a scarlet tinge from the heart, and you can see its shape blushing through the lungs down to between the fifth and sixth ribs, about an inch below and before the left nipple, where, from the brightness of the colour, you may see that the apex of the heart approaches very near the walls of the chest; and to the left of the lower third of the sternum, its body is in contact with them. So there is a mixture of dull and clear sounds on percussion in these situations, although, from the movements of the heart and lungs, the spots where one shades into the other are not fixed. You see, in the lower part of the left side, a yellow tinge, which rises, in a faint degree, as high as the mammilla. This indicates the vicinity of the stomach, with its clear drum-like sound, which becomes mixed with the pulmonary sound in this region, but to a degree varying according to the quantity of air contained in the stomach. Here, again, is the spleen, which, as you see by this blue tint, affects the sound of the small portion of the lateral lobe of the left lung.

Now you must not forget that the motions of respiration may produce changes in the character and relative positions of some of these sounds. Inspiration, as it enlarges the lung, renders the pulmonary sound clearer, and extends it over every part of the heart, and over a considerable portion of the liver. As we found that the complete and equal enlargement and contraction of the chest, as seen and felt, are signs of the free condition of the respiratory organs, so the sound on percussion becomes an additional sign of the healthy action in proportion as the clear pulmonary sound is extended at each expansion of the chest. Observe, when after a full expiration I strike my chest over the region of the heart, and over the middle region of the liver, the sound is dull, and scarcely pulmonary. I take in a full breath; and now you hear how clear it sounds all over these regions. Percussion is a test, therefore, not only of the *statical* condition of the lung, but of the *dynamical* state also. This point is not enough attended to by auscultators, and yet the neglect of it not only would deprive us of additional signs, but would tend to render deceptive the results of statical percussion. For example, in judging of the goodness of the sound on percussion, we generally compare the sounds on the two sides of the chest, or in two parts of the same side where naturally the structures and sounds

are the same: but if we do not attend to the movements of respiration, we may strike one part when the chest is contracted, and the other when it is full, and obtain results which differ from this cause only, and not from any internal change. In practising comparative percussion, therefore, in cases requiring delicacy, you should desire the patient to hold his breath for an instant whilst the comparison is made, and it is often useful to try the sounds when the chest is expanded to its utmost, when it is contracted, and in the intermediate states.

It would be going out of my order to describe the varieties in percussion from special diseases, but an illustration or two more will enliven the subject.

The indurations of the upper lobes of the lung in the early stages of phthisis are often small, and so scattered through its substance, that they scarcely affect the sound on percussion; but by a full expiration they are brought closer together, and if more on one side than the other, they may then more sensibly deaden the sound on that side, especially if gentle mediate percussion be used below the clavicles, and not on a very small surface. Again, the indurations, especially if of some standing, tend to restrain the lung from its full expansion, and if there be a difference on the two sides, it thus may be detected only on a full inspiration. In the disease called emphysema of the lungs, the air-cells are permanently dilated; they contain an unusual quantity of air, which expiration cannot expel; this may be detected by percussion as a dynamical test; the regions of the heart and upper part of the liver being covered by the permanently distended lung, even after expiration, give a clear sound. There is one point more to notice respecting percussion at the extremes of the respiratory act. Full inspiration makes the sound clearer; full expiration, the contrary: but they both raise the tone a little; they render its pitch higher. Why is this? For the simplest reason in the world; both actions *tighten the drum*; they strain walls of the chest, and render their vibrations quicker, and therefore the sound higher. It requires a musical ear to detect these differences, and I do not think it useful to dwell further on them.

I think I need add no more on the principles of percussion; but to those who have not practised it, I would say, do so without delay, and on every opportunity; for, like every other art in which our senses are exercised, there must be practice to familiarize you with the phenomena, and the mode of obtaining them. I trust that the principles which I have given you will guide and assist you in this

practice. 'There is no better way of trying to get this familiarity than by percussing your own chests. A few minutes when you rise in the morning occasionally spent in studying before the glass the outward marks and different sounds of the regions and their corresponding organs, will go far to instruct you in the practice of percussion, and in the character of the sounds in health; and the awkwardness and annoyance inseparable from first attempts will be felt only by the person who is least likely to complain of them.

You will soon find even in these trials that some art is required even in mediate percussion, which is the easiest. The finger or fingers of the left hand should be closely pressed on the walls of the chest, and if the object be comparison of the two sides, they should be placed on corresponding parts, whether between the ribs, along them, or across them. Take care, too, that the mode of striking be the same, whether you strike with one or several fingers, with their tops (in which case you must keep your nails pretty closely cut), or the flat of the last phalanx, or with the knuckles; each of which modes is sometimes preferable. I often find *flipping* with the middle finger and thumb [thus] give more uniform and delicate results: especially when the patient is in an inconvenient position, or suffers from tenderness of the walls of the chest, this is the best mode for abdominal percussion. In doubtful cases, however, various modes must be tried and practised, as I have before remarked, with due relation to the period of the respiratory movements. There are a few cases in which a little plate of thin wood or ivory, as recommended by M. Piorry, may be used with advantage; it should be covered with soft leather, to prevent the clack of the fingers on its surface. In M. Piorry's works "*De la Percussion Mediate*," and "*Du Procédé Opératoire*," &c. you will find some useful descriptions of the varieties of sound obtained by this instrument, but he much exaggerates its advantages. If you wish to know more of the principles of percussion, you may consult a paper of mine, published in the *MEDICAL GAZETTE* of January, 1837.

Now let us inquire into other modes of producing sounds in the chest, which may prove acoustic signs of the condition of the organs within. There are the motions of respiration, will not they produce sound? The contractions and relaxations of the muscles of respiration are in general too gentle to cause sound; but when forcible or sudden, they will sometimes occasion a sound of tightening, of the kind called muscular sound. This is of no con-

sequence, and gives no signs. But there are the internal motions, and the attendant passage of air to and fro in the lungs; these produce sounds, and inasmuch as this passage of the air is the great object of respiration, we may expect to find in these sounds signs of the manner in which this object is accomplished. These, and most of the other acoustic phenomena of the motions of the chest, were first discovered and described by Laennec, who may well be considered the father of the art of auscultation. We shall endeavour so to illustrate and extend this art, by the aid of physical and physiological science, that we may be enabled to deduce respectively from the phenomena the condition of the organs, and from any known condition of the organs, the phenomena which it would produce.

The air enters the lungs by atmospheric pressure, to fill the increased space made in the chest by the action of the muscles of inspiration. On its way to the most expandible parts of the lung, the fine tubes and cells, it strikes against the sides and angles of the trachea and its ramifications, with force sufficient to produce a particular hollow blowing sound. You may hear this on applying the ear to the fore part of the neck, or at the top of the sternum. As the current of air becomes subdivided and spread in the small bronchi, it loses a part of its velocity, and the sound becomes of a more diffused and less hollow character; it is more like the sighing of a gentle breeze among the leaves of trees, and in passing into the cellular terminations, all of the hollow or tubular sound is lost, as you may find on applying your ear to most parts of the chest. This sound may partly depend also on the opening and stretching of the tubes and cells. Where inspiration ceases, expiration begins, and a portion of air is pressed out of the cells and small tubes by the collapse of the walls of the chest, and by the contracting properties of the pulmonary tissues. Now, remark the difference between inspiration and expiration. In inspiration the air is the moving body, and rushing through the tubes distends the passive lung. In expiration the lung is the moving body, and by its contraction (backed by external pressure) drives before it the passive air. In either case there is a pressure exerted between the air and the interior of the cells, and doubtless this proves the means of assisting the chemical changes that take place. But you can see that there must be a difference between the sounds of inspiration and expiration. In inspiration, air moving with some velocity meets with the resistance of the angles and sides of the tubes and cells which it has to dilate. Here must be sound in the whole passages

of the air, from the nostrils down to the pulmonary cells. In expiration, the motion begins with the lungs, and, the air passively yielding to it, there is not motion or resistance enough to produce sound, until by the converging together of the small tubes, the impelled air is gathered into a current in the larger tubes, where, impinging against their sides with its now acquired velocity, it at length produces sound. These remarks will enable you to perceive, that in natural respiration there are three kinds of sound—*tracheal*, *bronchial*, and *vesicular*. In expiration, at most only two, *bronchial* and *tracheal*. I am not sure who first remarked these differences, which were overlooked by Laennec. They are mentioned by Andral, Louis, and Dr. Cowan, who accredits them to the late Dr. Jackson, a young American student at Paris, whom I knew as an uncommonly zealous and clever observer. They are worth notice, and are plainly produced in the manner which I have explained. We shall say no more at present of the distinctions between these parts of the act of breathing, but class them generally, as Laennec did, as the sounds of respiration.

I have said that the sounds of respiration can be heard on applying the ear to the chest; they are transmitted through the parietes with sufficient distinctness from the parts underneath; and as the healthy sounds vary in these different parts, we may judge of the natural distribution of the tubes by listening to these sounds. Thus we find, in any part of the neck, and at the upper part of the sternum, there is the hollow blowing sound which results from the passage of air to and fro in the trachea, which is therefore called *tracheal* respiration. A little lower down than this, over the space of two or three inches on each side of the top of the sternum, between the scapulae, and sometimes in the axillae, there is the sound called *bronchial* respiration, because its whiffing or tubular character denotes that the sound is produced by the passage of air in the bronchial tubes. Then there is the *vesicular* respiration, which is heard in most other parts of the chest; it is a diffused murmur, caused by the air penetrating through the minutest tubes, and into their numerous vesicles or cells.

Now a question will occur to you (at least I hope it will, that you may be more interested in the answer), how is it that the *bronchial* respiration is heard in comparatively few parts of the chest, when we find bronchial tubes of considerable size in most parts of the lungs, to within an inch or so of their surface? Why is not the sound a mixture of the tubular and vesicular sounds? Just consider that the sound in these tubes must be conducted through

the tissue of the lung to its surface before it can reach the ear; and what sort of a conductor of sound will this tissue be? We found that equality of density or rigidity renders a body a good conductor of sound; but here we have a flaccid tissue, composed of the unequal materials, membrane and air; and sound in traversing an inch of this tissue would have to pass from air to membrane, and membrane to air, thirty or forty times. We saw how a loosely folded cloth stops sound; still more especially does this spongy tissue of the lung arrest all the slighter sounds that are produced in the tubes within it; so that what we hear outside the chest, unless the tubes underneath are of very large size, or very near the surface, is only the vesicular sound of the superficial parts of lung. You may, if you please, take a hint from this, if you want to shut out the noise of a street, or of a noisy neighbour, or if you wish to keep your own noises to yourself, you will have on the shutters, or in the walls, some loose porous body, such as wool, cotton, shavings, or the like, which will effectually arrest and choke the vibrations in their passage, and will in a measure isolate you with regard to sounds.

And now mark an important corollary to our last proposition. As this arrest of the sounds of the interior depends on the light spongy structure of the lung, so any disease increasing the density of that structure will increase its conducting power, and enable it to transmit the sounds. Hence we find that a great increase of solid or liquid in the lung, as in pneumonia or tuberculous deposit, or the compression of its superficial parts by a moderate quantity of liquid in the pleura, as in a recent pleurisy, will often not only diminish the vesicular murmur in consequence of the obstructed state of the cells, but will add also a bronchial or tubular sound of breathing in those parts where naturally the respiration is purely vesicular.

Now, I think, we understand the ordinary sounds of respiration; let us attend to some varieties of them. As these sounds depend on the resisted motion of the air, so they will vary according to the velocity of that motion, and the degree of resistance to it, they will be loud when the air passes in and out forcibly and quickly, and low when it passes gently and slowly. So when you are listening to a person's breathing, it may be scarcely audible at its ordinary rate, but if he breathe quick and short, it will be distinct enough. Taking a long breath may not answer the same purpose, for although much air is thus taken in, it may not enter with sufficient rapidity to cause the increased sound.

Coughing answers better, for the long inspiration which follows coughing is generally quick also; and it is often useful where the sounds are obscure, to magnify them by this more forcible act. But there is a limit to the power of increasing the sound of respiration by increased effort. If a person tries to breathe very hard and quick, as after violent exertion, the movements of the lungs cannot keep pace with those of the external muscles of respiration, and the air does not freely enter, the sound will be diminished rather than increased.

As we can vary the sound of respiration by varying the act in the same individual, so we find that a difference exists naturally in different individuals: in some, as in many robust adults, you find the ordinary respiratory sound very low and faint; in others, as in children, in nervous females, and in slight irritable persons, you find the sound loud and distinct. In the last cases the respiratory movements are more brisk, and although air may often not be taken in more frequently, or in such great quantity as in the other cases, yet it enters more suddenly, and meets with greater resistance in its passage, so that it must cause more sound. As this loud respiration is commonly met with in children, Laennec called it *puerile* respiration. So, also, by rendering the respiration quicker and more energetic, you may make its sound loud in those cases where it is naturally faint, as by the quick short breathing which I have just mentioned; or better still, by desiring the person to hold his breath for a while; the quick strong inspiration which then follows is noisy enough. Disease will sometimes bring about the same change: thus if a considerable portion of the lungs be obstructed, the force of the act of breathing will be concentrated on the remaining portions, and the air will be carried in and out of them with unwonted energy and noise. Hence Andral terms this partially increased respiration *supplementary*. So also, under some circumstances, without any obstruction, the want of breath may be increased—as it happens during moderate exercise, and in some degree during the process of digestion, and on exposure to cold: here respiration is more energetic, and its sound louder. Further, as the act of breathing depends on a particular impression on the nervous system, so you can easily perceive when this system is preternaturally sensitive, the ordinary impression produces an increased effect; and here, again, the respiration becomes more energetic and noisy. This is the cause of the increased sound of respiration in certain fevers and other diseases where the nervous sensibility is exalted. Lastly, it is pos-

sible, by an external restraint of some parts of the chest, to render the sound of respiration louder in other parts. Thus, if you inclose the abdomen and lower part of the chest in a tight belt (or you may meet with the experiment already prepared in the persons of tight-laced ladies), you will often find the sound of respiration in the upper parts unusually loud; and you see by the heaving of these parts how their motions are increased.

Now you see there may be much variety in the sound of respiration, without disease of the lungs: and, excepting in the case last mentioned, it is where there is a comparative difference in the different parts of the lungs, rather than any absolute difference, that disease of these organs is indicated. Thus, if you find the respiration loud on one side and obscure on the other, or clear in the lower part of the chest and indistinct in the upper, you may well suspect some obstruction to exist in those parts where the sound is obscure; and the nature of that obstruction is then to be tested by percussion and other means.

There is another kind of variety in the respiratory sound that has not been attended to—a variety in its *duration*. In this, as in the other varieties, there are absolute differences in different individuals, and in the same individual under different circumstances; but I shall only notice the comparative differences in the same subject at the same time, which alone constitute signs of disease. We sometimes hear the sound of inspiration on one side distinct and prolonged during the whole inspiratory act; on the other side loud enough at first, but abruptly stopped before the act is complete, and often stopped with a sort of *hic*. This kind of hitch in the sound sometimes reminds me of M. Alexandre, the mimic; imitating the noise of planing, he gives you the idea of meeting with a knot in the wood, by stopping short the noise with a *hic*. So the stop put to the expansion of the lung is often as sudden; and this denotes that the obstruction is then complete. Hepatization of the lower portions of the lung will do this; so will a moveable plug of tough mucus in the bronchial tubes. In other cases, again, we find the circumstances reversed; there is in a part of the lung no sound during the first part of inspiration, but towards its end, when the chest is most expanded, there is a short wheeze. This happens where the bronchial tubes are so far obstructed that air will not pass through them until they are distended by a full inspiration, as in bronchitis. It happens also in pleuritic effusions, which distend the parietes of the chest beyond the medium state of respiration; it is only the *acmé* of inspiration that can then intro-

duce air into the compressed lung, and it is at this period alone that the sound is heard.

I must not detain you now with other morbid varieties of respiration, which will severally come under our notice when we consider the diseases which cause them. There is the *cavernous* respiration, caused by the passage of air in and out of an unnatural hollow or cavity in the lung; and the *amphoric* respiration is where this cavity is so large, that the sound is like blowing into a phial, or into this India rubber bottle.

In the next lecture I will give you a tabular view of these and other varieties of respiration, and of their remarkable accompaniments, the various *rhonchi*, which are produced by an increased resistance to the passage of the air.

LECTURES

ON

RHEUMATISM AND ITS EFFECTS;

By R. MACLEOD, M.D.

Physician to St. George's Hospital.

LECTURE III.

Maniacal Delirium sometimes indicative of Rheumatic Inflammation of the Heart—Cases in illustration.—Immediate effects of Rheumatism on the Pericardium, and on the interior of the Heart; description of the anomalous appearances.—Secondary changes resulting from Rheumatism of the Heart.—Disease of the Valves; Hypertrophy and Dilatation of the Cavities—Signs of these.

I HAVE alluded to the peculiar change in the expression of the patient which usually attends rheumatism of the heart. Now this sometimes goes much farther; it passes into the anxiety of delirium, or the wildness of insanity. This is no new fact; it was referred to by Andral many years ago; was stated in his Gulstonian lectures by Dr. Francis Hawkins; was enlarged upon by Dr. Latham; and more recently, has been especially mentioned by Dr. Watson in one of his published clinical lectures. So far as I know, in all the recorded cases in which rheumatism of the heart has been attended by symptoms of inflammation of the membranes of the brain, the patients have died, and the encephalon has been found intact, or at least without any unequivocal evidence of inflammation. Indeed it is the remarkable fact of no diseased appearances having been found in the part to which the chief

symptoms were referred, which led to the cases being placed on record. I have reason to believe, however, that pericarditis may occur, accompanied by cerebral symptoms of the severest character, and still the patient entirely recover under the persevering use of proper remedies. The following appears to be a case in point:—

Mary Hall, ætat. 27, admitted in the third week of rheumatic fever, January 25, 1836. She made no complaint excepting of the limbs, but her expression was anxious; and on being questioned, she acknowledged that she had some uneasiness in the region of the heart, in which situation pressure between the ribs gave pain. Nothing abnormal in the sounds of the heart, or respiration, could be perceived.

26th.—Makes no complaint, but shrinks from pressure in the region of the heart. On applying the stethoscope to-day, a rubbing sound is distinctly perceived at the left and lower third of the sternum; considerable faintness; much anxiety; *some incoherence*.

27th.—Denies that she has any uneasiness in the chest, but is too incoherent to be trusted as to what she feels. No rubbing sound to be heard, but pulsations heard more obscurely, and as if more distant.

28th.—Constant delirium, with so much restlessness and jactitation, that no satisfactory examination of the heart could be made for several days, during which time it was necessary to put on a strait waistcoat.

On the 4th of February, during a quiet interval, the pulsations of the heart were found to be feeble, distant, and intermittent.

The same examination, and with the same result, on the 5th. The pulse on both days from 100 to 105, feeble and intermittent.

8th.—Calmer. The pulse 100, more steady, but with occasional intermissions. Sounds of the heart again approach the ear, the idea of distance being diminished. No rubbing.

12th and 13th.—A fit each day, of epileptic character, to which it does not appear that she has been subject. Complaints of pain under the left mamma; no change in the character of the sounds.

From this time she gradually recovered, having had no farther recurrence of the symptoms, and the action of the heart having become quiet and natural.

I have nothing to say of the treatment in detail, my present object being only to illustrate the extent to which the functions of the brain may participate in this form of rheumatism. I may state, however, that after the supervention of the heart symptoms, she was bled once generally,

once locally; was blistered on the chest, and took calomel and opium, with purgatives.

Since this lecture was delivered, another case has presented phenomena considerably resembling the preceding.

Thomas Spratt, ætat. 39, of rather intemperate habits, was admitted into St. George's Hospital, September 21st, having laboured under acute rheumatism for three weeks. The attack commenced in the shoulder and side of the face, but soon extended to other parts, particularly the hands and feet, which were swollen and red, while he had all the common symptoms of rheumatic fever. He was bled and purged, and took calomel and opium, without any thing to arrest attention, till the 24th, when he was observed to be incoherent, with much wildness of expression. He was ordered a grain of opium every six hours; and the gums being sore, the calomel was omitted.

25th.—The delirium had increased, so that he was at times quite unmanageable; he had had what was called by the nurse "a fainting fit;" pulse 100, with occasional intermission.

Some, but rather obscure sound of friction, and some degree of bellows sound on applying the stethoscope over the heart. Half a grain of acetate of morphia was given every six hours, and a strait waistcoat put on.

28th.—Delirium continues; puts his hand occasionally to the heart, and on being questioned, says he has pain there; but his answers have been too incoherent since the 24th, to be relied upon. The sounds of the heart are obscure; but some degree of rubbing continues.

The acetate of morphia was increased to half a grain every four, and then every two hours.

30th.—Much quieter; pulse 85; sounds of heart more distinct; rubbing no longer audible; some blowing sound continues.

From this time he gradually recovered; and the sounds of the heart became natural after a time.

In another case admitted under Dr. Chambers during the summer, a patient had furious paroxysms of maniacal delirium; and on examining the body after death, the pericardium bore evidence of very acute and recent inflammation.

I admit that, as in the cases of Hall and Spratt, no opportunity was afforded of examining the brain, so it may be matter of question whether that organ was, or was not, the seat of inflammation. My belief that it was *not*, resulted from my previous knowledge, on the authorities above quoted, that sympathetic disturbance of the brain, of the most violent nature, may take place in carditis,

though there be no organic affection of the encephalon, or at least none which leaves any trace behind. The physical signs left no doubt of the existence of pericarditis, while there was neither heat of scalp, nor injection of the eye, nor intolerance of light, such as is usually met with in inflammation of the membranes of the brain. Again, in a case recorded by M. Andral, there were delirium, convulsive twitchings, tetanic spasms, coma, and paralysis; yet all this was found by the autopsy to have arisen from pericarditis, without a single vestige of disease in the head.

While I readily admit that the assistance to be derived from the stethoscope in general practice has by some been greatly exaggerated, yet here, in such a case as rheumatic fever, with extension of the disease to the heart, producing general symptoms which are obscure, or causing sympathetic affections of a distant organ calculated to draw off the attention from the real seat of the disorganizing process, or perhaps not exciting symptoms any where calculated to raise suspicion—one and all of which conditions I have seen; I say that, under such circumstances, the assistance to be derived from auscultation is paramount; and that he who refuses to make use of it, denies himself the only means we possess of arriving at a correct diagnosis—thus placing in jeopardy the life of his patient, to say nothing of the peril to his own reputation.

But there is another affection of the head, altogether different from the preceding, which, nevertheless, I am inclined to think has been confounded with it—namely, a true inflammation of the brain or its membranes, connected with rheumatism. This, however, in all the cases of it I have ever seen, has arisen in connexion with rheumatism of the synovial membranes, and without any pericarditis. On the other hand, I have never known fibrous rheumatism affect the brain except secondarily (I believe functionally), and after the heart had become implicated.

Rheumatism of the heart, in the great majority of cases, is cured, or so far alleviated that the patient recovers from the primary attack. Unless, however, the treatment has been both energetic and judicious, the organ receives an injury which leads to slowly increasing change in its structure, and thus ultimately to a fatal result. Some, however, are cut off during the first attack, and the appearances which then present themselves are convincing evidences of the nature of the disease. The pericardium is found to have been inflamed, and as regards this membrane I know of no difference which ex-

ists between the changes effected by rheumatic and common inflammation.

The results to which the inflammation has ultimately given rise, are sometimes in description confounded with those which spring directly and immediately from this cause. In a recent case the appearances are striking. On raising the sternum, lymph is sometimes perceived even in the anterior mediastinum. The bag of the pericardium is seen to be inflamed; and here, unlike what I had occasion to mention with respect to rheumatism of the joints, the effects are conspicuous, and the inflammation marked by a greatly increased number of vessels carrying red blood. The pericardium feels pulpy, or fluctuating; and frequently on cutting through it we do not at once expose the heart, but find a layer of lymph intervening, adherent partially or more extensively to both serous surfaces.

But the abnormal appearances are not always limited to the exterior. The valves, especially those on the left side, may be inflamed. Ultimately the change is manifested by their augmented size and thickness; but at this early period, and supposing it to be a first attack, the only thing which may be visible is what might be described as a slight tumefaction at the ostia; looking from the left auricle towards the ventricle, the aperture seems narrowed; and I have seen the portion of the valve where its sides meet in closing, of a flesh-colour, presenting through a magnifying glass the appearance of minute spots, like granulations, with vessels carrying red blood entering them from the interior of the auricle. In the same subject the aortic valves were of a distinctly pink hue to the naked eye, but without any vessels which could be detected even with a glass. There was a point, like a very minute granulation, on the centre of one of them, being apparently the earliest stage of those small bead-like projections which are frequently seen at a more advanced period. M. Chomel has also described the appearance of granulations, varying in size from a pin's head to a millet seed, as occurring in a recent case both on the mitral and aortic valves; the former having unequivocal evidence of its being inflamed afforded by a portion of adherent lymph. In the case to which I have alluded, there was perhaps some narrowing of the aortic orifice, as if the parts around had been slightly tumefied; but this was not so conspicuous as in the auriculo-ventricular aperture. Red vessels could be traced from the surface of the auricle to its valve, but not so with respect to the ventricle. On the right side there was some opacity of the mitral valve, but nothing fur-

ther. The pulmonary valves were healthy. This is the earliest condition in which I have had an opportunity of examining the endo-carditis which accompanies rheumatism—that morbid change in the valves of the heart which ultimately leads to their permanent opacity and thickening, and to various important alterations, not only in the heart, but secondarily in other parts of the body.

In a case which is related in the 5th volume of the Edinburgh Medical and Surgical Journal, the inner surface of the right ventricle is stated to have been inflamed, in a girl who died of rheumatic fever, while both the mitral and tricuspid valves were coated with lymph. In the very few cases, however, which I have seen examined, where death had taken place, in the primary stage of acute rheumatism, the appearance of inflammation has been most conspicuous in the auricles, particularly the left, and at the semilunar valves, particularly the aortic; but without extending into the ventricle. And again, if we are to take opacity and thickening as proofs of preceding inflammation, such appearances in those who die of the secondary consequences of a damaged heart are much more remarkable towards the auricular than the ventricular connexions of the valves common to both cavities; while changes in the semilunar valves, if not strictly limited, are more wont to extend towards the inner surface of the arteries than the ventricles; and more especially as regards the aorta.

The above description applies to the immediate effects of rheumatic inflammation on the textures of the heart; but in our post-mortem examinations we are much more frequently called upon to witness the changes which have been produced by the slow and continued influence which follows the primary injury; and here it will be most convenient to describe what those changes are, before we speak of the signs by which they are indicated.

The lymph which has been effused on the external surface of the heart may, there is reason to believe, disappear under the influence of proper remedies, and the integrity of the viscus be in this respect restored. Indeed it is apparent that the powers of nature, aided by judicious treatment, are much more successful here than as regards the restoration of the lining membrane, where *endo-carditis* has existed; for it is not uncommon to find the valves extensively diseased, in consequence of rheumatism, where the external covering of the heart is little or not at all different from its healthy condition.

The most common morbid appearance of the pericardium, of a permanent nature,

is opacity and thickening—sometimes general, but more commonly in patches. This change is much more conspicuous on the surface of the heart than in the free part of the pericardium, and may be accounted for by portions of lymph, effused during the period of active inflammation, having become organised. It is to be supposed that some degree of thickening of the pericardium attends that condition in which it has been inflamed, and remains subsequently opaque; but the extent to which increase of actual bulk occurs is extremely limited; and wherever there is an opaque patch of perceptible thickness, this may almost invariably be separated from the pericardium by *peeling*, and conveys the idea, when thus examined, rather of a layer or layers of adventitious membrane superimposed, than of interstitial increase of dimension in the pericardium itself. Or if the bag of the pericardium appear thickened, when we come to examine this minutely we shall generally find either that layers of lymph have been effused upon the surface, or that it has been poured out between the fibrous and serous membranes, so as to separate them.

In those cases where the effusion has been more extensive, or the absorbing powers less active, the two surfaces of the pericardium become glued together by the interposed lymph, which speedily assumes an organic form—at first soft, and easily torn, but progressively becoming more dense, till it assume the appearance of a close cellular membrane. Most frequently this union between the pericardial surfaces is general, but occasionally it is partial. In yet rarer cases the adhesions exist only at one or more spots; and sometimes the connecting texture presents the appearance of bands or layers joining the surface of the heart to the bag of the pericardium.

When the parietal pericardium is universally adherent to the surface of the heart, it also frequently forms anormal adhesions externally, particularly if the patient have been subject to repeated attacks of rheumatic fever. In such case the pericardium may be firmly united to all the adjacent parts, as if a quantity of liquid size had been poured in, so as to fill up every interstice, and had then become organized.

There are no symptoms by which the presence of mere thickened patches can ever be detected before death; nor, under ordinary circumstances, can adhesion of the pericardium be convincingly ascertained during the life of the patient. Where the adhesions are partial, it has been supposed that pain or other distressing sensation would be

produced when the individual was placed in such position as to cause any degree of suspension or dragging upon these adventitious ligaments; we should also expect adhesions about the apex of the viscus to disturb its action more than such as are situated at its basis. Louis has given a case, in which the patient was unable to lie on the back, in consequence of a smarting pain in the region of the heart being excited by this posture. On examining the body, a single firm adhesion was found extending along the right ventricle; so that in the supine posture this must have been dragged upon by the heart, and must consequently have put the parietal and cardiac portions of the pericardium on the stretch at these points of union. Dr. Hope has mentioned a "jogging or tumbling" motion of the heart as characteristic of such partial adhesions; but I fear that much remains to be done ere we can speak with confidence of such marks of diagnosis. When the adhesion is universal, so that the heart is, as it were, firmly glued to all the adjacent parts, a certain degree of dragging inwards of the intercostal spaces and epigastric region may be perceived corresponding to the heart's action; at least in some few instances this has appeared to me to be the case; but in others nothing of the kind has been perceptible during life, although on examination after death the pericardium has been found universally adherent.

It seems to be in certain stages and conditions of the thickening caused by lymph becoming organised, that the sound called *creaking of leather*, by the French, occurs; it is one which I believe to be very rare, and to have been mixed up in description with the more common sound of friction.

Important as we must acknowledge these results of pericarditis to be, they are, nevertheless, greatly inferior in frequency, and in their consequent evils, to those changes to which the internal parts of the heart are subject from rheumatic inflammation. The valvular apparatus is that the organization of which undergoes the most remarkable modifications, and leads to the most destructive consequences, both as regards the functions of the heart itself, and the integrity of the general system. With respect to the external lining of the heart, I do not know that one portion of it is decidedly more liable to disease than another; but if I were to be guided by the results of my own experience, I should say that where the inflammation did not extend over the entire pericardium, the anterior portion was more frequently inflamed than the posterior. In regard to the lining membrane, there can be no doubt that the manifestations are greatly more frequent and more severe in the left

than in the right side of the heart. This has been attributed by some to the more stimulating influence of the arterial than venous blood, but it is by no means obvious in what manner this should be productive of disease. On either side the portion of the lining membrane chiefly implicated is that which forms the valves, and considered with reference to rheumatism, the rationale of these parts being most obnoxious to attack is evident enough when we consider the pre-eminently fibrous nature of their structure. In those who have had the heart affected in rheumatism, the most common circumstance is to find both sets of valves of the left side diseased; but if the morbid alteration be limited to one, then the auriculo-ventricular is more apt to shew change of structure than the aortic valves, whereas I think that in valvular disease from other causes the reverse is the case. By much the most common kind of change is more or less of thickening, and this, more especially in the mitral valve, is sometimes accompanied by unequivocal evidence of previous inflammation of the adhesive character, its different portions being frequently more or less united together, or glued to the adjacent surface of the heart. When the auriculo-ventricular valves on either side are opaque and thickening, the participation of the lining membrane beyond them in such change is for the most part much more marked in the auricle than in the ventricle; corresponding to what I have already stated with regard to the acute and earlier stage of the disease.

In the early stage of valvular inflammation, as I have already said, we sometimes find little rounded prominences like granulations; and at the more advanced period which we are now considering, excrescences present themselves, to which it is most probable that those granular deposits have given rise. They consist of formations more resembling venereal warts than anything to which they can easily be compared; and hence, probably, it was that Corvisart attributed to them a syphilitic origin. They are met with both on the arterial and auriculo-ventricular valves; but, I think, more frequently on the latter, and may be situated either on the surface or edge of the valve: they are generally pretty firm, roundish, or elongated, and sometimes attain the length of several lines, floating loose in the stream of blood.

Occasionally, the thickening of the valves, which seems to depend on lymph effused between their layers or upon the surface, becoming organized, proceeds to a greater extent than we have hitherto supposed, and passes into cartilaginous or even bony induration. By these changes

the natural elasticity of the valves is partially or wholly destroyed, and their functions impaired to a corresponding extent: sometimes they are obviously incapacitated from acting, and remain as rigid projections at the orifices they were designed to close, impeding the flow of blood through them to greater or less extent. In rare, but well-ascertained instances, they become brittle, and are more or less lacerated, folded back into unnatural positions, or presenting loose flaps, moveable by the current, but no longer useful as flood gates. In the aortic valves more especially, the portion towards the free edge is attenuated, or even perforated, and almost eribri-form. It is by no means proved, however, that the changes above mentioned are the results of rheumatism affecting the heart.

In the preceding description of the effects of rheumatism on the heart, I have spoken at large of *pericarditis* and *endocarditis*, but I have hitherto said nothing of *carditis* itself; and for this reason, that I have never seen any satisfactory instance of the muscular fibres of the heart being inflamed, in connexion with rheumatism. I have, indeed, seen the substance of the heart softer than natural when the membranes had been inflamed; but I have never seen any thing which could be called tumefaction, or interstitial effusion, or the formation of purulent matter in the heart after rheumatic fever, though I have seen some of these effects result from its inflammation unconnected with rheumatism.

The changes which the muscular parietes of the heart undergo, in connexion with rheumatism, seem to depend chiefly, if not exclusively, upon mechanical causes. In the great majority of cases in which the valves are diseased, the walls of the heart will also be found changed, and such change generally consists in augmentation of bulk. The most common deviation from the natural condition, in this respect, is an enlargement of the left side of the heart; but there is a remarkable difference in the statements made even by the best authorities, as to whether this increased size of the cavities is more frequently attended with augmented or diminished thickness of these parietes; in proof of which, it is sufficient to mention, that Baillie says it is most common to find them attenuated, and Laennec, on the contrary, that it is most common to find them hypertrophied. In rheumatism, I have no hesitation in stating hypertrophy with dilatation to be by much the most common change.

The most simple explanation of the changes alluded to in the muscular structure of the heart, and the one most generally admitted, is that which connects them with the state of the valves. Now

the most common condition of these parts, as a consequence of rheumatism, is a certain degree of thickening and stiffness, both in the aortic and mitral valves, especially the latter—a degree of thickening sufficient to present an impediment, but not an insurmountable obstacle, to the passage of the blood through the ostia of the heart. This state of matters requires more vigorous action, both from the ventricle and auricle, but of course more particularly from the former; and under this increased action the muscular fibres grow, apparently on the same principle as any other muscle is augmented by exercise. But if the obstacle be greater, such as the heart cannot easily, or perhaps cannot at all, sufficiently overcome, then it is not difficult to understand how the cavities, never wholly emptied, should yield to the slow operation of this continued dilating force, and at length become permanently enlarged, and their exhausted parietes attenuated. I beg, however, not to be understood as holding that these are the only causes of hypertrophy, on the one hand, or of dilatation, on the other.

Where any impediment to the flow of the blood is presented by the valvular apparatus, it is to be expected that the muscular fibres which have to overcome such impediment will be those to be enlarged; and, as a general rule, obstruction at the aortic orifice gives us hypertrophy, with dilatation of the left ventricle; while any obstruction at the mitral valve affects the auricle in a corresponding manner. But there is yet a different state of matters: we may have the aortic valves perfectly healthy, and yet the left ventricle appear unusually muscular. I think it probable that in such cases the muscularity will be found to consist almost entirely of increased bulk of the *columnæ carneæ*, and that the mitral valve will be found thickened, or otherwise changed, in such manner as to have required more than usual effort to make it act. Such, at least, has been the case in several instances of this nature which I have examined; and in one which occurred at St. George's Hospital during the present season, these phenomena were conspicuously displayed. The aortic valves were healthy; the mitral much indurated. The *musculi pectinati* of the left auricle were hypertrophied, owing, I presume, to the obstacle they had to overcome in forcing the blood through the auriculo-ventricular orifice. The *columnæ carneæ* of the left ventricle, too, were very large and strong, having to act upon a stiff and unyielding valve; but the general body of the ventricle (that portion the action of whose muscular fibres had to propel the blood through the healthy aperture of the aorta) was in no degree increased in

thickness, and, in fact, presented a remarkable contrast to the muscular bundles connected with the diseased valve.

In the above cases, where the heart has been permanently damaged by rheumatic inflammation, the diagnosis, to such an extent as serves all the purposes of useful practice, is made without difficulty.

The most conspicuous symptom, supposing the nervous energy of the patient to be unimpaired, is to be found in the action of the heart. This is more turbulent than natural; the contraction of the ventricles occupies a perceptibly increased period, is accompanied by a kind of *heaving*, and there is frequently a greater than natural projection of the parietes of the chest at each pulsation, which is very characteristic. If one end of the stethoscope be applied over the heart, and the fingers applied to the other extremity, the blow may not only be felt but seen, by the impetus communicated to the instrument. There is also a peculiar prolonged swell in the arterial pulsations. If there be great enlargement with thickening, then, in addition to the above sign, there is also dullness on percussion over a preternatural extent of surface, corresponding to the increased size of the viscus. If there be dilatation without hypertrophy, then the dullness exists over an increased space, but without the augmented impulse; and these very simple diagnostic marks are sufficiently clear to afford the means of forming an opinion which will be correct in the great majority of cases. It is remarkable how frequently patients labouring under hypertrophy and dilatation, with the increased force and duration of the heart's action to which they give rise, are yet unaware of any thing unusual. I have repeatedly questioned such persons, and have often found that, unless under excitement, they felt nothing unusual about the heart; and, as a general rule, they do not complain nearly so much as those labouring under merely nervous palpitations without any organic change.

Another almost constant attendant of an old rheumatic heart, is the *blowing sound*; and as this generally bears a relation to the extent of valvular disease and to the force of the heart's action, it is reasonable to conclude that it depends upon the passage of the blood through the ostia of that viscus, whether in the proper course of the fluid, or by regurgitation. But it is to be kept in mind, that it may also arise from causes apparently independent of any permanent disease, and not yet, I suspect, clearly made out, notwithstanding the ingenious and interesting researches of Dr. Williams, which, however, have greatly contributed to the elucidation of this subject.

This sound varies very much in its

intensity and tone, passing by insensible gradations from a soft blowing to a harsh whizzing or rasping sound, and in some—not very rare instances—even striking a distinct musical note, like that from one of the large strings of a harp. That these depend upon differences in the shapes of the apertures or tension of the parts through which the blood passes, is very probable. I have no hesitation in admitting that the intensity of the sounds bears a relation to the extent of valvular disease and force of the heart's action, as a general rule, but I must add, that every now and then we meet with cases in which we find little apparent disease, where there had been much of this anomalous sound, and *vice versa*—clearly proving to those who have no theory to support, that there is something more in the phenomena than is yet “dreamt of in your philosophy.”

ON HYDROPHOBIA.

To the Editor of the Medical Gazette.

SIR,
I SHALL make no apology for sending you an account, somewhat minutely detailed, of a case of hydrophobia which occurred a short time ago at the Middlesex Hospital, because such a fearful interest is attached to this disorder that there is scarcely any circumstance connected with it the relation of which can appear superfluous. Indeed, the whole nervous system becomes affected by it in such an extraordinary and inexplicable manner, that its moral as well as its physical symptoms, and the state of the mind as well as that of the body under it, are inquired into by most persons with almost equal interest.

I am further induced to offer the following case for insertion in your journal, because although in a practical point of view it may be thought to be less interesting than some others, yet in one respect it appears to me to be instructive and likely to be useful.

To those who have witnessed several cases of hydrophobia, or who have paid much attention to the subject, it is well known that many of the symptoms which attend it may vary greatly in different instances. But it ought to be known, more generally than it is, that the peculiar and essential symptoms of the disorder may in some cases be very slightly developed, so as even to be overlooked through the greater prominence of other symptoms which are bet-

ter known as characteristic of a different pathological state.

It may be useful, therefore, to publish a case like the following, which shows that in its general aspect hydrophobia may closely resemble phrenitis or extreme cerebral irritation, and yet may run its usual course, and terminate as quickly and in the same manner, as when its distinctive symptoms have been far more strikingly displayed.

Your obedient servant,

FRANCIS HAWKINS, M.D.

Physician to the Middlesex Hospital.

Curzon Street, May Fair,
October 19th, 1837.

William Hayes, aged 28, a tailor, applied for admission into the Middlesex Hospital, on Thursday morning, October 5th, 1837, complaining that he was unable to drink; that the noise of water appeared to choke him, and to stop his breathing; and that the same effect was produced by a current of air, if the hand were passed quickly before his face, his eyes being shut. He was evidently in an anxious and excited state. Pulse about 80, quick and slightly irregular. Tongue white, and covered with a creamy fur. His bowels had been open twice this morning.

The account which he gave of himself was, that about two months since he had seen a dog pelted by some boys in a canal, and the animal, whilst he was endeavouring to help him out of the water, had fastened upon his hand and wounded three of his fingers: the nail of one was torn across, and had afterwards come off. After he had beaten the dog, as an ungrateful cur, the boys drove him away, and of his subsequent fate he had no knowledge. He persuaded himself that the dog could not be mad because he was in the water and barked at him in a natural way, and he had adopted therefore no measures of precaution. The wounds on his hand had healed readily enough, the scars being still visible on two of his fingers. He positively denied having been anxious about the bite; yet his remarks indicated rather more than a common acquaintance with the ordinary symptoms of hydrophobia. He declared that he had been temperate, and that his health had been good; but he added that he had always been nervous, which he explained by saying that he had been subject to tremors. He went on to say that he had suffered no inconvenience from his accident until two days since,

when pain occurred in the hand which was bitten: he described it as a stinging pain, extending up the arm: indeed, there was uneasiness, he said, in the whole of the right side. On the night before his admission he had gone to bed feverish, and thought he had taken cold, his throat being slightly sore. He was unable to sleep, and got up to drink, but water appeared to choke him. He went out for medicine, and finding no shop open, applied at the hospital, where some pills were given to him, which he took and returned home to bed, but was still sleepless, and he had been able to take little or no breakfast; whereupon he came again to the hospital and requested to be admitted.

His own history of his case was confirmed on the following day by his father, to whom he had written from the hospital nearly in the following terms:—"I am afraid I have got the disorder which I told you of on Sunday,—the dog,—I am now at the Middlesex Hospital," &c.

It appeared also from the account of one of his fellow-workmen, that on the Monday before his admission he first complained of an uneasy sensation in his hand, and on the Wednesday he was so restless that he declared he could settle to no employment, and was constantly speaking of his hand. On the same night he was feverish, as already related, and was received into the hospital on the following morning. His condition on that day (Thursday, October 5th, 12½ p.m.) has already been described. He was more like a person under the dread and expectation of evil, and who was striving to make up his mind to meet it, than like one under actual suffering. There were no involuntary spasms apparent at first sight, and yet, on close observation, a slight catch in his breathing,—as if an inspiration were suddenly arrested, and followed by a short pause,—might occasionally be detected. His conversation and answers, although rapid, were clear and collected. But the tone of his spirits was very unequal. At one moment he appeared to have convinced himself that his fears were groundless, and endeavoured to laugh them off; at another he gave way to despondency, and anticipated speedy death. He entreated that he might have no experiments tried upon him, unless it were to give him poison, fearing, he said, that for the sake of science his feelings would be but

little regarded. At a subsequent period, when urged to allow his hair to be taken off, he refused with a forced gaiety, alleging as a reason that he should not like to look ill in his coffin. As he refused at this time to attempt to swallow liquids none were offered to him; but, at his own request, some rice pudding was brought, of which he swallowed a small quantity, although with difficulty, and he complained that it was too dry. When moistened with milk it seemed to be more palatable to him, yet after taking a few spoonfuls he soon gave it up again, although he could willingly, he said, have eaten more. He requested that he might not be placed in a separate ward, as the company of the other patients was agreeable to him, and he earnestly begged for some entertaining books of light reading to amuse himself with, without too much taxing his attention. I was desirous if possible to have the cicatrix of the wound extirpated, a measure which might still be advisable if hydrophobia were really threatened; or if the complaint were imaginary, which might contribute to allay his fears and compose his mind. But this measure appeared, on consultation with my colleagues, to be wholly impracticable, because three fingers had been wounded, and one of them probably over the whole of its summit, the nail having been torn across. And at this time certainly the symptoms did not appear to justify amputation of a hand, and that the right hand; for what symptoms were there which might not be explained on the supposition of a state of excitement and irritation, produced, or at all events increased, by the fear of hydrophobia? His present condition being hysterical rather than feverish, some pills were ordered for him containing sulphate of iron in combination with the Pil. Sap. c. Opio and Extr. Coloc. Comp.

At 5 P.M. he complained that he was worse. He was flushed in the face and agitated, the pulse being also somewhat quicker than before: he had taken two of his pills, and passed another evacuation, which was of good colour, but small and worm-like, as if the bowel had been contracted. He professed the same horror of liquids, and declared that he had as much difficulty in swallowing as before. Yet still the motion of water did not appear to excite any actual spasm; he merely remonstrated with those who

once or twice splashed it about, on the cruelty of doing that which was painful to him for their own amusement. Having been asked to suffer his forehead to be moistened, he declared that he would do it himself: accordingly he dropped his hand into a basin placed rather behind him, (because he should do it, he thought, more easily if he did not see the water) and drew his hand hastily across his forehead. Still there was no visible spasm, and he seemed pleased, and boasted of what he had just done. He also swallowed a few grapes, though not with much ease or dispatch; it was observed, however, that he took them faster and more easily when not aware that he was watched.

He was ordered to be kept quiet, and to take ten grains of calomel and two of opium immediately, and that this should be followed up by half a drachm of musk, alternately with a grain of acetate of morphia every second hour.

At 10 P.M. he was cooler and less feverish; pulse quieter and less frequent. He said, with some degree of exultation, that he had slept a little at intervals. He had taken his pills and swallowed his musk with tolerable ease. He thought he should like to have some grapes placed by him, but was better pleased that these, and whatever else was brought to him, should be covered up at first, than that they should be at once exhibited to him.

In this state he was left, endeavouring to compose himself to sleep, and he continued quiet until nearly 12 o'clock, when he was unfortunately disturbed by the entrance of some of the medical officers into the ward. He resented this disturbance with great irritation, and became quite furious when asked if he would drink some water. He began to rave and became ungovernable, and from that time in fact until the moment of his death, was constantly in a state of frantic delirium. Early in the morning, upon seeing in the ward one of those gentlemen who had disturbed him at night, he darted up, and leaped over one bed after another with surprising strength, and it was with difficulty that he was at length secured and kept in any degree of order. He could only be persuaded to enter another ward by the assurance that he should walk into it himself, without constraint; or any force being offered to him. He began now to be excessively irritated if any one spoke to him,

or indeed if a single word was spoken in his presence; commanding silence in the most impassioned manner. He appeared to be less offended if he was approached firmly and openly, than if any one advanced towards him softly and cautiously, by which his suspicions were manifestly awakened. In the separate ward he begged to be attended by one of the patients for whom he had conceived a liking, and there was but one of the nurses by whom he could bear to be approached, the presence of any of the rest appearing to irritate him beyond measure; and he declared that no one but those just mentioned should enter the room, or, if they did, he could not be answerable for any violence which he might commit.

Friday, Oct. 6th.—At 12 P.M. I found him greatly altered since the former evening. He had just left his bed, and, on my entrance, he sprang to the middle of the room, where he stood, shrunk and ghastly; his lips and countenance had become somewhat livid, and, in the midst of his ravings, a catch in his breathing might be observed more frequently than before. He suffered himself, however, to be persuaded to lie down again, and promised to allow me to feel his pulse, if I would stand on one side rather than the other. He complained that if he attempted to swallow, his water passed from him involuntarily, a circumstance which has been noticed in this disorder before. He endeavoured with all his might to impose silence on all around him; whoever spoke appeared to speak daggers to him; and he declared that if many persons were to enter the room, and to stare upon him, he could not command himself, but thought he should leap over the banisters. At one moment, having been accidentally left alone, he contrived to lock himself into his room; and it now became necessary to break open the door, and to put a strait waistcoat upon him. When this had been done, instead of struggling with greater violence, he became more composed, and was less irritated by the presence of numerous persons, and by their conversation. He made at that time an earnest request that one of the students (whom he singled out and requested to come forward, because his countenance, he said, was amiable and engaging), might be allowed to attend him. His complexion, since he had been excited by the en-

trance of many visitors, and by his struggles to escape from them, had acquired a better hue, and his breathing was carried on with less interruption, so that he was thought by many, who now beheld him for the first time, to be labouring under a fit of insanity, or the commencement of phrenitis.

As he was still at liberty to get out of bed, it appeared right to confine his feet; but he no sooner caught sight of the leather brought for this purpose, than he sprang out of bed with extreme violence, and it was necessary to bring him back again by main force. During his recent struggles he vomited a small quantity of yellow matter, and wished he could be sick again. I directed, therefore, two grains of the potassio-tartrate of antimony to be given to him, on some bread sopped in milk, which he had asked for. It was doubtful whether he swallowed it; but soon after he allowed me to put two grains more upon his tongue, which presently excited vomiting, and he expressed himself as being greatly relieved. This remedy had the effect of reducing the force and frequency of the pulse; and it was directed that a similar dose should be given to him in an hour's time; after which a grain of calomel, with a grain of muriate of morphia, were to be given every hour.

Between five and six in the afternoon his state was considerably changed. He had become more and more delirious, yet his memory, perception, and reason, appeared at times to be perfectly correct. His temper, however, had become much exasperated, and he expressed his wrath by sometimes spitting violently about him. He had vomited two or three times, and was besides constantly pouring out a large quantity of saliva. The force of the temporal arteries was greatly reduced. Attempts had been made to administer the morphine, but without much success. About 11 o'clock, his bed having become very wet with the excessive flow of saliva, it was thought proper to shift him into another, for which purpose, as soon as his feet had been unloosed, he gathered them up, and sat up in bed; a slight retching then came on, upon which he fell back, and almost instantly expired, appearing to sink exhausted, and not as if suffocated by any sudden or permanent spasm.

Examination.—On the following day,

at 2, P.M., the body was examined. It was, at first sight, evident that the blood had remained in a fluid state, for although 15 hours only had elapsed since the time of death, the back had become extremely livid from the subsidence of the blood. The spinal column was opened, and the theca vertebralis having been accidentally wounded over the cervical vertebrae, some clear fluid oozed out, though not more, probably, than the natural quantity. The whole of the spinal marrow was exposed, but no appearance of disease whatever could be discovered in it. Neither was there anything remarkable in the appearance of the brain and its membranes, except a slight partial thickening of the arachnoid, and a very little fluid effused underneath it. In the ventricles there was but a small quantity of fluid, and the choroid plexus was in a natural state. The only remarkable circumstance observed in any part of the nervous structures was a slight but decided vascularity of the pons varolii and medulla oblongata; the surface of these parts was injected with bright red vessels; whereas in no other part of the brain and its membranes had there been any thing like arterial injection observed. When the head was raised at this period of the examination, the fluidity of the blood was rendered further manifest by the appearance of the face, which had become perfectly purple from having been suspended during only a short time over the table.

The membrane lining the fauces and covering root of the tongue was rendered unusually rough and irregular by an enlarged state of the mucous glands.

The mucous membrane of the œsophagus was in some parts abraded, and in other parts its superficial interior lining had the appearance of a fine false membrane. In the stomach the mucous coat appeared to be softened, but there was no breach of its continuity. In some parts and patches of it there was the common appearance of vessels greatly congested. The small intestines, throughout their whole extent, appeared to be perfectly healthy.

The epiglottis, and other parts about the glottis, were dark and highly injected. The lining membrane of the larynx and trachea was also slightly congested; and the lungs contained a good deal of serum. There was a

moderate quantity of fluid in the pericardium. The subclavian veins were empty. In the cavities of the heart there were some dark clots of blood, coagulated, but not firmly.

The principal cicatrix, which was on the middle finger of the right hand, was also carefully examined, but there was no vascularity round it, nor anything whatsoever unusual in its appearance.

REMARKS. — "Can this be hydrophobia?" was the question of numbers, on beholding the poor patient whose case has now been related, even until a very short time before his death. For, in truth, up to a late period of the disorder, there were no decided symptoms manifested which might not be accounted for by an hysterical state of the system in the first instance, and afterwards by a state of delirium, the effect of extreme cerebral irritation, or of commencing inflammation. On the day of the patient's death, when all doubts respecting the nature of his case were in the opinion of some persons removed, yet others, who then saw him for the first time, were positive in their judgment that his disorder could not be hydrophobia; and these were persons not only of general experience, but who had seen hydrophobia before. Nay, it actually happened that some of those who at the commencement of the case were most inclined to suspect it to be one of hydrophobia, were dissuaded from this belief by its further progress, and became strongly of an opposite opinion. However, the manner of the patient's death served to convince some of the most sceptical as to the real cause of it, because it occurred so speedily, and the scene was closed without the typhoid or comatose symptoms which they expected to supervene. The examination, finally, may be said to have proved, both negatively and positively, that the death was truly caused by hydrophobia; negatively, from the absence of every sign of cerebral inflammation or congestion; and positively, from those very alterations of structure, however slight, which are usually found in cases of hydrophobia, and in dogs affected with rabies. Not but that there were some persons to whom all these proofs appeared insufficient, and who professed themselves to be still undecided as to the nature of the case.

For my own part the doubts which I

entertained when first called to take charge of the case were gradually but progressively removed, and became less and less every time I visited him.

Early on the morning of Thursday my opinion was perfectly formed. For I observed that although maniacal symptoms had supervened, yet that when the patient was in his least excited state, the essential symptoms of hydrophobia, viz., the extreme sensitiveness of the cutaneous nerves, and the spasmodic affection of the muscles of respiration and deglutition, were manifested, though still slightly, yet more clearly than before. Nevertheless, while the case was so generally considered doubtful, I deemed it best that it should be treated in a such a manner as seemed likely to afford the patient the best chance of recovery, if his case should *not* be hydrophobia. But late on the Wednesday night so little doubt appeared to me to remain, that I was desirous of endeavouring on the following morning to diminish the sensitiveness of the cutaneous nerves by anointing the patient freely with an ointment containing delphine, while at the same time acetate of morphia or some other sedative should be applied to a blistered surface over the cervical vertebrae. But the frenzy in which I found him rendered this and most other remedial measures almost if not entirely impracticable.

The course of the disorder, whilst it was under our observation, appeared to be divided naturally into three distinct stages. In the first of these the symptoms of hydrophobia were already manifested, although in the mildest manner; then came the stage of maniacal excitement; and lastly that of the exhaustion which immediately preceded death. But previous to these, there was the state of restlessness which intervened between the recurrence of pain in the wound, and the first difficulty of deglutition. And prior also to that was the longer interval between the infliction of the wound, and the secondary pain in the cicatrix. This interval is sometimes termed the period of incubation. Thus we have five stages of the disorder which, I suppose, are more or less distinctly marked in the course of every case of hydrophobia.

On contemplating the foregoing case it is consolatory to find that neither excision nor cauterization of the bitten part had ever been resorted to. For it would

tend to lessen our common security against the worst sufferings of humanity, if those measures of precaution had been adopted early and yet had proved ineffectual.

The period of incubation in this case did not exceed in its duration the usual average; which is also satisfactory, because it would increase the anxiety of those who have the misfortune of living under the apprehension of the disorder, if this period should be often greatly prolonged. There are, however, good reasons for believing that during this period of incubation the poison which has been received may still be thrown off, without producing its effects upon the system. This is credible, not merely because so many escape who have been bitten by rabid animals, but further because the period of incubation appears to be somewhat more uncertain for this than for most other poisons; and because before its effects upon the system are manifested, it has appeared in many cases that the health of the recipient has been impaired by some cause or other, but for which it is possible that his constitution might have resisted its influence altogether. Of those, again, who have been the subjects of hydrophobia, many appear to have been of a nervous temperament, as was the case in the present instance; and if it be true that one constitution is more susceptible of this disorder than another, so may, possibly, the same constitution be at different times.

As soon as the second period of the disorder has commenced, which may generally be known by the recurrence of pain in the wound, extirpation of the cicatrix seems still to be desirable; for although there have been cases in which even amputation has been practised early after the commencement of hydrophobia, without appearing to delay its fatal course even for an instant, yet who shall determine the precise point at which the system has become so much affected as to render this measure useless?

It would seem that the extension of the influence of the poison from the inoculated part to the system at large—that process which appears to take place at the end of the period of incubation—is not necessarily an instantaneous nor even a rapid one; for in this case the pain in the hand continued for several days before the constitutional symptoms

appeared. The cicatrix, however, on a careful examination, presented no appearance of having undergone any recent alteration.

In the third stage of the disorder, when the system has become affected, and the symptoms of hydrophobia are unequivocally manifested, there is no remedy, however severe or violent, of which it would not be fair to make trial, provided that it offered the smallest probability of subduing the disease. The peculiarity, however, of the present case was, that the symptoms were by so many thought to be equivocal, even to the last, that extreme measures were unjustifiable until they had become impracticable, or at least utterly hopeless. In such a case, as long as there was a doubt, who would not have given the patient the present benefit of it?

The frantic state, the high degree of delirium; to which the patient was unfortunately excited, might naturally appear to have accelerated the approach of the ultimate stage of exhaustion; yet I believe that cases of hydrophobia have run their fatal course as rapidly as this, in which there has been far less excitement, and only a little degree of delirium. Again, it may be thought that the remedy which succeeded in this case in reducing the excited state, viz. the potassio-tartrate of antimony, may also have had some share in producing the subsequent state of collapse. However that may be, yet certainly for a time, even by the patient's own statement, this remedy was productive of considerable relief.

The difficulty experienced in this and in other cases of hydrophobia, of administering medicines, and, indeed, of making use of remedies of any kind, drives us to the conclusion that if this disorder, when it has once declared itself, is ever to be combated with effect, it must be by means of injections into the veins. And although the chief manifestations of the disorder are confined apparently to the functions of the nervous system, yet, after all, the cause of them may reside in the blood. This opinion, which I believe is entertained by many persons, is rendered the more probable, if it be true, as we are informed, that rabies may be produced in animals by inoculating them with the saliva of a patient labouring under hydrophobia.

UTERINE DISEASES.

To the Editor of the Medical Gazette.

SIR,

I TAKE the liberty of sending you the three following cases of uterine affection, which belong to a class of complaints as important, perhaps, as any a medical man is called upon to treat, as they but too frequently render the lives of those afflicted with them completely wretched, by the loss of health, and the mind being worn down by bodily pain and uneasiness, and by the apprehension of an incurable disease being established. Should you deem them worth a place in your GAZETTE, they are quite at your service.

The first case occurred when I was clinical clerk under the late Dr. Cholmeley, at Guy's Hospital. The history was as follows:—Mrs. S., ætat. 23, married four years; has had no children; of sallow complexion; suffered before marriage much pain at back part of the head and lower part of abdomen, with great debility. After marriage was much worse, and there was a tumor at the upper part of vagina. Consulted a physician without benefit. The catamenia irregular, and of a green colour, and caused great suffering; micturition difficult and painful; the abdomen tender on pressure; skin cool; tongue coated with a white fur; bowels generally confined. Dr. Ashwell was requested to make an examination; and he reported that the cervix uteri was indurated and inflamed, the slightest touch giving great pain. On her entry a dose of calomel and castor oil were given, to clear out the bowels; after which, Dr. C. ordered the following:—

Pil. Hyd. gr. iv.; Pulv. Ipecac. gr. i.;
Ext. Hyoscy. gr. iv. om. noct. M.;
et Ammon. Sub. gr. v.; Mist. Camph.
3x. bis die. M.

This medicine acted like a charm, and in three weeks she left the hospital quite well.

The next case is a Mrs. F., who two years since consulted me for what she feared was an incurable disease of the womb, as she had been under several medical men, who had done her no good. Her symptoms were very similar

to the first, viz. a bearing-down sensation in the vagina, tenderness of abdomen, the unhealthy look of countenance, and a coated tongue. In this case there was pain in the left side, about the situation of the ovarium, and her illness had come on since her last confinement, when she suffered much from the placenta adhering, and which she said was *torn away*. I gave her the same medicine as was given in the former case, and with the happiest effects. She was cured in one month, and has been well ever since.

The third case is a Mrs. G., æt. 23, whom I attended with her first child two months since. Her labour was natural, but the child lived only twelve hours. I was requested to see her three weeks afterwards, and found her labouring under great pain and tenderness in the hypogastric region, and such a bearing-down feeling in the vagina, that she could scarcely sit. Her pulse quick and irritable, her eye sunken, and her countenance totally altered since I last saw her. As this case was of a more acute character, she was confined to her bed. I ordered six leeches to the pubes, and hot fomentations, in addition to the blue pill and extract of henbane. In one fortnight she was quite well.

This treatment, sir, so efficacious and so easy of practice, is highly deserving of a trial, as such cases but too often resist the ordinary means made use of, and it is to be feared have baffled many men, and reflected not a little discredit upon them in the end, especially after a great deal of local treatment, such as warm injections, ointments, cupping on the loins, and sometimes mechanical supports,—a practice by no means uncommon at the present day.

There is another disease over which the same remedies exert a wonderful influence, and that is hysteria. I have a patient now, who has been completely out of health for months, and has suffered dreadfully from hysterical fits, and who appeared to all her friends to be sinking. I at the first gave her the ordinary remedies—steel, bark, ammonia, and henbane—with but little effect. At last I was induced to give her blue pill, and extract of henbane; and she has not had a fit since the second dose, and is now almost well. I have been surprised that this remedy is so well borne by such delicate females, for

weeks together (for they are generally delicate women), who, if they were to take it but for a few days when in health, would probably be completely salivated.

Hoping I have not trespassed too long upon you, I am, sir,

Your obedient servant,
EDWIN ELLIS.

Lower Tulse Hill, Brixton,
Nov. 1, 1837.

CHOLERA INSURANCE SOCIETIES.

To the Editor of the Medical Gazette.

SIR,

WHEN the cholera was in the country, in 1832, I called my neighbours together and explained to them the best preventive measures that could be adopted, and the propriety of assisting each other in a time of such a general calamity.

We succeeded in procuring seventy-five families to pay from 6d. to 2s. 6d. per week, and in four months we had in hand fifty pounds. It was remarkable that, in the adjoining villages and neighbourhood, there were seven cases, and only one in town; but *not one case existed after the formation of our Society*, which I attribute to the moral confidence they all felt—the *parent of a physical energy sufficient to resist the contagion*.

The money was returned to the assurers when the cholera left the country, with interest, as it had been deposited in a Savings' Bank.

I have sent you a copy of our rules, that if any of your readers may choose to recommend such a system at the present time, they may see them. They will, I beg to observe, be entirely unsuccessful in any appeal they wish to make to the working people, if they allow the servants of selfishness to exercise, use, or govern in any degree, a work of good will. The guardians are useful as an amputating knife in cutting off the corrupt parts of the body politic, but there is as much healing virtue in the one as the other, and they mar, ruin, or neutralize, every attempt to improve the character and conduct of the working people. They have

done as much mischief in arresting the progress of good will amongst the people, as they have of profit to the absentees and usurers, for whose particular benefit they were created.

I remain, sir,
Your humble servant,
H. L. SMITH.

Southam, Warwickshire,
Nov. 1, 1837.

ANALYSES AND NOTICES OF BOOKS.

“L'Auteur se tue à allonger ce que le lecteur se tue à abrégé.”—D'ALEMBERT.

A Lecture on the Functions of the Lymphatic System. By ROBERT JAMES GRAVES, M.D. M.R.I.A., &c. Second Edition.

THIS lecture, which was originally published in 1827, and again in 1834, did not fall under our observation till very recently; a circumstance which we regret, as it contains some very interesting original opinions, not only on the subject of which it specifically treats, but on various others incidentally alluded to.

We find, for example, that the view taken of the functions of the lymphatic system, by Dr. Graves, ten or twelve years ago, is precisely analogous to that put forward by Müller, in his valuable work on *Physiology*, the translation of which has just appeared. Dr. Graves observes,

“This great and marked distinction may therefore be made between vertebrated and invertebrated animals, that the vascular system of the former includes two parts, one containing red blood, circulating in arteries and veins, and the other a transparent colourless fluid, conveyed towards the heart by the lymphatics; while the vascular system of the latter is single, being confined to vessels circulating a colourless fluid.

“The solids of red-blooded, compared with those of white-blooded animals, present a difference corresponding to that of the fluids: for as the vascular system of the superior classes contains both red and colourless fluids, so we find both red and white structures or tissues in the solid portions of their bodies. In the mammalia, the muscles both of voluntary and involuntary motion, which constitute so great a part of the bulk of the body, belong to the red structures; among the white may be

enumerated the cellular, serous, synovial, and fibrous membranes, ligaments, cartilages, the animal basis of bone, &c. In the invertebrated animals, on the contrary, the whole solid system belongs to the class of colourless tissues, or at least includes nothing similar to the red solids of vertebrated animals.”

And again:—

“The following reasons seem to me conclusive in proving, not only that the white tissues have a proper circulation of white blood, but also that they are capable of undergoing a two-fold species of inflammation:—

“1st. When we irritate the white part, it soon becomes red, from the appearance of minute vessels carrying red blood. These vessels appear often so suddenly, that we are forced to admit their previous existence in the part; and the existence of vessels being thus proved, it follows that the fluid circulating in them must have been white, for otherwise they would have been visible. This simple experiment on the living animal enables us to prove the existence of minute vessels in white parts, conveying in health a white fluid, and directly continuous with the arteries, as is shown by the immediate entrance of arterial blood into these vessels, when the part is stimulated*.

2d. We find that the pseudo-membranes so often formed between the pleura pulmonalis and pleura costalis, are capable of becoming vascular, red, and inflamed, and are sometimes found covered with an exudation of coagulable lymph.

“It is quite evident that in such membranes, quite unconnected as they are with any red part, the vascularity observed during inflammation must belong to themselves. When the inflammation subsides, the vessels into which red blood had found its way again convey a white fluid, and consequently become invisible.

“3d. Where a serous membrane covers another white structure, in the manner that the synovial membranes are attached to the cartilages and capsular ligaments of joints—when we find, as is often the case, that the internal surface of the joint becomes highly red and inflamed—we must attribute this morbid vascularity to one or other of

* See experiments by Hunter, in his *Treatise on Inflammation*. Also Thompson on *Inflammation*.

the white structures, for in this case no other structure enters into the composition of the part.

"4th. The synovial membranes furnish us also with conclusive evidence against the opinion that the *serous exhalation* is not derived from, but is merely an exudation through the serous membrane, and that this exhalation is derived from some part furnished with red vessels, for the synovial membranes in health are covered by a copious exhalation, serving to lubricate the articular cavities; and which secretion cannot have been produced except by a white structure, as none other enters into the composition of the joint.

"5th. The exhalation of cellular and serous membranes is closely allied, in chemical composition, to the serum of the blood, and is therefore probably derived directly from it, as it circulates in the white parts, and not indirectly from red blood circulating in red parts.

"6th. That a circulation is carried on in the white parts, has been proved by the experiments of Mayer, who detected prussiate of potash in the tendons, ligaments, &c. in a few hours after he had injected a solution of this salt into the lungs of animals.

"All these reasons seem conclusive in proving that the white tissues are during health provided with vessels continuous with the arteries, but conveying only the serous portions of the blood; and that in disease these vessels admit red blood, and thus these parts become the seat of red inflammation.

"In the higher order of animals, as the mammalia, all the blood is sent from the heart through the arteries, to be distributed to all parts of the body. This blood consists of two portions—a red fibrinous portion, and a serous albuminous portion. The two portions together circulate through the arteries, and are returned by the veins, passing through parts of a red colour and fibrinous composition; while a part of the serum enters into the minute arterial branches, which, supplying the white parts, do not admit the red or fibrinous portion; and thus a serous or white blood is distributed to the white structures, whose albuminous secretions and albuminous basis agree so exactly with the composition of the fluid thus destined to support their nutrition and carry on their functions.

"As a serous or white blood is con-

veyed from the heart to the white parts, it is evident that some means must be provided for its return towards the heart; and for this purpose we find these parts plentifully supplied with lymphatics, vessels which discharge the office of conveying back the white blood from the white structures, and which may therefore be called the *veins of the white parts*."

If these extracts be compared with pages 275, &c. of Baly's translation of Müller, the similarity of the doctrine, with regard to the lymphatics, will become obvious, and the same remark with respect to Breschet has already been made in the review of his work, in the last number of our "British and Foreign" contemporary.

While we are on the subject of Dr. Graves's claims to originality, which we are desirous to vindicate, we may allude to the fact of Carus having adopted very nearly the same expressions as the physiologist of Dublin. Thus—

"It deserves particular attention," says Carus*, "that we meet in the vertebrated animals a repetition of a vascular system, destined to carry a simple colourless blood (the lymphatic system), to which is added another system of vessels of a still higher rank, and destined for the circulation of red blood: this repetition is precisely analogous to the repetition of the ganglionic system of nerves, in the higher classes of animals, after the cerebro-spinal nerves have been superadded to that system."

While in the lecture before us, published, be it remembered, a year before the work of Carus—

"In the higher classes of animals there are not only two circulating systems, one of red blood and another of white blood, but also a twofold system of nerves, the cerebral and the ganglionic; the latter of which, in invertebrated animals, seems to perform all the nervous functions necessary to their state of existence, while these animals are also remarkable for possessing only a simple vascular system."

To the lecture are appended some "additional notes;" and in these we observe two circumstances worthy of notice. The first is the anticipation of the discovery of lymphatics in the brain,

* Grundsätze der vergleichenden Anatomie und Physiologie, &c. Bänd. iii. p. 2. Dresden.

very distinctly stated in the following passage :—

“ In invertebrated animals, the vessels carrying white blood back to the heart, *i. e.* the lymphatics, are simple, and cannot, *when minute*, be in any way distinguished from the minute ramifications of the veins in higher animals. The similarity of structure between minute vessels containing white blood in a state of health, and minute veins, is well exemplified (as argued in the text) in the conjunctiva, where white veins suddenly become red veins, when inflammation supervenes. In the same way it is extremely probable, that the lymphatics of the brain, answering to the white veins of the lower orders of animals, and the short simple lymphatics observed in other parts of the structure, escape notice only because they empty themselves very speedily into the venous system of red blood.”

The second point of interest to which we refer, as contained in these notes, is the view given of the functions of the placenta :—

“ It is probable, therefore, that the maternal vessels and those of the fœtus may carry on an active interchange of principles, although no direct communication by opening or mouths exists between them. The vessels of the mother may carry principles necessary for the nutrition and respiration of the fœtus, and may freely impart them to its vascular system; while, on the other hand, every thing which requires to be excreted from the fœtal system, may, in a like manner, as easily find its way into the maternal veins, and so be gotten rid of. I cannot help thinking that this view of the placenta, which, by comparing the relation between the ultimate ramifications of the maternal and fœtal vessels with those of the secreting vessels and ducts in glands, points out not merely the analogy, but almost the identity of this relation, deserves the attentive consideration of physiologists. It is to Müller that we are indebted for understanding the analogy which the lungs bear in their structure to glands; and his researches first suggested to me the idea, that the placenta may be considered as a temporary gland, destined for the nutrition and respiration of the fœtus.”

Now in making the above extracts, we do not mean to charge Carus, Müller, or Breschet, with plagiarism; but

when we find views and opinions attracting notice, as coming from the pens of foreigners, which have previously been announced by a distinguished countryman of our own, we feel it to be but fair to state the facts simply as they are, that the credit which always attaches to originality may be bestowed where it is justly due.

An Essay on Pyrexia, or Symptomatic Fever, as illustrative of the nature of Fever in general. By HENRY CLUTTERBUCK, M.D. 8vo. 136 pp. Highley.

So long since as 1804, Dr. Clutterbuck endeavoured to prove that all fevers are symptomatic, and depend on inflammation of some part of the body, and, therefore, that fever *per se* really does not exist; the disease usually known by the name of idiopathic fever being an inflammatory disease, the seat of which is the brain; and though it would appear to occupy the entire organ, yet it does so unequally and differently at different times, and in different individuals.

The present work is devoted to that constitutional disorder termed Pyrexia, or Symptomatic Fever, and which the author believes to be invariably produced by, and, therefore, always dependent on, inflammation. Its characteristic symptoms are, heat of skin, frequency of pulse, thirst, and foulness of tongue. Cullen's definition is objected to by Dr. Clutterbuck, on the ground that the “*viribus artuum imminutis*” of the celebrated Edinburgh professor does not apply to pyrexia, but is peculiarly characteristic of idiopathic fever. In other words, our author regards the prostration of muscular strength, alluded to by Cullen, as being the consequence of inflammation of the brain, and, therefore, as not being observed in inflammation of other organs.

After examining the state of the blood in, and the varieties of, pyrexia, the Doctor examines the causes of this affection. All kinds of irritation, he observes, are not capable of producing it. It seems, indeed, to be that kind which inflammation excites, that is the chief, if not the sole cause of it. It is, indeed, true that a number of other circumstances, not very obviously connected with inflammation, give rise, at times, to febrile action. Mercury and

antimony, for example, produce this affection, and their influence is thus explained: "These substances appear to be taken up by absorption, and, being carried into the bloodvessels, probably act on these, so as to excite them into increased action, if not to produce actual inflammation on their surfaces." This is somewhat analogous to Messrs. Morgan and Addison's view of the operation of poisonous agents generally, on the body. The states of menstruation and pregnancy, which sometimes occasion febrile symptoms, depend on some condition of the uterus very closely allied to, if not identical with, inflammation. Dr. Clutterbuck, however, admits that some exciting causes (as violent exercise, the hot bath, the use of alcohol, &c.) occasion excitement of the vascular system, and raise the temperature of the body, yet without exciting inflammation. "It may be questioned, however, whether the state thus induced is really of the nature of pyrexia; for it is not attended by that general disturbance of functions, and the coated tongue, that are observed in genuine symptomatic fever: they appear rather to be cases of simple vascular excitement, which soon subsides again; with no other result than a degree of weakness or exhaustion, that naturally follows over excitement of any kind."

In speaking of the nature of pyrexia, Dr. Clutterbuck says, "Upon the whole, with regard to the intrinsic nature and mode of production of *pyrexia*, or symptomatic fever, it may, I think, be concluded that *inflammation*, considered as the ordinary remote cause of this affection, acts as a source of irritation to the brain, disturbing its vascular action, and thereby disordering its functions, but differently in the different stages. The change thus taking place in the condition of the brain, influences subsequently, and in a corresponding manner, the whole *sanguiferous system*" by sympathy.

The three last sections of the work are devoted to the consequences and treatment of symptomatic fever.

The foregoing extracts and remarks will put our readers in possession of the principal topics discussed by Dr. Clutterbuck. We have preferred giving an analytical rather than a critical notice of his work, on account of the necessarily limited space we could devote to it.

There are several opinions expressed in this "Essay" which some persons may not be disposed to admit: but being the opinions of a physician of great experience, and of one endowed with a highly cultivated and logical mind, they require examination. Moreover, the works on Symptomatic fever are very small in number compared with those on Idiopathic fever, and hence, therefore, Dr. Clutterbuck's book is the more valuable, and will be perused with so much the more interest, even by those who may not assent to all his opinions. In conclusion, we beg to thank the Doctor for his "Essay," and strongly recommend it to our readers.

—
Die Pest des Orients. Von Dr. C. J. LORINSER. 8vo. pp. 461. Berlin, 1837. Schloss.

THIS is a work of much research. The first part is occupied with an historical account of the plague; the second with its pathology; and the third with the preventive measures adopted in different countries for its counteraction. In an appendix is subjoined a narrative of the last plague which ravaged Silesia between the years 1708—1712.

—
Die Sinnestäuschungen. Von Dr. F. W. HAGEN. 8vo. pp. 348. Leipzig, 1837. London, A. Schloss.

THIS is a curious treatise on illusions of the senses. The author investigates at considerable length their causes and varieties, their relations to bodily and mental diseases, their importance in a medico-legal point of view, and the appropriate methods of cure.

MEDICAL GAZETTE.

Saturday, November 4, 1837.

—
"Vixit omnibus, licet etiam nihil, dignitatem
Artis Medice tueri: potestas modo veniendi in
publicum—il, dicendi periculum non recuso."

CICERO.

THE PROFESSORSHIP OF PATHOLOGY.

It is rumoured that many of the Council of the New London University are still in favour of the establishment of a

pathological chair; and it has even been said that on a division there was a majority of one only against the infliction of this new course of lectures on the wearied and worried students of London. It is strange that the formidable minority of the Council did not reflect that the number of compulsory courses is already so great, that most students find it difficult, not to say impossible, to attend them. It requires undaunted energies, and the most skillful distribution of time, to attend during the same season the lectures required by the College of Surgeons and the Society of Apothecaries, together with the due proportion of dissections and hospital practice; and hence, as we lamented in a former article, this complicated knot of difficulties, hard to unloose, is cut with the greatest facility—the pupil, we fear, too often contenting himself with the certificate, instead of the lectures.

The outcries for additional lectures (perhaps we ought rather to say, for additional certificates), are occasionally heard even from those who are chiefly contemplating a course of study for the general practitioner; but to make a complete physician so much is required by some theorists, that we are reminded of the requisites for a poet, as given by Imlac in *Rasselas*, and feel inclined to answer with the Prince, changing only one word,—“Enough, thou hast convinced me that no man can ever be a *physician*.” Thus Dr. Rothe*, a celebrated German writer, recommended the study of logical, metaphysical, moral, political, statistical, technological, agricultural, mathematical, geographical, chronological, genealogical, heraldic, diplomatic, numismatic, and historical works, to precede chemistry and anatomy;—as a kind of whet to the student’s appetite, we suppose.

We will do the Council the credit to take it for granted, that if they increase the ordinary curriculum of the London pupil, they will increase the time of study in proportion, and not drive more of our alumni to the sham-certificate system. But the misfortune is, that if they merely augment the time in proportion to the augmentation of lectures, they will not do enough; for in London, as elsewhere, the curriculum is over-much for the time. On the Continent five years are a usual period; but then the courses to be attended are so numerous, that it is obvious to every practical man that the graduates must have about as much knowledge of many of the subjects as might be gathered from one of Pinneock’s catechisms. At Edinburgh things are as bad, or worse.

“The period of study for graduation in this University being four years, and the imperative courses being fourteen in number, while the more important ones, such as anatomy, chemistry, surgery, and practice of physic, are always attended at least twice, though the statutes do not require this, from unwillingness on the part of the medical faculty to prevent all recourse to extra-academical instruction, the student is occupied in the class-room at least four or five hours a day on an average, independently of the time required for practical chemistry, practical anatomy, and hospital attendance. Such being the case, it is obvious that any course not really necessary must be injurious: and that any course which jars with another, from the similarity of its subject and station in the order of study, must be still more hurtful”*.

The curriculum of the Worshipful Society in London is even more difficult to put into execution, as twelve courses of lectures, besides dissections and hospital practice, are there crowded into two years and a half; besides which, the student usually attends during the

* Letter to the Lord Provost, &c. by James Syme, Esq., p. 4 and 5.

* Young’s Medical Literature, p. 3. Second edit.

same time (as witness his certificate) the lectures on surgery required by the College.

Suppose, however, that the time were so much extended as commodiously to include not merely the long list of lectures now prescribed, but also one course more; ought that one to be upon pathology? Decidedly not. All those who value the art of medicine as prolonging human life, and subtracting some units from the sum of human misery, will agree with us in preferring a course of lectures on public and private hygiene. We should not then hear it confessed, and complained of on all sides, that some of the simplest problems relating to diet are yet unsolved; nor would Assistant-Commissioner Mott be able to assert, without fear of contradiction, that medical practitioners do not know how much a healthy man should eat. We do not, however, recommend any addition to the already overloaded curriculum. One student in a hundred, with abundance of time at his command, may attend such a course; but for the mass it must be sufficient to treat of some of the more prominent points of private hygiene in the lectures on physiology; while public hygiene, being evidently a point where law and physic touch, will find its place in forensic medicine.

If pathology means, as it ought to do, the doctrine of symptoms, it is already included in the lectures on the practice of physic and the practice of surgery; if it means, as it is often made to do, an account of morbid anatomy, again it forms a large part of the medical and surgical lectures, besides being amply set forth in the dead-house of every hospital. This is by no means the weakest point in our modern medical education; and thousands know the appearances commonly found on dissecting a phthisical or apoplectic patient, who do not know how much bread

should be allowed to the inmates of a union workhouse. It is worthy of remark, that at Edinburgh they have, besides their professor of medicine, a professor of the institutions of medicine; so that the doctrine of symptoms, at any rate, must be plentifully taught. As to the other thing which pathology has been made to signify, morbid anatomy, Dr. Thomson, the professor of pathology, believes that neither the professor of the practice of physic "nor of the Institutions, will affirm that they have hitherto been accustomed to teach their respective departments of medicine with the aid of coloured delineations of diseases, and with a particular reference to the morbid preparations preserved in the Museum or of the Royal College of Surgeons, so as to have anticipated, or in any degree superseded, the purpose, uses, and advantages of the plan adopted by the professor of pathology in teaching pathological anatomy*."

It appears, too, that there are three surgical professors—one of surgery, one of clinical, and one of military surgery; so that it would be strange indeed if the doctrine of symptoms, and the marks which disease leaves in the human frame, were not sufficiently taught. Dr. Thomson, it is true, in the above extract, affirms that morbid preparations and coloured delineations of diseased structure are little attended to by the medical professors. If so, the omission ought to be supplied; an easier thing, assuredly, than saddling the exhausted student with a fifteenth certificate. But we deny *in toto* the justice of this allegation as regards London, in the schools of which, coloured drawings, plates, and preparations, are systematically exhibited by the lecturers on medicine and surgery.

An unpleasant feature of the contro-

* Remarks on the "Memorial of the Town Council of Edinburgh, &c. &c. respecting the Professorship of Medicine and General Pathology," by the present Professor, p. 6.

very raging at Edinburgh on this subject, is, that it has been made to assume a tradesman-like rather than a scientific appearance:—"If we don't sell a very cheap article," cries the shopman to his master, "our second-rate customers will all go over the way." "If we don't make things very easy to students," cry the Edinburgh professors to the Town Council, "they will all go over to Glasgow." But even if you come down to the Glasgow standard, how can you in anywise stand a competition with Aberdeen and St. Andrew's? On the other hand, Dr. Thomson shows that some of the medical students and graduates of the University of Glasgow petitioned the *Senatus Academicus*, on the 14th of December, 1833, to add several courses to those specified in the regulations then lately published—viz. "one course of anatomy, practical anatomy with actual dissection, practical chemistry, diseases of the eye, natural history, and medical jurisprudence*." How many more months or years the petitioners wished also to see added to the time of study, does not appear; but unless a considerable number of months (say fifteen, at least) had been tacked on to the curriculum, we fear that the temporary lustre which a Glasgow diploma might acquire from this splendid array of subjects, would soon have faded away; for that impartial tribunal, the medical public, would not have been slow to discover that an M.D., or newly-certificated man, knew about as much natural history as a young lady fresh from school, or a Russian surgeon†.

* Remarks on the Memorial, &c. p. 17.

† Some ten years ago, it appeared from certain notices in the newspapers, that the Autocrat of all the Russias wished to have a batch of English surgeons. They were to be divided into two classes, the higher one receiving about £60 a year. The first-class men, however, were not to get this handsome salary for nothing; on the contrary, a most appalling catalogue of arts and sciences was required from them. Among other things they were to be skilled in natural history, and be able to write Latin. We never heard whether his Imperial Majesty succeeded in ob-

We, however, have nothing to do with these party squabbles, and would wish to argue the question on higher grounds. It is of no importance to the common weal whether students prefer Glasgow to Edinburgh, or *vice versa*; but it is of the greatest importance that they should attend a reasonable number of judicious lectures, and, therefore, that they should not be overwhelmed by such numerous courses, that professor and pupil are alike seduced into a common conspiracy, while the certificate becomes the assertion of what, in the polite language of the Houyhnhnms, was called—*the thing that is not*.

The lectures being once reduced within sensible limits, we should certainly advocate the adoption of the excellent suggestion of Dr. Kerrison, in our number for August 19, of having some plan for ascertaining the actual number of times each pupil had attended.

In addition to this, we would propose another improvement, of equal importance—perhaps even of still greater, as it might be put into certain execution, without our being obliged to rely on the faithful austerity of the whole army of lecturers.

It would consist in dividing the whole course of study into two or more portions, and obliging the pupil to pass a preliminary examination in certain subjects, before he entered upon the study of others. Thus, supposing the five years' apprenticeship to be still continued, the pupil might pass an examination in botany, chemistry, and reading prescriptions, at the end of half this period, thus leaving the remaining half for the uninterrupted study of the other branches of his medical education.

taining any first-class recruits in this country. If his Majesty's examinations were strict, then unquestionably we have not yet seen the surgeon capable of coming up to his demands; but we conjecture that the Czar was lenient, and would have been satisfied with answers culled from the ninepenny treatises we referred to above.

At present it often happens, as every practical man knows, that all the subjects are left unstudied, and almost untouched, till the last few months, when, by the aid of manuals, grinders, and midnight lucubrations, the idler attempts, and too often succeeds, in attaining the wished-for goal:

—— “post tot discrimina tutus!”

[Since the above was written we have received some documents regarding the Pathological Chair in Edinburgh; they will be found at page 222. We understand that Dr. Thomson, who had come to London for his health, has returned to Edinburgh for the purpose of teaching the class himself, the Town Council having refused to allow his proposed deputy to officiate.]

CHOLERA.

CONSIDERABLE alarm was excited some days ago by the reported re-appearance of cholera at the eastern extremity of the metropolis. The fact is, that well-marked cases of the disease have lately occurred on board the Dreadnought, and in some other situations, but without shewing any decided tendency to assume an epidemic character. Now in this there is nothing beyond what has been witnessed every autumn since the first irruption of the disease in England—the cases, however, having progressively become more rare. Three months ago we saw a well-marked case of cholera at the west end of the town, but it evinced no disposition to spread. Lately, a similar case occurred at St. Bartholomew's Hospital, when the only circumstance which led to any doubt of its being cholera, was, that the patient recovered.

Should the disease become epidemic (of which, however, there is at present no indication), we hope that our professional brethren will give a fair trial to the plan of administering sugar of lead with minute portions of opium, so

strongly recommended by Dr. Graves in a late number of this journal.

DEATH OF DR. MACKINTOSH.

WE regret to have to announce the decease of Dr. Mackintosh, of Edinburgh, which took place last week, in consequence of an attack of typhus fever. Dr. Mackintosh was the author of an elementary work on physic, and of various papers in the medical journals. He devoted a great portion of time and labour to the investigation of cholera, and has made some important contributions to its morbid anatomy, the results of which were communicated to the late scientific meeting at Liverpool. He was a zealous and active man, perhaps a little too *energetic* as a controversialist. He had a considerable class, and his death will be severely felt in the school with which he was connected, leaving, as we believe it does, a blank both in medicine and midwifery.

ST. GEORGE'S HOSPITAL.

Sequel of the Case of Strangulated and Mortified Intestine—Operation for Necrosis of the Tibia—Severe Injury of the Leg; Amputation.

THE case of long-continued strangulation, reported in our last number, was just beginning to offer a chance of recovery, when the account was closed on the 17th, twenty-four hours after the operation. During the rest of the day fluid healthy fæces continued to pass through the wound, and there was no tenderness of the abdomen; but the report at 9 P.M. was, that she had vomited repeatedly, with greater weakness, and a pulse of 130 or 140, and frequent spasmodic attacks of pain, during which the folds and turns of the intestines were seen with the most curious plainness through the parietes; and during the spasms the contractions and consequent alterations of position in the several bowels, were easily perceptible.

Ordered: two eggs, and two gills of pale brandy.

Oct. 18th, 9 A.M.—Slept comfortably; less pain, though still a good deal in spasmodic attacks; tongue loaded and dry; pulse only 100, weak and fluttering; sickness continued at intervals. Fæces constantly passing away, so that the distension of the abdomen is much lessened.

Ordered to continue the eggs and brandy, which she likes.

1 P.M.—Sick occasionally; milk and water, and porter, keep down better than the brandy and egg, which she has brought up.

Ordered a mutton chop also.

9 P.M.—Scarcely any pain now left; but when it does come on it is spasmodic, and the convolutions of the bowels are still very evident. No sickness except once, about an hour after her meat; pulse 120, soft, more full; skin warm and comfortable; tongue rather glazed and dry.

19th, 9 A.M.—Slept a good deal; no pain; no sickness. She is weak, however, and the pulse rapid; skin rather colder, and clammy occasionally.

Red wine, two glasses; tapioca, &c.

9 P.M.—Very comfortable during the day, but this evening the spasmodic pains have returned again at short intervals, during which the spasm of the bowels is very apparent; skin comfortable, however. Feels much better.

20th.—The pains of last night proved to have been connected with the uterus, for early this morning symptoms of miscarriage appeared, since which she has been quite free from pain, and the abdomen is soft and flaccid. She menstruated as lately, however, as ten weeks ago. She seems more exhausted, though quite free from any disturbance connected with the strangulation.

1 P.M.—Weaker; pulse 130; fæces passing readily; discharge from the uterus continues, though not very great. Says she feels much better, though exhausted; not sick once since yesterday afternoon.

21st, 1 P.M.—Very restless during the night; cold clammy skin; pulse not perceptible; looks exhausted. She has just taken a chop, however, besides wine and other things.

6 P.M.—Died,—a little more than five days after the operation.

On examination of the body the next day, there was found to be no trace of inflammation of the peritoneum generally. The part strangulated was the ilium, about six inches from the cæcum, and the two ends of the bowel were adherent to the edges of the femoral ring; and the skin being thin, there was not half an inch distance from the orifice of the mortified bowel to the points of adhesion; so that there seemed every reason to believe that, if she had lived, the artificial anus would have been cured. The part below the opening was small and contracted; that above thickened and vascular, (as would be expected after nine days' strangulation,) but nearly empty, and apparently per-

forming its functions well, without inflammation. So also was the stomach, which was strongly contracted in the middle, and contained only a little brown fluid, though she had taken a chop four hours only before her death, while pulseless and cold, and apparently incapable of digesting food.

The uterus was large and dark coloured, and in the interior the membranes of an ovum were found adherent to the fundus; while the rest of the surface of the uterus, from which the membranes had separated, looked almost sloughy. No embryo, however, was found.

The ovaria were very vascular, as if inflamed, especially the right; both contained numerous Graefian vesicles, and in the right was a large corpus luteum, beautifully developed, from which, doubtless, the ovum had escaped. Around the uterus and ovaria was about a table-spoonful of dark purulent fluid, as if from inflammation of the peritoneal surface of those organs.

The result of the examination, coupled with the preceding symptoms, was evidently such as to justify the conclusion, that she had nearly recovered from the effects of the long strangulation and mortification of the intestine, and that she would probably have got quite well, but for the unfortunate coincidence of a pregnancy, and consequent miscarriage, while in a weak and exhausted condition.

The only operation performed last Thursday, the 20th, was for necrosis of the tibia, by Mr. Hawkins. The patient was a young man, whose leg had been crushed on a railroad six months previously, so as to cause much local mischief and subsequent inflammation, and the death of a large portion of the bone, which had been extensively denuded by the accident. An incision three or four inches long was made to the bone in its upper half, and the shell of new bone removed by three or four applications of the trephine. Out of the opening thus made, several inches of dead bone were extracted. Another incision was then made near the ankle, and some bone having been sawn out by the trephine in the same manner, some more dead bone was extracted, which was too securely wedged in by new bone to be extracted from the upper opening. Seven or eight inches of the shaft were thus dead, and removed. The inflamed parts bled a good deal, and some blue lint (lint dipped in a saturated solution of sulphate of copper), was therefore placed in the cavities of the bone. A good deal of inflammation, and slight sloughing of the skin, followed the operation, accompanied by a good deal of fever, but the wound

became quite clear by the 24th, and has continued healthy since that time till now (Nov. 1st), and the cavities already a good deal filled with healthy granulations.

On Thursday, October 23d, a man was admitted in the afternoon, who had had his leg dreadfully mangled by a heavy waggon wheel going over it, which had produced the most severe compound fracture of the tibia, near the knee joint, besides fractures and compound dislocations of one or two joints of the toes, with much laceration of the soft parts, and had literally torn the upper end of the fibula out of its situation to the tibia at the knee. He was exhausted and delirious by the accident, and the limb was amputated above the knee by Mr. Babington in the evening. He slept well after the operation, and has gone on very well since that time till November 1st, without presenting any thing deserving especial notice.

ACCIDENTS AT THE LONDON HOSPITAL.

Mr. SCOTT, *Surgeon*.

Mr. HAMILTON, *Assistant-Surgeon*.

Oct.	Sex.	Age.	Case.
17.	M.	38	Fractured ribs.
18.	M.	45	Compound fracture of tibia and fibula.
	M.	53	Fractured radius and severe contusions.
	M.	47	Fractured ribs.
	F.	64	Inflamed arm, from punctured finger.
	F.	66	Fractured metatarsal bones.
19.	M.	21	Fractured tibia.
	F.	37	Sprained ankle.
	F.	15	Scald face and neck.
	F.	47	Burnt arm.
20.	M.	15	Scald.
	M.	27	Cut head and severe contusions.
	M.	41	Contused back.
	M.	45	Cut head.
	M.	50	Contused face.
	F.	68	Fractured humerus.
	F.	62	Hernia, strangulated umbilical, (<i>u.</i>)
	F.	60	Contused foot.
21.	F.	59	Contused chest.
22.	M.	45	Fractured ribs.
23.	M.	38	Fractured ribs.
	M.	26	Fractured fibula.
	M.	42	Ruptured vein in the leg.
	M.	63	Contused back and hip.
	M.	71	Contused foot.
	M.	39	Contused neck.

In-patients 26

Out-patients 39

Total 65

(*a.*) In this case the patient had been the subject of umbilical hernia for 28 years, during which time she had constantly worn a truss, although the whole of the protrusion could not be returned. On admission, she was labouring under all the symptoms of strangulation, which had existed about twenty hours. The operation was immediately performed; the sac contained a considerable portion of adherent omentum, and beneath this a large fold of intestine. After dividing the stricture the intestine was returned, and the omentum left in the sac. The bowels were freely relieved soon after the operation, but the woman never rallied from the state of depression she was in at the time of admission, and she died in about 36 hours. Being a Jewess, no post-mortem examination was allowed.

DISCUSSION IN EDINBURGH REGARDING THE PATHOLOGICAL CHAIR.

THE College Committee reported that they were of opinion that the proposal of Dr. Thomson, to have his class taught by Dr. Simpson, might be acceded to.

The following letter from Lord John Russell, in answer to the Memorial of the Council wishing the consent of Government to the abolition of the Chair, was also read at the same time:—

“Whitehall, Oct. 16, 1837.

“My Lord,—Considering the great importance of the decision which it is my duty to form upon the Memorial of the Town Council respecting the Chair of General Pathology, I trust that it is unnecessary to advert to the delay which has occurred in forwarding my reply to it.

“As your Lordship and your colleagues do not now dispute the right of the Crown to institute that or any other professorship, the question to be resolved is, whether any circumstances have occurred since 1831, to induce the belief that the step then taken by Viscount Melbourne, upon deliberate reflection, has been attended with consequences detrimental to the general interests of the University, or injurious to the character of its medical education.

“After a careful review of all that passed on the institution of this Chair, and a grave consideration of the arguments brought forward in support of its abolition, I confess that I arrive at a different conclusion from the Memorial, and feel convinced of the soundness of the judgment which led to its establishment. And in this opinion I am supported by many most eminent and disinterested profes-

sional men. It is complained that the compulsory attendance on this class, having added to the labours of the students at the University, has, therefore, been one of the means of diminishing their numbers; though it is not asserted that it has made any great effect on the number of the graduates. I am inclined to think that the decrease in attendance being, as I take it, general throughout the University, and not confined solely to the medical classes, may be attributed to other causes than the introduction of the class of general pathology into the curriculum, viz. the existence of many other seminaries which have sprung up, and are continuing to do so daily, all over the country, and the means afforded in London and elsewhere of attaining medical knowledge.

"I view this question also on still higher grounds. The curriculum of the University of Edinburgh has hitherto given security to the public as to the efficiency of those who attain the honours of its medical degree, and it would involve her Majesty's advisers in a deep responsibility were they to consent to abolish a branch of instruction which, by all the accounts I can obtain, is daily increasing in estimation with the profession, not only in England, but generally throughout Europe.

"I will not enter into the comparison you have drawn between the curricula at Edinburgh and Glasgow, for I feel assured that on reflection your Lordship and colleagues will see, that if the Government wish to advance the interests of science, and secure to the public the advantages of high professional knowledge, their course should not be to lower the course of study in Edinburgh to the level of Glasgow, or any other Scotch University, but to endeavour to raise them to the higher standard. If, on the contrary, the argument in the Memorial were admitted to be valid, several others of the Chairs in Edinburgh should be abolished, and the cheapest education considered as synonymous with the best.

"I must, therefore, respectfully decline to comply with the prayer of the memorial; but I beg to assure your Lordship, that in so doing I am guided solely by a feeling for the cause of medical science, and the permanent welfare of the University of Edinburgh. And I deeply regret that we should differ so widely in the conclusion to which we have come to on the subject.

"I have the honour to be, &c.

"J. RUSSELL.

"The Lord Provost."

The consideration of the Report of the

Committee was first taken up, when, after considerable discussion, and the approval of the Report having been moved and seconded, Treasurer McLaren moved, as an amendment, that it be remitted to the Law Committee to consider and report, whether, considering that Dr. Thomson has twice intimated his inability to conduct the class of Pathology, and his belief that he will never be able to resume his duties, the Town Council is obliged to sanction the appointment of a substitute, or whether they are entitled to refuse their sanction to any course of imperative lectures which is not delivered by the present Professors—with power to take the opinion of Council.

Mr. Deuchar seconded the motion.

The Council having divided, 15 voted for the amendment, and 10 for the motion.

With regard to Lord John Russell's letter, after some discussion, the Lord Provost was requested to acknowledge its receipt, and to notice the error into which the noble Lord had fallen, in assuming that the magistrates and Council do not now dispute the right of the Crown to institute the Professorship of Pathology, or any other professorship. Treasurer McLaren, and other members, expressed great regret at the conclusion to which the noble Lord had come respecting the Chair, and remarked that the letter was any thing but creditable to him. This was disputed by Messrs. Laing, Ritchie, and others.

QUAIN'S ANATOMY.

To the Editor of the Medical Gazette.

SIR,

THE parts of *Hernia* are of such general interest and importance, and the want of an elementary work on anatomy, illustrating the subject by plates, has been so long regretted, that the announcement of a new edition of Quain's Anatomy was very favourably received. It was confidently hoped that Dr. Quain would not have lost sight of the necessity of simplifying, by means of engravings, the intricate parts of hernial anatomy; and as the omission must diminish considerably the sale of the work, I take the liberty of suggesting the annexation of the subject to the second volume.—I am, sir,

Your constant reader,

B.

Banbury, Oxon, Nov. 1, 1867.

ACTION OF CHLORINE ON THE
ALKALOIDS.*To the Editor of the Medical Gazette.*

SIR,
HAVING observed in your ably conducted journal the remarks of Mr. Schweitzer, on Chirayitine, Quina, &c., I write for the purpose of correcting an error into which he has fallen, in attributing to Mr. Roper those experiments on the action of chlorine and ammonia upon the alkaloids, quina and morphia. If your correspondent will refer to page 362, in the next number of the GAZETTE, he will see Mr. Roper's letter, who, acting as my amanuensis, inadvertently signed his own name. These experiments were first published in your journal of 1832, and subsequently with some additional remarks in the Philosophical Magazine, of February 1835, the date of both of which publications will be found prior to that of the more detailed account of M. André, in the Journal de Pharmacie of 1836. In conclusion, if you will have the goodness to correct a slight error in Mr. Roper's note, my name being spelt "Mason," instead of Meeson, you will oblige,

Yours most obediently,
H. A. MEESON.

Guy's Hospital,
Nov. 1st, 1837.

SIR JAMES CLARK.

HER Majesty has been pleased to create Dr. James Clark a baronet of the United Kingdom.

NEW MEDICAL WORKS.

Quain's Anatomical Plates of the Vessels. Folio, 2*l.* 14*s.*; plain, 3*l.* 18*s.*, coloured.

Rudiments of Physiology. By the late J. Fletcher, M.D., &c., 8vo. 9*s.*

Prize Thesis, on the presence of Air in the Organs of Circulation. By J. R. Cormack, M.D., 1*s.* 6*d.*, 8vo.

Dr. Bennett on the Physiology and Pathology of the Brain, 8vo., 2*s.* 6*d.*

First Principles of Medicine. By A. Billing, M.D. Second edition, 8vo. 6*s.*

RECEIVED FOR REVIEW.

An Exposition of the Signs and Symptoms of Pregnancy. By W. F. Montgomery, A.M. M.D. &c.

Treatise on the Diseases and Injuries of the Larynx and Trachea. By Frederick Ryland.

Kramer on the Diseases of the Ear. Translated by Jas. R. Bennett, M.D. &c.

On the Use of Auscultation and Percussion. By Julius Wolff, M.D., of the Royal College of Göttingen, Heidelberg, &c.

The Retrospective Address delivered at the Fifth Annual Meeting of the Provincial Medical and Surgical Association. By James L. Bardsley, M.D. &c.

On the Superior and Recurrent Laryngeal Nerves. By John Hilton.

Homœopathy: a Thesis.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED
CERTIFICATES.

Thursday, Nov. 2, 1837.

Frederick Margetts, of Hilton, St. Ives, Hunts.—Edward Crickmay, of Beccles, Suffolk.—Oswald Copland, of Chelmsford.—George Bott Churchill Watson, of Derby.—William Gibson Jalland, of Nottingham.—Wm. Deans, of Crediton, Devon.—Adolphus Caudle, of Henfield, Sussex.—William Furnival, of Warrington.—Robert Dunning, of Hull.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, Oct. 31, 1837.

Abscess 2	Fever, Typhus 14
Age and Debility . . . 66	Heart, diseased 4
Apoplexy 9	Hooping Cough 3
Asthma 1	Inflammation 16
Cancer 2	Bowels & Stomach . . . 2
Childbirth 2	Brain 7
Consumption 57	Lungs and Pleura . . . 1
Constipation of the	Influenza 1
Bowels 1	Insanity 2
Convulsions 43	Jaundice 4
Croup 1	Liver, diseased 1
Dentition or Teething . 7	Measles 12
Diabetes 1	Mortification 3
Diarrhoea 2	Paralysis 2
Dropsy 19	Scrofula 2
Dropsy in the Brain . 9	Small-pox 9
Dropsy in the Chest . 1	Spasms 1
Epilepsy 2	Tumor 2
Erysipelas 2	Unknown Causes . . . 134
Fever 16	
Fever, Scarlet 9	Casualties 9

Increase of Burials, as compared with } 322
the preceding week }

METEOROLOGICAL JOURNAL.

Oct.	THERMOMETER.	BAROMETER.
Thursday . 26	from 29 to 54	29.95 to 29.80
Friday . . 27	47 53	29.50 29.58
Saturday . 28	35 51	29.60 29.32
Sunday . . 29	33 47	29.38 29.40
Monday . . 30	34 57	29.35 29.26
Tuesday . . 31	40 49	29.31 29.43
November.		
Wednesday 1	37 57	29.02 28.98

Wind, S.W.

Except the 31st ult. generally cloudy, with frequent and heavy showers of rain; wind very boisterous during the afternoon of the 1st inst.

Rain fallen, 1 inch, and .075 of an inch.

CHARLES HENRY ADAMS.

WILSON & SON, Printers, 57, Skinner-st., London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, NOVEMBER 11, 1837.

LECTURES

ON

FORENSIC MEDICINE;

Delivered at the Aldersgate School of Medicine.

By **SOUTHWOOD SMITH, M.D.**

Physician to the London Fever Hospital.

LECTURE I.

INTRODUCTORY.

Object of Medical Science not merely the removal of Disease—Available to the ends of Justice. Why Forensic Medicine is included in the Medical Curriculum.—Progress of Civilization.—Progressive value of Life.—The Medical Man the most important Witness when Murder is committed—When violence short of Murder is inflicted—When it is necessary that an individual should be deprived of the enjoyment of Social and Civil Rights—When a person is irresponsible for his actions—In all questions relating to the Public Health—Quarantine.—Factory Question.—Why it is necessary to make Forensic Medicine a distinct branch of Study.—Term proper for the designation of this branch of Science.—Precise province of Medical Witness—Of the Lawyer.—Subjects included in the field of Forensic Medicine.—Tabular View.

GENTLEMEN,—That branch of science which it is your privilege to study, and the blessings of which I hope it will be your happiness largely to diffuse among your fellow beings, does not complete its office when it mitigates the suffering of hopeless disease, or changes disease to health; nor even when, guiding the hand of the surgeon into the very inmost recesses of the body, it enables him to avert the progress of a mortal malady, such, for example, as aneurism, or at once to extirpate some malignant growth, which is incompatible with the function of a vital organ, or which, by the incessant irritation it excites, is

rapidly exhausting the powers of life. These are objects which it does accomplish, and the magnitude of the service it thereby renders to humanity is not the less, because it is rendered daily and hourly.

But there are other services which it is called upon to afford, less often, it is true, but, when the occasions for them arise, scarcely less momentous in their nature and results.

In some cases, indeed, the award of justice is more important than the removal of pain, or the cure of disease; and the vindication of innocence is a higher object even than the preservation of life itself. Before "legal tribunals, on innumerable occasions, justice cannot possibly be awarded, innocence cannot be vindicated, crime itself cannot be visited with the punishment which it merits, speaking vulgarly—with the correction which ought to be applied to it, speaking philosophically—unless medicine and surgery, and their great allies, chemistry and physiology, and several other branches of physical and vital philosophy, concur in bringing the aid of science to the administration of the law.

So strong was this conviction in the minds of a few enlightened men, and so energetically, and, after a time, so effectually, did they labour to communicate this conviction to others, that it has at length become the universal conviction. Accordingly, the authorities who have at present the regulation of medical education, have now decided that the study of medicine (using the term medicine in its largest sense,) with a special reference to its application to legal purposes, shall form an indispensable part of the medical curriculum.

Perhaps, then, I cannot more profitably occupy your time on the present occasion, than by pointing out the philosophical, the moral, and the professional grounds, on which this decision may be regarded as

wise. The statement of these grounds will afford me a fitting occasion to shew the peculiar advantages which are likely to result from the obligation under which every medical student is now placed, to give a separate and extended consideration to the subject of Forensic medicine.

In the savage state there is no institution of property, and no security for life. In the barbarous or semi-savage state, the protection of property is exceedingly imperfect, and life is correspondingly insecure. From the establishment of property result the culture of the soil—the invention of manufactures—the accumulation of capital—leisure for the cultivation of art, science, and literature—and a consequent and a continually advancing improvement in the intellectual and the moral character of man. But the blessings which thus flow from this beneficent institution, are crowned by the protection which it affords to human life, and by the value which it causes to be set upon it.

In a certain stage of civilization, the crime of murder is punishable only by a pecuniary fine. The amount of the fine affords a measure of the value of life in the estimation of the community; for the price set upon an individual's life is always determined by his rank; being great or small, according as his rank is high or low.

Two remarkable changes take place whenever, in the progress of civilization, the great institutions of property and of law become settled and secure. In the first place, rank ceases to be the standard of the comparative value of life; the rich and the poor, the prince and the peasant, the philosopher and the clown, are placed in this respect on a perfect equality. And in the second place, the value of life is so prodigiously raised, that the wilful and violent destruction of it now constitutes a crime of the highest magnitude, and is visited with the severest penalty which the collective power of the community can inflict on an individual man.

In this state of society, from the earliest infancy, the sympathies of all hearts, and the associations of all minds, are so directed by education, that the utmost terror is connected with the perpetration of the crime of murder; so that the murderer, though no human being but himself be cognizant of his guilt, is the prey of a secret remorse, such as few have been found able to sustain; while the discovery of the crime at once cuts off the criminal from all intercourse with his fellow beings, deprives him even of their pity, and induces them, without exception, with a stern and inflexible feeling of duty, to shed blood for blood—to take life for life.

But the institution of law would be use-

less, unless, when the law is violated, it were possible to bring home to the wrong-doer evidence of his guilt. In proportion as in the progress of society its institutions really become provisions devised by reason for securing justice, in that proportion is the administrator of the law careful that the penalties annexed to its violation are inflicted on no one, unless satisfactory evidence be adduced that he has really committed the crime laid to his charge; and the enormity of the offence, and the magnitude of the penalty attached to it, do but increase the demand that the proof in every case be clear, exact, and complete.

Now it happens that in almost all the cases in which life is taken away by a criminal act, some of the most important questions that arise are questions which can be satisfactorily answered only by those who are acquainted with the phenomena of life. The exact nature of injuries inflicted on the living body can be ascertained only by those who have studied the structures to which life is attached—who are acquainted with the functions in which life consists—who know the derangements which constitute disease, and the diseases which terminate in death. In the great majority of cases of this class, therefore, the medical man is the only witness in whose power it is to afford that precise and complete evidence which is required in the administration of justice. The studies to which the physician and surgeon are devoted render them familiar with the influence of external agents upon the structure and functions of the body, with the effects produced by a variety of noxious agents, and more especially with the phenomena that result from the external and the internal administration of specific poisons—the agents by which the crime of murder is generally perpetrated. Accordingly, it is upon the evidence given by the medical witness—evidence which his knowledge of subjects of this class peculiarly fits him to collect and state—that the fate of the accused, in a great measure, depends; while it is quite certain that exact knowledge of this kind is indispensable, not only to the detection of the particular poison administered in any individual case, but often also even to the discrimination of death by poison from death by natural causes.

Again, in every community in which civilization has made tolerable progress, not only is life protected with the most extreme care, but all violence wantonly or maliciously inflicted on the person, though it fall short of producing death, constitutes a crime of high magnitude. Hence the dismemberment, the disfigurement, or the disablement of the body, are crimes to which specific penalties are attached. But of the

mode in which injury of this kind has been inflicted—the agent by which it has been accomplished, whether mechanical or chemical—the probable evil consequences that will ensue—and the extent and duration of those consequences—of all this the physician or surgeon is the only competent judge, the only man who can give to the administrators of the law exactly the information they require. This, therefore, is another class of subjects in relation to which the evidence of the medical witness is constantly required in the courts of justice.

But if in the early and rude states of society the protection afforded to life itself be slight, still less care is taken to secure the enjoyment of life, by securing to each individual the means of enjoyment. Among the means of enjoyment may be reckoned especially the use of property in all its shapes, and in all the modes in which it is capable of affording pleasure, together with the exercise of various rights and privileges which grow out of the social state. Against the deprivation of any member of the community of any of these, without a paramount cause, the legislator, in proportion as he has accomplished the object of his science, has effectually guarded. But there are diseases, some of them congenital, others the result of causes which operate at various periods of life, which incapacitate the individual, both for taking care of his own property—for making it the means of enjoyment to himself, or to others—and at the same time for performing various social and civil functions. Hence arise questions relating, on the one hand, to idiocy and insanity, and on the other to impotence, to sterility, to legitimacy—questions often of the utmost delicacy and difficulty, involving the dearest interests of the human being. And with these questions of disqualification are necessarily connected questions of irresponsibility—questions of exemption from punishment, notwithstanding that actions have been committed which in themselves are subject to the heaviest penalties. On all questions of this class the medical witness is the only man who can guide the judgment of the jury and the judge—who can prevent them from misdecision; that is, who can save them from permitting, or from inflicting, the most terrible evils.

But the administrators of the law are, moreover, frequently called upon to decide questions of another kind—questions relating to the public health, respecting which they must necessarily seek for guidance from medical science—questions which involve, on the one hand, large amounts of property employed in carrying

on certain branches of manufacture, and on the other hand the salubrity of the neighbourhood in which the manufactories are situated. Certain processes carried on in these manufactories are highly deleterious, or are supposed to be so. They poison the air, or they poison the water, or they injure in some mode or other, real or supposed, the public health. Are these processes really pernicious? If so, do they admit of remedy? If they admit of remedy, what is the effectual remedy? If they admit of no effectual remedy, do they endanger the public health to such a degree as to render it indispensable to sacrifice the amount of property in question, rather than permit the far greater sacrifice implied in the injury of the public health. To all these questions, the true solution upon which alone can be formed a just legal decision, can be given only by medical science.

There are, too, several questions relating to the public health, which cannot be properly included under Forensic medicine, but which would readily find a place under a general term, of more extensive significance, such as that of State medicine. Questions of this class are, all questions relating to the salubrity or insalubrity of particular localities; the special causes of the salubrity or insalubrities of such localities; the structure of buildings with a view to health, and more especially as respects their proximity to each other, and the facility with which they admit of free and perfect ventilation; the drainage of the country, in general, and the draining of the streets and houses of the city, the town, the village, in particular; the cleanliness of houses, streets, villages, towns, and cities; the immediate removal of all putrefying vegetable and animal substances; the freshness and sound condition of provisions—vegetables of all kinds, meat, bread, flour, fruits of all sorts, water, &c. On the condition of these most important agents depends the general prevalence, or the entire absence, of a very large proportion of the diseases which afflict and shorten human life; and in particular on their condition depend most essentially, and perhaps entirely, the presence or absence of epidemic and pestilential diseases.

But even that court, which is above the law itself, and which makes the law, the High Court of Parliament, is on some occasions under the absolute necessity of seeking counsel of medical science, and counsel from this source it ought to seek on more occasions than it does. There is at the present moment one question of the highest commercial importance, which it ought to investigate anew, and in the investigation of which it ought to obtain the assistance of the best medi-

cal science it can secure, viz. the question of Quarantine regulation, respecting which there are in actual and constant operation practices formed on prejudices and absurdities worthy only of the age of witchcraft—practices which uselessly and grievously impede and fetter commerce, which cost the country, for doing worse than nothing, hundreds of thousands of pounds annually, and which tend to keep up notions which as they had their origin in the darkness of barbarous ignorance, so they are completely dispelled by the light of science—notions, the moral and social influence of which is as pernicious as their foundation, when viewed by the eye of reason and knowledge, is seen to be baseless.

I may refer to a recent example in which Parliament sought counsel of medical science, relative to a question in which vast mercantile interests were involved, in which the interests of nearly four hundred thousand of the working people of the country were equally involved, and which some of the best and some of the worst passions of our nature had alike combined to complicate and perplex. I refer to the Factory question; on which difficult question Parliament sought counsel, among other sources, of medical science, and to the essential difficulty of which, medical science afforded the solution.

On the one hand, it was asserted that the great factories of the country were the sources of wide-spreading and fatal diseases and of the most frightful vices; and on the other hand it was affirmed, that these same factories were perfectly healthful, and free from the production of any kind or degree of vice. The labour to which the infant portion of the manufacturing population was subjected, was said to be so excessive as to produce extreme physical suffering, and permanent bodily deformity and disease; and, according to representations constantly and vehemently made, this excessive labour was extorted by the most cruel punishments. On the contrary, the labour of the factory was declared to be light and easy, and in no degree excessive, and the punishment of the children was affirmed to be not greater than the chastisement which takes place in a well-regulated school or family.

The Government issued a commission to ascertain and report upon the facts of this matter. Civil commissioners were appointed to investigate more especially the bearings of the question on the great commercial interests of the country, and medical commissioners to examine more especially into the health of the factories in general, and of the infant workers in particular. On the examination of the whole body of evidence, collected by the commissioners,

it was reported that the labour, or rather the duration of the labour, of the young people was excessive, and that up to a certain age—viz. that of puberty—the labour, instead of being continued 12 or 13 hours per day, as was the case at that time, should be restricted to eight hours per day: at the same time a method was suggested, viz. that of working by two sets of children, instead of one, by which the requisite restriction might be imposed upon the labour of the children, without interfering, in any material degree, with the labour of the adult. The plan suggested by the commission was adopted by the legislature, and the law, now that obedience to it is enforced, is found to accomplish the purpose for which it was enacted. In all the factories in the country the essential principle is recognized and obeyed, which before the labours of the commission was maintained to be impracticable, viz. the separation of infant from adult labour; the expediency of placing a legislative restriction on the former, and of leaving the latter entirely free; and the concurrent testimony of all classes shews that the statute, founded on these principles, gives, upon the whole, satisfaction to all parties; to the operative no less than to the mill-owner; while the public clamour, consisting partly of truth and partly of falsehood, and raised partly from benevolence, but far more from sinister and selfish motives—that clamour which had so long fostered discontent among the work-people, and impeded the operations, and even endangered the capital of the manufacturers, has been completely silenced.

If, however, we restrict our view, which we must necessarily do on the present occasion, to the subjects which fall properly within the province of Forensic medicine, we shall find that the information which the judges of the law require of the medical witness, is spread over almost the entire field of medical and surgical science. Anatomy, which teaches the structure of the body; physiology, which investigates its functions; semeiology, which makes us acquainted with the symptoms that denote a morbid condition of those functions; pathology, which instructs us in the changes of structure produced by disease; chemistry, which enables us to detect, under all its disguises, the presence of any deleterious substance in the body; and toxicology, which teaches us the special action of the entire class of poisons; all contribute their aid, each in its peculiar mode, to save the administrators of the law from error, and to guide them to a just decision.

But you will say, these branches of the medical science constitute the entire sci-

ence ; why, then, make a distinct branch of study of what must necessarily be included in the general study of medicine? This is a natural question, and the answer to it will place in a still clearer light the nature and object of our subject.

You may make yourselves thoroughly acquainted with every branch of your art and science required in ordinary practice ; you may know promptly and accurately what to do for the benefit of your patient, under the ordinary circumstances of disease, or even in a case of sudden and great emergency ; but a case falls under your notice—a suspicious or a criminal case—a case involving life and death, respecting which you are required to give evidence in a court of justice. Here, new, unexpected questions arise—questions which the court requires to be solved with clearness, accuracy, decision—questions which no one who is not familiar with the more abstruse and difficult points of your science can possibly answer. You may really know all that is necessary to the formation of a clear and just opinion on this judicial case ; you may not be positively ignorant of a single fact or phenomenon, the knowledge of which is requisite to the decision of the matter in hand ; and yet, without some previous consideration, perhaps even without some close study, it may be impossible for you to bring what you really know to bear with the requisite precision and force on the point in question.

In a court of justice, too, you are placed in a new situation. Questions are presented to you requiring to be answered promptly, distinctly, decidedly, accurately—questions which now come to you in a new shape : not like questions proposed to you by persons as well acquainted with your profession as yourselves, but questions proposed by persons wholly ignorant of your profession. Such persons have difficulties which you never before understood—indicate doubts and uncertainties which had never occurred to you. They call upon you in the most solemn and earnest manner to resolve those difficulties—to set at rest those doubts. But if the matter in hand have been considered by you only generally, vaguely, loosely, now, at the very moment when you are called upon to decide on professional responsibility, on the stake of your own professional reputation—difficulties and doubts may for the first time come into your own minds. There may be no real difficulty—no ground for doubt. There may be difficulty—there may be good ground for doubt. Ought I to give a positive opinion? Ought I to give a restricted and guarded opinion? This thought occurs to you—you do not feel sure—you hesitate. All eyes are on you. The

barrister urges—urges you for a positive answer. The judge scrutinizes not the words only that fall from your lips, but the mind that dictates them—the intelligent, instructed, decided, trustworthy mind, or the ignorant, uninformed, hesitating, and untrustworthy mind. You are profoundly conscious of all this. The probable consequence is, that you stand there, before highly intelligent and deeply interested spectators, professing to be a man of science—appearing there for the express purpose of bringing your scientific knowledge to bear on a particular point for the guidance of justice ; yet there you stand confused, unable to connect your ideas, still more unable to express them—bitterly feeling in your own heart, and exciting the resentful feeling in others, that you throw perplexity, confusion, and darkness, on that which you were called upon to disentangle, and to set in a simple, clear, and practical light.

It is singular that perhaps the very greatest mind, at least in particular departments of our science, which our country has ever produced, was once placed in a situation in a court of justice, scarcely less humiliating than that which I have just supposed. I allude to no less a person than John Hunter. That illustrious man was called to give evidence in a court of justice, anterior to the period when any general attention had been paid to the subject of Forensic medicine. The case on which he was examined as a medical witness, was one of great importance. He ventured into court without having taken the precaution to examine the point on which he was to speak with scientific exactness and conscientious care. The consequence was that the cause of truth and justice, as far as that cause was capable of being affected by his evidence, was endangered, and his great and deserved reputation could not protect him from the severe animadversion of others at the time, nor save him subsequently from his own self-reproach.

Mr. Hunter was called to give evidence as to the operation of a particular poison in the case of Captain Donegan. To this remarkable case I shall have to recal your special attention, for another reason, at a subsequent lecture. It was presumed that this great physiologist must be well acquainted with the action of poisons on the animal economy. On this presumption he was summoned to give his assistance in the present case. In what manner did he perform his office? What was the impression made by it upon the minds of those who were called upon to consider its weight and bearing. Hear what the judge who presided on the trial said of it in his

address to the jury:—"You have had," says his lordship, "one gentleman called, who is of the faculty, and a very able man. I can hardly say what his opinion is, for he does not seem to have formed any opinion at all in the matter. He at first said he could not form an opinion whether the death was or was not occasioned by the poison, because he could conceive it might be ascribed to other causes. I wished very much to have got a direct answer from Mr. Hunter if I could, what, upon the whole, was the result of his attention and application to the subject, and what was his present opinion; but he says he can say nothing decisive."

This is the account of the matter given by the judge. The case, as recorded, is so clear, that I take it, as you will see hereafter, as an example of satisfactory circumstantial evidence. His own regret (as we are informed by Sir Astley Cooper) at the position in which he allowed himself to be placed, terminated only with his life. "He found himself (says Sir Astley) a good deal embarrassed on the occasion—the lawyers took advantage of his embarrassment, and he used to express his regret publicly in his lectures, that he had not given more attention to the subject, before he ventured to give an opinion in a court of justice."

Thus has the most distinguished man in our profession left upon record a lesson taken from his own experience, which we ought none of us ever to forget.

How different your situation if you enter the court with a prepared mind—prepared by study—that is, in possession of accurate knowledge, all the bearings of which you have carefully and thoroughly considered: your appearance then will be collected and calm, your replies plain, concise, clear—your language that of common parlance, so free from technicality that it will suggest to your hearers that you have considered their situation, that you have supposed them to be unacquainted with the subject, and that your single aim has been to put them in possession of all that is really known about it. If a question in explanation be asked you, it will be the means of exhibiting, in a clearer light, the precision and accuracy of your knowledge. If counsel impugn your fact, it will afford you an opportunity of adducing the evidence of it. If he question your inference, it will but afford you an occasion to show in a more striking manner that it is a strictly legitimate one. If he carp at your language, it will enable you to bring out what is in your own mind with greater exactness and impressiveness.

No one can doubt that it is possible to do all this. No one can doubt that it is

desirable to do it. No one who, both for his own interest, for the honour of his profession, and for the common advantage of the community, would not wish to see himself in the latter rather than in the former position.

Well, nothing is more easy. There is commonly no real difficulty in judicial questions; no reason why every medical man should not appear with as much honour before an assembled multitude in a court of justice, as he does with the approval of his own conscience at the bedside of the sick. The main subject with which he has to deal is the same; it is the view which he is to take of it only that is different. He is able promptly and effectually to relieve the sick, because he has studied the resources of his art in relation to the phenomena of disease. Let him but study the resources of his science in relation to the wants of the administrators of the law, and he will appear with as much dignity in a court of justice as he does with usefulness at the bed-side of the diseased.

Suppose, for example, that you study the different branches of this science with a special reference to all the main questions upon which you can be called to give evidence in a civil or criminal court. Then every one of these branches will present itself to you in a new aspect—*anatomy, physiology, semeiology, pathology, chemistry*;—there is not one of these departments of your science which you do not now view in a new light—which does not receive a new direction, and consequently which does not produce new results, and those of the highest importance, to the very best interests of the community. What, for example, does the science of semeiology become to you now? That by which you ascertain, with clearness and exactness, the difference between the symptoms of poisoning and the symptoms produced by natural disease. What the science of pathology? That by which you distinguish the appearances in the dead body that indicate death by poison from those that indicate death by natural causes. What the science of chemistry? That by which you are enabled to discover the exact poison that has been administered; and often, from its state of solution in vegetable and animal mixtures, to exhibit it pure, and even in the solid form, to the naked eye. What the science of physiology? That by which you are enabled, by means of direct experiments on animals, to settle by analogy doubtful questions relative to the operation of poisons on man.

The use, then, of separating Forensic medicine from every other study, and of erecting it into a distinct science is, that

it embodies into one condensed and well-ordered whole, the information which is spread over a vast and diversified space. Ranging over the entire field of medical and surgical science, the science thus formed collects from innumerable sources facts and principles, which by no other means could be viewed in combination; from the resources of one branch it supplies the deficiencies of others; by means of one it tests the accuracy of another; and the whole mass of knowledge which it thus accumulates from every tributary source, it pours in one direction, with a precision and force worthy of its object—the detection of crime, and the vindication of innocence*.

Various names have been proposed to designate the science which, in this manner, has for its object the application of the different branches of medical science to the administration of justice. Medical jurisprudence is one: as bad a name as could be well devised, for it does not concern the knowledge of law as applicable to medicine, but the knowledge of medicine as applicable to law.

Perhaps the least objectionable and the most definite designation of it is the name which now seems to be superseding every other, viz. Forensic medicine. The term medicine being understood in its largest sense, this name will then signify medicine capable of application in the forum, or in a court of justice. It is not, then, a branch of medicine, but the application of every branch of medicine, which is capable of such application to a legal purpose.

The term State medicine is less definite, though more extensive; for this name comprehends Forensic medicine, and something more, namely, whatever the science of medicine is capable of contributing towards the conservation of the public health.

But by whatever name we designate this application of medical science, it is of great importance to perceive clearly its precise object. It does not include, as the term medical jurisprudence would appear to indicate, two widely different subjects, medicine and law. In the application of medical science to a legal purpose, the functions of two sets of professional men, the physician and the lawyer, are indeed brought into operation; and perhaps it will help to place the subject in a clearer point of view, if we consider the exact nature of the work which each of these professional men has to do—if we define the limit

which separates the portion of the field occupied by the one, from that occupied by the other.

In relation to every case which he is required to investigate, or upon which he is called to give evidence, the medical witness has two things to do: 1st, he has to furnish the tribunal with certain facts; and next, to state the inference which in his judgment the facts warrant. It is a case, suppose, of alleged poisoning. There are certain symptoms during life. Are those symptoms sure indications that poison of some sort has been administered? Might they on the contrary have been produced by natural disease? Certain appearances present themselves on the inspection of the body after death. Are those appearances unquestionably the effects of poison, and if so, of what poison? Or are they the products of disease, and if so, of what disease? A substance of a suspicious nature is found in the alimentary canal; its physical and chemical properties, when tried by their appropriate tests, what do they declare it to be? Is it poison, or is it no poison? If it be poison, what poison is it? No deleterious substance is appreciable by the eye in the alimentary canal; but the contents of the stomach and intestines are analysed; the analysis yields a product of a suspicious nature; on further investigation this suspicious substance turns out to be one of the corrosive poisons. This is established by chemical tests. Well, the invaluable light which chemistry has now shed on this subject, does it agree with the indications already afforded by the semeiology of the case, or by the pathology of it; or may further light be thrown upon it by any physiological investigation?

Such is the nature of the inquiries which it is the office of the medical witness to pursue; and exact answers to which it is his duty, as far as it is in his power, to find. And why is he specially called upon by the judges of the law to furnish answers to questions of this class? Because he, beyond any other man, is qualified to give correct answers. On all subjects of this kind he is what in the language of jurisprudence is termed a skilled witness; his peculiar knowledge enables him to give the judges precisely the information of which they are in search. On all the subjects that come before them, what the judges want are correct answers to certain questions; and they always seek, or ought to seek, for such answers from the best sources at their command. And the variety of sources from which it may be necessary occasionally that they should seek information, is well illustrated by the following case:—

Near the Place St. Michael, at Paris, op-

* See Christison on Poisons.

posite the Rue St. Hyacinthe, there lived a woman, a widow, who kept a small shop, and slept in a little room behind it. She passed among her neighbours as being rich, and was supposed to have considerable sums of money in the house. Her only domestic was a lad, who slept in a room three stories above the shop, the stairs to which had no communication with the part of the house in which his mistress resided. In order to reach his chamber it was necessary to go into the street. When the boy went to rest, his mistress herself always closed the door of the shop after him; and locked it, and kept the key in her own possession. One morning the shop door was observed by the neighbours to be partly open; yet neither the boy nor his mistress was stirring. On examining the door it had no appearance of having been broken open; but on entering the house they found the widow lying dead in her bed; the body was covered with wounds, which had been inflicted apparently by a knife that lay stained with blood in the midst of the room. The dead body contained in one hand a small quantity of hair, apparently torn in the struggle from the head of the murderer; while the other grasped a portion of a cravat. Near the bed was a coffer, which had been forced open. Suspicion fell upon the boy. A locksmith was employed to examine the lock of the door; he proved that the lock had not been forced; the key was found in the bed-room of the boy. A hairdresser was employed to compare the hair in the hand of the murdered woman with the hair of her servant; the hairdresser pronounced it to be the same. The washerwoman who had been in the habit of washing the boy's linen, was employed to examine whether the portion of cravat found in the other hand of the dead body, was similar to the cravats worn by the servant; she was of opinion that it was perfectly similar. A well-known companion of the lad was called to state whether the knife found in the room had ever been seen by him in the possession of the accused, and whether he believed it to be his; he declared that such was his belief; and lastly, a surgeon was employed to examine the wounds, who stated that they were certainly inflicted by a sharp cutting instrument, and that without doubt of such wounds she died.

Now in this case you see the information afforded the judges was derived from five different sources, of which the medical evidence formed one. Each of these deponents was a skilled witness: each was capable of giving correct information on a particular point. In relation to questions

of this class, the medical witness is generally capable of giving exact information on numerous points, and those of the very highest importance; he is capable of furnishing certain facts, the very facts wanted by the administrator of the law, in order to enable him to form a just decision. So much, then, for the first duty of the medical witness; that of affording to the jury and the judge the facts they need to guide them to a just conclusion.

But besides certain facts which it is the part of the medical witness to ascertain, collect, and state, he has another duty to perform, of equal magnitude, and, if possible, of still deeper responsibility; for, in the second place, he has to declare the inferences which he believes to be deducible from the facts which he has ascertained. The nature of this duty, and the consequences that result from the manner in which he performs it, appear to me to be illustrated in a very striking manner by a case, the details of which were told me by one of the judges of the Justiciary Court of Scotland, who happened, on the trial in question, to be the presiding judge.

Two men in a cottage quarrelled. A scuffle ensued. One of the men was killed. The quarrel originated, there was reason to believe, in the predetermination of the survivor, who was resolved to excite it; and who, it was proved in evidence, had a deep and rooted ill-will against the deceased. The body was examined judicially by no less than six medical men. A wound was found toward the back part of the head. Two questions were to be decided: First, was this wound the cause of death? Secondly, was it produced by a fall on the floor, or was it inflicted by any kind of weapon? If produced by a fall on the floor in the scuffle, it was a case of manslaughter; if inflicted by a weapon, it was murder. All the medical witnesses were agreed that the wound was the cause of death; five out of the six stated that it might have been produced by a fall on the floor; the sixth declared that in his opinion this was impossible; for, on a most careful examination of the exterior of the wound, as exhibited in the integuments of the head, in the bone of the skull, and in the brain, he was satisfied that it was produced by some pointed body capable of piercing through all these substances; and that if occasioned by a fall on the floor, it could have happened no otherwise than by the striking of the head against some sharp body sticking up out of the floor, such as a nail. At the time of the event the floor was carefully searched; no such body could be found. On the other hand, no weapon could be discovered in the

room or about the person of the defendant, by which he could have inflicted such a wound.

Now the life or death of the defendant in this case depended on the inference drawn by the medical witnesses, from a fact obvious to their senses. The inference drawn by one was opposed to the inference drawn by five. In the mind of the judge who presided at the trial, accustomed as he was to evidence, there was, indeed, not the slightest doubt that the inference of the single witness was the correct inference. But the inference of one skilled witness could not be taken in opposition to the inference of five presumed to be equally skilled. The man was acquitted of the charge of murder.

After the trial he confessed that he went into that room determined to have his revenge, and that in the scuffle he snatched up the snuffers, the sharp point of which he struck with all his force on the head of his enemy. It had penetrated through the bone of the skull, and gone deep into the brain.

It is scarcely possible to conceive a case which places in a stronger light both the nature of the duty of the medical witness, and the care with which the duty should be performed.

The result of this trial to the single medical witness, whose sagacity had enabled him to detect the truth, is worthy of notice. It was a trial that excited great attention. The presiding judge had been struck with the clearness of mind, the patient, cautious investigation of this medical witness: he often spoke of it to the magistracy and others by whom he was surrounded; and the consequence was, that this medical man soon found himself in possession of the confidence of the best educated and influential people of the neighbourhood.

What the law is it is without doubt highly desirable that the medical man should know, as it is desirable that he should be acquainted with every thing that is to be known; but of the applicability of the facts which he states, and of the inferences which he deduces to the rule of law, this it is for the tribunal, not him, to determine; and as a medical witness, either with the fact of the law, or with the bearing of his evidence upon it, he has nothing whatever to do.

And in like manner as the medical witness has no concern with the law, so the lawyer has no concern with the medical fact, or with the medical inference, excepting it be to extract, in the fullest and most perfect manner, the entire medical evi-

dence, to examine its trust-worthiness, and having decided on that to apply it to the rule of law. "It is my opinion," says the medical witness, "that such a blow has been inflicted, and that it has been followed by such a consequence; for my experience has taught me that such is the sequence." And if there be no adverse evidence, if the statement of the medical witness agree with the statement of other medical witnesses, the lawyer must take this statement as the true statement. The lawyer's experience will not allow him to affirm that the sequence stated by the medical witness is not the true sequence; he must therefore receive it as the true one, and to the aggregate facts of which this sequence is a part he must apply the law.

Clearly, then, do you now see the exact province of the medical witness, and the exact province of the administrator of the law. And what is the impression forced upon the mind by this view of their relative duty? An impression of a responsibility attaching to the medical witness, that is, I had almost said, appalling. The lawyer has merely to draw out, merely to elicit the evidence of the medical witness, to sift it, to weigh it, and then to determine its applicability to an appointed rule; but the facts themselves, the inferences deducible from those facts, that is, the material with which the lawyer deals, all this the medical witness has to furnish.

It would be idle to say any thing with a view to enforce the obligation under which this position and office place you as medical witnesses, to prepare yourselves fully for the performance of your duty. I will only add a word relative to the subjects which require your special study, and the consideration of the most important part of which will be included in the present course.

Every thing in relation to which you can be called to give evidence in a court of justice, must relate either to the extinction of life—to the infliction of personal violence, without the extinction of life—to disqualification for the enjoyment of social and civil privileges, and the exercise of social and civil functions; or to irresponsibility for the commission of acts which the legislature has pronounced criminal.

Life may be extinguished either by purely physical causes, or by intentional human agency.

The extinction of life by causes purely physical includes all the causes of sudden death, whether from internal diseases or from external agents.

The extinction of life by intentional human agency, includes poisoning, suffocation, wounds and bruises, suicide, fœticide, infanticide,—includes all cases of this sort when brought about by human purpose.

The infliction of personal violence without the extinction of life, includes the dismemberment, the disfigurement, and the disablement of the body; rape; corporal punishment; together with injuries sustained in consequence of unskilful surgical operations, and improper medical treatment.

Disqualification for the enjoyment of social and civil privileges, and the exercise of social and civil functions, relates either to physical or to mental disqualifications. Physical disqualification relates especially to disqualification for marriage, and includes the subjects of impotence, sterility, and legitimacy; while mental disqualification includes the extended and most important subject of insanity, in all its forms and degrees; together with the distinct, though closely related subject, of irresponsibility.

In this table is brought into one view the main divisions of this portion of the field of knowledge.

I.—EXTINCTION OF LIFE.

1. Physical causes.
 - Internal diseases.
 - Sudden death.
 - Apparent death.
 - External agents.
2. Intentional human agency.
 - Poisoning.
 - Hanging
 - Drowning
 - Suffocation
 - Pregnancy.
 - Parturition.
 - Birth.
 - Fœticide.
 - Infanticide.
 - Snicide.
 - Wounds, &c.

II.—PERSONAL VIOLENCE WITHOUT THE EXTINCTION OF LIFE.

- Dismemberment.
- Disfigurement.
- Disablement.
- Rape.
- Corporal punishment.
- Unskilful medical treatment.
- Unskilful surgical operations.

III.—DISQUALIFICATION.

- Physical.
 - Impotence.
 - Sterility.

- Legitimacy.
- Age, &c.
- Mental.
 - Idiocy.
 - Insanity, &c.

IV.—IRRESPONSIBILITY.

Gentlemen, it is singularly fortunate for the interests of the community, that while on the one hand the very nature of medico-legal investigation precludes the possibility of its being pursued by any but a scientific man, and while, on the other, none but a person of skill, trained to observation, and exercised in the practice of discrimination, is competent to notice and record the moral evidence, so these two qualifications are combined in the well-educated medical practitioner. Without scientific attainment he is unfitted for any part of his profession, while his situation about the patient places him in the best position for observing and collecting the moral evidence; he sees all the parties in the first instance, comes into intimate communication with them; no look, no attitude, no expression, whether voluntary or involuntary, whether accidental or designed—no action can escape the notice of the acute and enlightened medical practitioner.

Your assumption of the character of well-educated medical men is a contract which you enter into with the public, that you will gain the information necessary to enable you to perform with promptitude and skill the various duties of your profession. When they consult you, they do so in the confidence that you have fulfilled your part of this contract. In a judicial case more especially, they will never forgive—they never have forgiven—any detected betrayal of this confidence, whether such betrayal were the result of ignorance or of indolence. Be your previous reputation what it may, you will lose all character for ever, if once, in a court of justice, before the eyes of intelligent and deeply-interested spectators, you indicate incompetence. To this subject appear to me to apply with peculiar force the words in which one of the most honoured and revered teachers of our day was accustomed to conclude his introductory lecture on anatomy. Those words have always appeared to me not memorable only, but deeply affecting. "In other professions," says the late Dr. Baillie, "indolence may be a folly; in ours it is a crime!"

ON THE
PRACTICAL INFERENCES
WHICH RESULT FROM
SOME LATE PHYSIOLOGICAL
INVESTIGATIONS
RESPECTING THE
LAWS OF THE VITAL FUNCTIONS
IN THE MORE PERFECT ANIMALS.

By A. P. W. PHILIP, M.D. F.R.S.L. & E.
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To the Editor of the Medical Gazette.

SIR,

It is a remarkable fact,—but on comparing the whole of the evidence it will, as far as I am capable of judging, be found undeniable—that, in the present advanced state of medical knowledge, a cause of many of the most formidable diseases to which we are subject is in constant operation around us without attracting our attention, till its consequences are such as often defy the most assiduous application of our art. If such be the case, it is evident that the error must be deeply seated. It can have no other foundation than a mistaken view of some of the fundamental laws of our constitution.

It will appear, I think, from the facts to which I shall refer, that we are constantly meeting with cases in which the fatal result might have easily been averted, had we been more early aware of the nature of the change which had for a long time been imperceptibly, but with certainty, tending to such a termination.

I shall, in the first place, inquire what errors have existed in our views of the animal economy which may be supposed capable of such effects; then enumerate the proofs that such have been their effects; and in the third place, point out the means of detecting them before they have arrived at their necessarily fatal stage; concluding with a statement of the general principles of the treatment by which their progress may be arrested.

ALL the facts to which I am about to refer, as well as the manner in which they necessarily influence our views respecting the more important vital

functions have, in different publications*, been laid before the public from time to time, as it appeared to me that any addition had been made worthy of publication; but those relating more particularly to the subject of this paper have never been selected and reviewed in that order which is necessary to place the practical inferences from them in a sufficiently connected point of view.

I mean the present and following papers to answer the same purpose respecting the practical department of the subject, which is answered respecting the physiological, on which the practical department is in a great degree founded, by the last of the papers just referred to, in which the Royal Society, although in opposition to their usual proceedings, did me the honour to republish various statements which had, during more than twenty years, from time to time appeared in their Transactions, while the inquiry was going on; in order to place, in one point of view, the whole of the facts, and thus enable us to arrive at the conclusions which necessarily result from them, when an opportunity is afforded of comparing them together.

The physiological part of the subject was laid before the Royal Society, because physiology forms a branch of general science; and although one of the foundations on which practical medicine rests, does not, like the subject of the present paper, exclusively belong to our profession.

The physiological object I had in view, namely, to remove the various contradictions which, in some of the most essential points, obscured our knowledge of the general laws of the animal economy having, in the manner pointed out in the paper just referred to, as far as I am capable of judging, been accomplished; it only remains for me to pre-

* Various passages in twelve papers, published in the Philosophical Transactions in 1815, and succeeding years, particularly in the papers published in those Transactions for the years 1833, 1834, and 1836—the two first, on the Nature of Sleep and Death, and the last being a general review of the Functions of Life, with the inferences which necessarily arise from it: my Experimental Inquiry into the Laws of the Vital Functions, 3d edition; the Introduction to my Treatise on Fevers and Inflammations, 4th edition; my Treatise on Indigestion, 7th edition; that on the Means of Preserving Health; that on the Effects of minute doses of Mercury in restoring the Functions of Health; my Gulstonian Lectures, delivered before the College of Physicians in 1835, &c.

sent, in a connected form, the practical inferences which necessarily arise from a correction of the physiological errors; and point out how strikingly these inferences correspond with what we are constantly in the habit of observing in actual practice; for to the same inferences, we shall find, we are equally led by correct physiological views, the phenomena of disease, and the effects of our remedies.

It will be generally admitted that, except as far as it depends on the simple principle of employing the means, more frequently suggested by accident than reflection, which had been useful in similar cases; the practical part of our profession rests wholly on our knowledge of anatomy and physiology, a knowledge of the structure and functions of the animal body.

From the first of these sources (independent as it is of previous knowledge, and exercising only the simplest faculties of the mind) all practical knowledge of the healing art possessed in the rudest state of society is derived. As the limited nature and uncertainty of such a source appear, we are led to the means of assisting it.

We by degrees perceive the necessity of distinguishing the local and general, and the more immediate and remote, effects of our remedies; and soon find that to make any considerable progress in such investigations, some knowledge of the structure and functions of the subject of our practice is necessary; and that in proportion as this knowledge is acquired, our means of relief become more effective.

When I entered on the investigations just referred to, all our doctrines, as far as respects the general laws of our frame, were involved in a confusion which none of the facts, of which we were in possession, enabled us to unravel. I therefore began by repeating many of the experiments of the physiologists who had for their object the elucidation of those laws, at the head of whom I found Haller, Le Gallois, and Hunter. I found the results of their experiments such as they state them to be; but the inconsistency of their inferences left no room to doubt that some of them are fallacious; which will not surprise us when we consider the state in which they found the subject; for any thing that had been done re-

specting the general laws of the animal economy before their time was comparatively of little account, being in most instances either so evident as not to require the aid of experiment, or erroneous.

It had, however, the merit of being to a certain degree consistent—a consistency not difficult of attainment, because, as the test of direct experiment had been little resorted to, it was easy to make any supposition which was necessary towards attaining it; but after the time of Haller, who may be regarded as among the first of those physiologists who applied with any accuracy to physiological inquiries the principles of inductive reasoning, the dreams of the older physiologists began to share the fate they so justly deserved, and the consistency of our systems immediately began to suffer.

The few facts at first ascertained were stubborn things, and would not conform to the loose and fanciful theories which had prevailed. Thus difficulties arose, and, even so lately as at the time my labours commenced, prevailed to so great a degree, and caused such confusion in our doctrines respecting the general laws of our frame, that some despaired of seeing it effectually removed.

If, as appeared from the experiments of Haller, the power of the heart is independent of the nervous system, and this organ is incapable of being influenced through its nerves; how comes it to be subject to the passions? and how comes it, as M. Le Gallois proved, that its power may be destroyed by crushing the spinal marrow? Whence does it arise, as appears from the experiments of the latter, that the sudden destruction of a part of the spinal marrow impairs, and may even destroy, the function of the heart, while the destruction of the same part of the former organ by small portions, in no sensible degree affects its power? How comes it that the affections of the mind so powerfully influence the heart, when the total removal of the brain, as proved by M. Le Gallois, does not even in the slightest degree influence it? Why, if the power of the heart depends on the spinal marrow, as it appears to do from the experiments of M. Le Gallois, the accuracy of which I have ascertained by repeated trials, have fetuses been born alive, where no spinal mar-

row had ever existed? or why, if the power of the heart depends on the spinal marrow, does it continue to perform its usual motions after it is removed from the body? Why, if, as M. Le Gallois maintained, all vital organs bear the same relation to the nervous system, is the function of the heart uninfluenced by dividing the eighth pair of nerves, or even by decapitation, and that of the stomach and lungs impaired by dividing, or even by throwing ligatures round, these nerves? Why do the motions of respiration cease on the destruction of the medulla oblongata, since the nerves of the muscles employed in respiration arise from the spinal marrow, which M. Le Gallois proved by experiments, the accuracy of which cannot be questioned, to be capable of exciting the muscles which derive their nerves from it, independently of that and all other parts of the brain? M. Le Gallois points out this inconsistency, and admits that he sees no means of removing it.

However accurately detailed the results of the experiments, such contradictions left no room to doubt the inaccuracy of some of the inferences. It was evident that while many facts of importance had been discovered, others of a leading nature must have been overlooked, the discovery of which alone would remove the foregoing inconsistencies, and afford any useful knowledge of the general laws of our frame.

It was with this view that the investigations in which I have so long been engaged were undertaken. As the object of the present paper is wholly of a practical nature, I shall not enter into physiological discussions, but merely point out the manner in which the results of my experiments have effected their object, referring to the *Philosophical Transactions*, and my *Experimental Inquiry into the Laws of the Vital Functions*, for the detail and application of the experiments.

The following are the positions ascertained by the investigations referred to, by which the difficulties above detailed are removed, and the general laws of the animal functions reduced to that simplicity which is always observed in the works of nature, when a correct knowledge of them is attained:—

It is true that the heart, as ascertained by Haller, cannot be influenced through

its nerves in the same way as the muscles of voluntary motion; not from the heart being placed beyond the influence of the nervous system, as he supposed, but from that influence being conveyed through nerves of different properties from those by which it is conveyed to the muscles of voluntary motion; for in our frame, beside the nerves of sensation and those of motion, there is a third set—the ganglionic nerves, the functions of which we shall find are as different from those of both these sets of nerves, as their functions are from those of each other*.

It is true that in destroying the spinal marrow, the effect on the heart is so essentially influenced by the manner in which this is done, that its function may either be instantly destroyed, or, as far as we see, in no degree affected by it. But this arises not from the cause to which it is ascribed by M. Le Gallois—a cause which has no existence†—but we shall find is a consequence of the heart being placed under the influence of every part of the spinal marrow, from that by which it is united to the brain, to its extremity at the lower end of the spine‡.

The heart, we find, is uninfluenced even by the removal of the whole of the spinal marrow, because, in direct opposition to the inference of M. Le Gallois, its powers are wholly independent of this organ; but while the nervous connexions are entire, the heart is placed under the influence of every part of it. The same observations apply to the brain: its influence on the heart is not confined to any particular part, but extends to every part of it. The heart may be influenced through whatever part of either the brain, properly so called, cerebellum, or spinal marrow, provided the agent operate on a considerable portion of any of these organs; for if confined to a very minute portion, it makes no sensible impression on the heart§, which, being influenced by every part of the former organs, feels not, in an observable degree, the effect of an impression made on each minute portion of them. Hence it is that the sudden destruction of any considerable

* *Philosophical Transactions* for 1829 and 1836.

† *Experimental Inquiry into the Laws of the Vital Functions*, Part I. When this Inquiry is referred to, it is always to the third edition.

‡ *Philosophical Transactions* for 1829 and 1836; and *Inquiry into the Laws of the Vital Functions*, Part II.

§ *Inquiry*, &c. Part II.

portion of either may destroy the function of the heart, while its slow destruction—that is, by a succession of minute parts—may not at all sensibly affect it. From the whole of the facts here referred to, it arises that the heart is uninfluenced by the removal of either the brain or spinal marrow, or even of both at the same time; but while the nervous connexions remain entire, it may, through any considerable part either of the brain or spinal marrow, be excited, and that to any degree of which it is capable, or wholly deprived of power, according to the nature of the cause operating on a considerable portion of those organs, and the extent of the part subjected to its immediate operation.

We now see why, although the heart is subjected equally to the power of the brain and spinal marrow, its vigour may have been perfect where neither organ had ever existed; for fœtuses have been born well grown, and in other respects of the usual size, without either brain or spinal marrow, an instance of which I have witnessed*; the power of the heart, though influenced by, being wholly independent of, both. Thus also it is still capable, as we have seen, of performing its usual motions after it is removed from the body. All the preceding observations apply to the vessels as well as to the heart. The motion of the blood in the vessels, with the exception of the arteries, in which it feels the impulse given by the heart—that is, in the capillaries and veins—depends on their own powers alone, being wholly independent of the heart; and the relation of their powers to the brain and spinal marrow is in all respects the same as that of the heart itself.†

The different vital organs are differently affected by being deprived of the influence of the brain and spinal marrow, because the nature of the functions performed by that influence in them is wholly different. The brain

and spinal marrow, we have just seen, bestow no power on the heart and blood-vessels: they both derive their power from the muscular fibre; a power, whether with respect to the muscles of voluntary or involuntary motion, wholly independent of, though subjected to, the influence, of both the brain and spinal marrow‡; while the powers of the other vital organs—the stomach and lungs, for example—except as far as they depend on the mere muscular power, are wholly dependent on the brain and spinal marrow†. The stomach will never secrete a particle of gastric juice after it is deprived of the influence prepared by the brain and spinal marrow, nay, after it is deprived of that of either; and will not secrete a perfect gastric juice if deprived of the influence of any considerable portion of either; and the same observations apply to the chemical changes which take place in the lungs‡: nor is this all, for if a considerable portion of the influence of either be withdrawn, the structure as well as function of all secreting organs will sooner or later, according to the degree in which that influence fails, be impaired§; and if the cause of failure be very powerful, or of long continuance, wholly destroyed||.

It thus appears that the brain and spinal marrow supply an influence on which not only the power of secretion but the whole of the assimilating powers depend.

Through what channel is this influence, on which the functions of life so immediately depend, conveyed?

Anatomy, with the aid of the physiological experiments on which this paper is founded, affords a ready reply. The ganglionic nerves, according to the sense in which I have used this term¶,—that is, the nerves which either enter or send branches which enter those ganglions which receive nerves from different sources, and the nerves which proceed from such ganglions. These may justly be termed the nerves

* From such instances, compared with the facts to which I am about to refer, we have reason to believe that the vital organs of the brain and spinal marrow, like the lungs, are inactive in the fœtal state; and that their functions in this state, like the functions of the lungs, are effected by the powers of the mother.

† My paper on the Sources and Nature of the Powers of Circulation, in the Phil. Trans. for 1831. It occasionally, though rarely, happens, that the impulse of the heart extends to the capillaries, but this is always indicative of a morbid state of the vessels.

* My paper on the Functions of the Nervous System, in the Phil. Trans. for 1829; and the 2d Part of my Inquiry into the Laws of the Vital Functions.

† Ibid. and two papers in the Philosophical Transactions for 1815, and one in 1822.

‡ Ibid.

§ Ibid. and Philosophical Transactions for 1827.

|| Ibid.

¶ Philosophical Transactions for 1829.

of life, for on the influence they convey the formation and maintenance of all our organs depend.

It cannot be without surprise that we learn that, in the present advanced state of medical science, the functions of these nerves, as far as life is concerned the most important of our frame, should have remained unknown; those of the nervous system, properly so called, having been supposed to be confined to the functions by which it administers to the sensorial powers. Such alone are those ascribed to it even in our latest and best accounts of the present state of the profession*. As we have remained in ignorance of the functions of the vital organs of the brain and spinal marrow, and indeed of the existence of such organs, and consequently of the influence prepared by them, and conveyed by the ganglionic nerves; that the failure or other morbid states of that influence should have been overlooked, is only a necessary consequence. It is on this, we shall find, is founded the position stated in the commencement of this paper; which, however unlooked for it may appear, is not the less correct—namely, that a cause of many of the most formidable diseases to which we are subject, is in constant operation around us, without attracting our attention till its consequences are such as too often defy the most assiduous application of our art.

Only one of the inconsistencies above stated remains to be considered, which has its origin in no serious attempt having been made to draw the line of distinction between the sensitive and vital functions—namely, that pointed out by M. Le Gallois, relating to the function of respiration, which it appears from what is said in my papers published in the *Philosophical Transactions* for 1833, 1834, and 1836, exercises so extensive an influence on all the other functions of the system, both in health and disease. It is true that the muscles employed in the act of respiration derive their nerves from the spinal

marrow, and that this organ is capable of its immediate functions, independently of the brain, among which the excitement of the muscles of respiration is one; but respiration is the most complicated of all our functions. In it many other functions conspire with this function of the spinal marrow. It appears from what is said in a paper published in the *Philosophical Transactions* for 1829, on the Functions of the Nervous System, and re-published in my treatise on the Nature of Sleep and Death, that among these functions is one of the sensorial power, which is destroyed by injuries of the medulla oblongata; and the failure of which as unavoidably interrupts the act of respiration, as the failure of the muscular or nervous powers on which the motions essential to that function depend. Thus it is that injury done to this part of the brain, however unimpaired the powers of the spinal marrow, immediately destroys respiration.

Such are the means by which all the contradictions which clogged our knowledge of the general laws of our frame were removed by the long-continued investigations in which I have been engaged. We are now prepared to review the practical inferences which necessarily flow from that knowledge, which we shall, in what remains to be said, find equally confirmed by the principles on which the functions of our bodies depend, the phenomena of the diseases in question, and the effects of our remedies.

It appears from the facts recapitulated in the paper which the Royal Society did me the honour of publishing in their *Transactions* for last year, that the powers of the nervous system, properly so called, perform but a subordinate part in the sensorial functions; they only afford the means of conveying to the sensorial organs the impressions which excite them, and to the muscles of voluntary motion the dictates of the will; while in the vital system they afford the leading power, that to which all its other powers are subordinate, yet it is in this system that the powers of the brain, spinal marrow, and nerves, have been overlooked. Can we be surprised, then, at finding that many diseases having their origin in the affections of these powers, should in their early stages be obscure; an evil increased in consequence

* See in the Report of the British Association for the Advancement of Science for 1833, a paper by Dr. Henry, of Manchester; and a Dissertation on the state of Medical Science from the termination of the 18th century to the present time, by Dr. Allison, Professor of the Institutes of Medicine in the University of Edinburgh, in the *Cyclopædia of Practical Medicine*, published in 1834.

of many of the vital organs being ill-supplied with nerves of sensation. Such, it will appear from facts I am about to state, is the source of a change which in many instances proceeds with few well-marked symptoms, till it at length produces effects that too often defy all our remedies.

One other point necessary to a clear understanding of the practical department still remains to be considered, before I conclude that part of the subject which relates to the laws of our frame.

As no serious attempt had been made to draw a correct line of distinction between the immediate effects of the sensorial and nervous powers, in like manner there had been no serious attempt to draw the line of distinction between the powers of which the animal body partakes, in common with inanimate nature, and those powers which are peculiar to itself, and we shall find that it is of no small consequence in our practical inferences, that it appears from the facts stated in the paper just referred to, that while the sensorial and muscular powers, and the powers of the living blood, are peculiar to the living animal; the nervous power, properly so called, the leading power in the vital functions, that power to which all their other powers, if we except the principle of vitality itself, are subservient, is in its general nature identical with one of those powers which operate in inanimate nature, it having been proved that all the functions of that power can be effected by voltaic electricity, made to operate under the same circumstances under which it operates*.

Such, indeed, has been the confusion which has prevailed on this part of the subject, that the nervous influence and vital principle have been confounded and regarded as one and the same, an error from which even Hunter is not exempt; although, of so different a nature are these powers, that the latter has no existence except in the living animal, while of the former it partakes in common with inanimate nature.

The vital principle belongs exclusively to no particular set of organs, but equally animates all living parts, the organs of the sensorial and muscular powers and the blood, as well as those

of the nervous power, properly so called. Is not the vital principle that which bestows on all living parts the properties which distinguish them from inanimate matter; while the nervous influence is an agent prepared only by a certain set of those parts, for the preparation of which both their own peculiar properties and their endowment with that principle are equally indispensable? How confused, then, have been our views of the animal economy, when such a physiologist as Hunter could have confounded with the general vivifying principle an agent, the existence of which in the living animal depends exclusively on one particular set of its organs, and is itself one of those powers of which the living animal partakes in common with inanimate nature! The vital principle might, even with more propriety, have been confounded with the muscular power, for wholly different as the properties of these powers are, the muscular power is at least one of those peculiar to the living animal.

The truth is, that the nature of the nervous influence having never been seriously made the subject of experiment, has been regarded as of so mysterious a nature, that it has been open to any fanciful properties which might be ascribed to it.

If there be any truth in the experiments publicly repeated both in London* and Paris†, and that on a great variety of animals, with the same results; the nervous influence, properly so called, that is in opposition to the powers of the sensorial organs, is, of all the powers of our frame, that which is best defined, and with which we are most familiar.

All its effects in the living animal, like those of electricity in the inanimate world, are chemical processes‡. This is strikingly the case with respect to the only function of the vital organs of the brain and spinal marrow, to which I have not already had occasion to refer. It is many years since Sir Ben-

* See the Philosophical Transactions, and the journals of the Royal Institution, both for the year 1822.

† De l'Influence du Système Nerveux sur la Digestion Stomacale; par MM. Breschet, D.M.P., Chef de Travaux Anatomiques de la Faculté de Médecine de Paris, etc.; H. Milne-Edwards, D.M.P.; et Vasseur, D.M.P. (Mémoire lu à la Société Philomatique, le 2d Août, 1824.) Extrait des Archives Générales de Médecine, Août 1823.

jamin Brodie* proved experimentally that the process which maintains animal temperature depends on the powers of the nervous system; and nearly as long since I found, by the same means, that the influence on which this process depends, is equally supplied by the brain and spinal marrow†: and that here, as in the other effects of the nervous influence, voltaic electricity is capable of its function. The temperature of living arterial blood was immediately raised several degrees by voltaic electricity, while no effect of this kind could be produced by it on living venous blood, which had undergone the action of the nervous influence.

In my next communication I shall give the first part of the practical inferences depending on the facts referred to in the present paper, and confirmed by the practical experience of thirty years. Many of the results I am about to lay before the reader were rather inferences from practical observations than the results of an application of the foregoing statements to the phenomena of disease; with many of which statements I was in the earlier parts of the above-mentioned period unacquainted. Thus actual practice and physiological experiment have mutually assisted each other in arriving at the practical results, and consequently have each tended to confirm the inferences from the other.

I am, sir,

Your obedient servant,

A. P. W. PHILIP.

Camden-Square,
Nov. 6, 1837.

SOME ACCOUNT
OF
THE SURGICAL CASES
WHICH WERE ADMITTED INTO
THE LIVERPOOL NORTHERN
HOSPITAL,

From the 1st Sept. 1836, to the 1st Sept. 1837.

To the Editor of the Medical Gazette.

SIR,

You have given insertion, in the MEDICAL GAZETTE, to two annual reports of

* Sir Benjamin Brodie's Papers in the Philosophical Transactions for 1813 and 1814.

† The first edition of my Experimental Inquiry into the Laws of the Vital Functions, published in 1817.

the surgical practice of the Liverpool Northern Hospital; will you oblige me by admitting the third annual list of the surgical cases which have been received into the hospital during the last year, viz. from the 1st of September, 1836, to the 1st of September, 1837?

I am only able to give a relation of some few of the most interesting cases which have come under my own care. The hospital now contains so many patients, that it would be difficult to take notes of the interesting cases of my colleagues as well as of my own; I must therefore leave to them the report of the cases which have come under their care. The following list, however, is of all the surgical cases which have been admitted into the hospital during the past year.—I am, sir,

Your obedient servant,

JOHN M. BANNER.

Liverpool, Oct. 10, 1837.

Inflammation of the interior of large joints	15
Abscesses in the knee-joint.....	1
Ulceration in interior of large joints	11
Dislocations	18
Contusions of joints	31
Wound in the knee joint	1
Common inflammation (cases admitted with).....	6
Abscess.....	9
Wounds—incised	52
punctured	9
lacerated	23
of arteries	3
Cut throat	2
Burns	18
Scalds	4
Contusions	48
Concussion of the brain.....	14
Compression of the brain	5
Fractures—simple	166
compound.....	36
Inflammation of the periosteum	4
Necrosis	2
Ulcers—ill-conditioned	41
Gangrena senilis	1
humidus	6
Iritis	4
Adhesion of palpebræ to cornea	1
Sloughing of the cornea.....	2
Ulceration of the cornea	1
Veins—inflammation of.....	2
varicose	3
Hernia	1
Hydrocele.....	3
Ulceration of rectum	4
Recto-vesical fistula, with calculus in the prostate	1
Testicles, disease of	4
Retention of urine	6

with effusion of blood. In one there was fracture of the parietal bone, with depression and laceration of the membranes; and in the fifth case there was a fracture extending across the skull, dividing it into hemispheres, and also rupture of the meningeal artery: in this case, symptoms of compression came on gradually. On examining the head after death, a large coagulum was found situated on the left side, between the skull and dura mater.

There were three cases of abscess *in perineo*; in two of these there had existed stricture of the urethra. The third arose from an injury.

There were four cases of stone in the bladder: three of them were operated on; the lateral operation was performed in two, and the recto-vesical operation in the third. In the fourth case, which occurred in a female, there had been a quantity of calcareous matter passed by the urethra for a very long time, and the patient continued to pass large quantities: it came away in thin broad pieces, which were rough, and perforated in many places. The symptoms which were present indicated disease in the kidney; consequently no means for the removal of the calculi in the bladder were resorted to.

The following are a few cases which I have selected from my note-book. The cases were taken down at the bedsides of the patients, and consequently are not drawn up in the best manner; they are, however, correct, and as such may be of some value.

CASE I. — Compound Dislocation of both Ankles—Comminuted Fracture of the Bones of the Feet—Amputation of both Legs—Death.

Thomas Thomas, æt. 41, was admitted into the hospital with compound dislocation of both ankles. He is of a full habit, and has generally had good health. He is a well-sinker. On the evening of the 3d of September, 1836, whilst descending in a bucket into a well 26 yards deep, the bucket turned over, and he was precipitated at least 17 yards. He was insensible for some time after the fall, and the first thing he remembers after it is, that two of his companions were standing by him in the well. He was conveyed as soon as possible to the hospital.

There was a large wound extending from the outer ankle of the right leg to

the centre of the sole of the foot. There was considerable displacement, the opening extended into the joint, and the bones of the foot were much comminuted.

There is a similar wound on the left foot, with this difference, that it extended more over the front of the foot; there was much displacement here, and considerable hæmorrhage from the wound, which extended into the joint, and there was comminuted fracture of the bones of the foot.

At the time of admission, the patient was faint, and it was some time before he rallied sufficiently to bear the operation of amputation, which had been determined on. The left leg was first removed (the one from which there was hæmorrhage). He bore the operation very badly, and was several hours before he rallied; in consequence of this, we determined to defer the second operation, which was not performed for sixteen hours after the first. This he bore much better than the first, and appeared to be doing well until the middle of the night (fourteen hours after the second amputation), when vomiting of bilious matter came on, which continued incessant; the pulse was quick and weak, and he complained of great languor. Mr. Blower, the house-surgeon, prescribed effervescing saline draughts, with small doses of opium, every three hours, and administered brandy often during the night.

5th. — The patient is restless; the countenance anxious; he has passed a sleepless night; the pulse is 130, and very weak; the tongue is dry; the vomiting is constant; the fluid ejected is much tinged with bile. The left stump was examined; it was quite cold, and the muscles were of a greenish colour, and on being touched did not convey any sensation to the patient.

R Pulv. Opii, gr. j.; Hyd. Submur. gr. ij. M. f. pulvis, 2d quaque horâ sumendus. Brandy to be given often. The stump to be fomented.

He gradually became weaker, and in the night of the 5th he died.

Examination of the Feet.—The left foot: the os calcis was found literally smashed, and the internal cuneiform bone was also much broken. A fracture also extended some distance into the astragalus; the capsular ligament was much lacerated.

The right foot: the os calcis was

much comminuted, as were also the cuboid and cuneiform bones. In this foot the astragalus had escaped, but the fibula was fractured, and there was considerable laceration of the ligaments.

Post-mortem Examination.—On opening the abdomen there was seen about two drachm of thick yellow fluid, resembling bile: this fluid was lodged between the gall-bladder and colon, to which it (the bladder) firmly adhered. On pressing the gall-bladder there did not appear any escape of bile into the abdominal cavity, nor could there be found any laceration of the liver; which was white, hard, and very adherent to the surrounding peritoneum. There were several ecchymosed spots on the intestines, and one (very large) on the stomach. The peritoneum over the right kidney was very dark, and beneath it was a considerable quantity of effused blood. The substance of the kidney appeared bruised; the inner surface of each os ileum was also ecchymosed. On examining the chest, there was an effusion of blood in each side of the thorax; the right side contained about eight ounces, the left four. The lungs were very dark; they crepitated, and floated well in water. There was a very dark place under the pleura, covering the fourth, fifth, and sixth ribs of the right side, near their junction with the vertebrae: these ribs were fractured, and a little above this part, at the root of the lungs, it was thought blood exuded through a very small opening; this, however, was very doubtful. The heart was large, and so adherent to the pericardium at every point, as to make their separation difficult. The inner surface of the aorta presented a bright florid appearance (such as is sometimes seen in cases of sudden death).

CASE II.—*Recto-Vesical Fistula—Calculus in the Prostate—Operation—Cure.*

John Green, æt. 11, was admitted into the hospital on the 8th of November, 1836. He is a healthy-looking boy, stout, and very short in stature. The grandmother, who accompanied the patient, gives the following statement:—"Rather more than three years ago, we first perceived that there was frequent desire to void the urine, and that while he was doing so he would frequently cry, as if in pain. We ascribed this to two or three accidents which had pre-

viously happened to him; for instance, on one occasion, whilst in the act of voiding his urine, a boy seized Green by the penis, and pulled it so unmercifully that inflammation and swelling came on, which continued a long time. About six weeks after this, he fell across a large piece of wood, and again produced inflammation and swelling of the penis. It was about five or six months after this that we first noticed the difficulty in making water; this appeared to increase upon him, and his rest was so much disturbed that we determined to obtain medical aid: for this purpose we took the patient to the neighbouring town (in Scotland), and had the opinion of two surgeons. They passed an instrument into the bladder, and felt a stone; they proposed that he should undergo an operation, which was objected to. The symptoms after this became gradually worse, and he often parted with blood in his urine, which now came away in drops. It was about a year after we first perceived the difficulty in voiding the urine, that he parted with a small calculus; it was nearly white, and about the size of a large pea, though much longer. After this stone came away he appeared much easier, and for ten or twelve months was comparatively free from pain, and the urine flowed much more freely. It was about a year ago that he became again very unwell; he was feverish, and constantly parting with drops of urine, mixed with blood and mucus. These symptoms continued for five or six weeks, when suddenly he felt relief: this arose from the urine coming away from the seat, and in three days after we first perceived this change, he had considerable difficulty in parting with his stool, and after straining much, he parted with a large stone. After parting with this stone he was easy, and, with the exception of the urine coming the wrong way, we should think there was not much the matter."

My friend, Dr. Mackenzie, had seen this patient in Scotland, and had obtained possession of the calculus which had been passed by the rectum. It is of the size of a large walnut, and weighs half an ounce (within twenty grains): it is composed principally of phosphate of lime.

A sound was passed into the bladder, which was found very much contracted. A calculus was felt, though very indis-

tinety; a roughness, as if the sound passed over the surface of a stone, was perceptible, but this could only be felt by drawing the instrument considerably out of the bladder, so that its convexity came in contact with the prostate gland.

On passing the finger far into the rectum, an opening sufficiently large to admit the point of it was felt; but the finger could not be got in contact with the bare instrument; it appeared to get into a cavity between the bladder and rectum, and, with the exception of one occasion, the bare sound could not be felt; in that instance, by endeavouring to strike the calculus with the sound, the instrument passed very much further than heretofore: this led me to pass my finger into the rectum, when I found the end of the sound protruding some distance into the bowel. The patient stated, that occasionally he parts with drops of urine through the urethra. I appointed a person to watch him, and orders were given to keep the urine.

The report was, that the patient had occasion to go to stool every ten or twelve minutes, and appeared to part with a small splash of water through the rectum; that he had not parted with any urine by the natural passage. The fluid he had parted with was about a pint in quantity, and was mixed with faecal matter. On questioning the patient, it appears that he never parts with formed stools, but always water by the rectum; and this has been the case ever since the bladder and rectum had ulcerated.

On passing a catheter into the bladder, on one occasion, a very small quantity of fluid came through it, which was faeculent, and had very much the appearance of that which comes through the rectum.

This patient remained in the hospital until May 1837, before a decisive opinion was formed as to what ought to be done. This arose, in a great measure, from the difficulty we had in feeling the stone. It was not until after many soundings that we were unanimous as to the existence of a calculus. Then another consideration was, the excellent health Green was then in; for with the exception of the inconvenience of having to part with the urine by the rectum, he was in good health and spirits.

On the 10th of May, however, he was attacked with febrile symptoms, and

parted with considerable quantities of mucus and blood by the rectum, and occasionally blood by the urethra. In consequence it was determined, as soon as these symptoms subsided, that an operation should be performed for the purpose of removing the stone, which we conceived was producing irritation by its presence.

On the 25th May the symptoms were much relieved, and the operation was performed as follows:—

Weiss's speculum ani was introduced into the rectum, after the patient had been secured as for the lateral operation; previous to which the straight staff was introduced into the bladder. An incision was made in a straight line, commencing from the prostate gland, and was continued to the point of the staff, when it was found that an incision of little more than an inch could be made, in consequence of the contracted state of the bladder. The straight staff was now withdrawn, and a curved one introduced, which enabled me to enlarge the incision about half an inch. On passing my finger through the opening into the bladder, I felt the stone firmly imbedded in the prostate gland. In consequence I enlarged the wound anteriorly, and with the aid of the forceps removed the stone. The calculus was firmly situated, and required slight exertion to disengage it. On introducing the finger into the wound after the stone was removed, the bed in which it had been contained felt rough, and there was evidently an incrustation of calcareous deposit attached to the cyst. The attachment was so firm, that an attempt to remove it would have been unjustifiable. The calculus weighs forty-eight grains, and its composition is the same as the stone which had been passed through the rectum.

June 1st.—The patient has not had an unfavourable symptom since the operation. The bowels have acted regularly; the whole of the urine has passed through the rectum; this morning a small quantity of the urine passed through the urethra.

4th.—He has passed his urine with greater freedom through the urethra; he is free from pain; the stools have contained more solid faecal matter.

7th.—He has had a slight febrile attack, with sore throat. By the use of leeches and saline purgatives he is better.

9th.—He has suffered a good deal from tenesmus, and is more feverish; complains of great thirst; yesterday he parted with two or three small pieces of calculus through the urethra; he continues to pass the greater part of the urine through the natural passage.

10th.—He has parted with a substance from the rectum; it has the appearance of a piece of thickened mucous membrane, covered with an earthy deposit; it is at least an inch and a half square. After passing this he became quite easy.

19th.—He continues to go on well; the principal part of the urine passes through the urethra; a large quantity yet passes through the rectum. The health is good.

27th.—He appears well in health, and states that all the urine passed through the urethra yesterday and today. Mr. Arnold, the house-surgeon, saw him pass urine this morning; it came away in a full and good stream.

July 1st.—He states this morning that he thought he had parted with a small quantity of urine by the rectum; but that he now has occasion only three or four times a day to part with his water, and once in the night, and that it always comes away with great ease, and in a full stream. I passed my finger into the rectum; the opening which I formerly felt from that gut towards the bladder was very much contracted, but was still there.

4th.—He was made an out-patient.

18th.—He attended at the hospital; he was in good health, and continues to pass the urine through the urethra in a good stream, and can retain it in the bladder a long time.

Oct. 7th.—Green called to see me today; he continues quite well; he now goes to school, and seldom requires to pass his urine between eight in the morning and twelve o'clock at noon; and seldom parts with it more frequently than three or four times in the day. The stools are solid, and he parts with them once, sometimes twice, a day.

May we not conclude, in this case, that the calculus which was situated in the prostate had prevented the flow of urine through the urethra? The opening in the bladder, caused by ulceration, could never be correctly made out; for notwithstanding on one occasion the sound passed from the bladder into the rectum, it was what I never could effect

again. There appeared to be a cavity between the bladder and rectum, for I could readily get my finger through the opening into the bowel, but could on no occasion feel the staff or sound which was in the bladder; there always appeared an intervening substance, like the side of the bladder. The opening in the bladder and the one into the rectum were certainly not opposite to each other. On one or two occasions, on passing my finger through the opening from the rectum, a quantity of fluid appeared to escape past the sides of my finger, but in no instance could I feel the bare sound in the cavity my finger entered; there always appeared to be the bladder between them. It was on the sixth day after the operation that the patient passed urine through the urethra; and although it was a very small quantity, yet it proves that there was a power left in the bladder to propel the urine through the natural passage, which would probably have taken place much earlier if the stone had not occupied the situation it did in the prostate. I conceive that the calculus in the prostate must have existed at the time the large stone passed from the bladder into the rectum, and might possibly have been in some measure a cause of the great irritation which the boy suffered in the commencement of his complaint.

CASE III.—*Fracture of the Coracoid Process of the Scapula.*

Alexander McCulloch, æt. 40, fell down a long flight of stairs, and alighted on his shoulder; he suffered very great pain, and was unable to move the arm. He went to a *bone-setter*, who told him that his shoulder was out of joint. He had considerable extension made by several men, and at last the bone-setter said "that he had put it in." Nevertheless the pain continued, and the inability to move the arm became greater. In consequence he made application for admission into the Northern Hospital.

Aug. 31st.—It is five days since the accident. The patient is a strong thick-set man, in full health. The shoulder and arm are considerably swelled, and he complains of numbness in the fingers. He suffers much on the arm being moved; the head of the humerus is in its proper position. A hard protuberance is felt in the axilla, which we ascertained

to be the coracoid process, and a small portion of the glenoid cavity of the scapula. The inflammatory symptoms which were present yielded under the antiphlogistic treatment. The parts were kept in as near a state of apposition as they could be placed in, and were continued so for several weeks. There was not any improvement; the pain continued great, the arm swelled, the numbness remained, and the process continued quite loose.

He was now ordered to use the arm gently, and passive motion was prescribed. By this treatment the parts appeared to adapt themselves to the case, and in two months he was able to use the arm in certain positions; he could never, however, raise it to a straight line. At the time he left the hospital the numbness had become much less, the swelling had nearly subsided, and he had a tolerable use in the arm. When I saw him in December, he was able to do the work of a warehouseman. The process continues moveable.

CASE IV. — Compound Fracture of the Skull, with Depression — Bone removed—Death.

John Fell, æt. 24, was admitted into the hospital on the 27th of September, 1836. He has always been in good health; he is a sailor, and has led a temperate life. It appears that a piece of iron, upwards of 9 lbs. in weight, fell a distance of seven yards on to his head while he was discharging a cargo from his ship. He was insensible at the time, but very soon recovered his recollection. He walked to the hospital from the dock, a considerable distance, supported by two men. The accident happened at noon, and he was admitted into the hospital soon afterwards; he was quite sensible at this time, and described accurately the manner in which the accident happened. There had been a considerable hæmorrhage, and the blood continued to flow freely; there is a wound $3\frac{1}{2}$ inches in extent over the left parietal bone; on making an examination, a fracture of this bone was felt extending in length $2\frac{1}{2}$ inches, and in breadth one inch; there were two pieces of bone driven in, and were resting on the dura mater. The hæmorrhage came from within the skull; there was a considerable quantity of brain under the scalp. It was an hour and a half after the man's admission before I visited him;

he was then quite sensible, the hæmorrhage had ceased, the two pieces of bone were quite loose; they were resting on the dura mater, and so covered with bruised brain, that they appeared driven into that organ; they were very readily removed; a coagulum was also removed from between the bone and dura mater; there was about half a teaspoonful of brain adhering to the loose portion of skull. In the operation of removing the bone there was some hæmorrhage from the meningeal artery: the patient was removed to bed.

8 P.M.—The patient is quiet and free from pain; the pulse is at 84, and rather weak; he has slept a little; cold lotion is applied to the head; he takes saline purgatives with tartarized antimony.

Sept. 29th.—He has had a good deal of sleep, at intervals, during the night; the face looks flushed, and he complains of slight pain in the head. The pulse is full, hard, and quick; the tongue is white and moist; the wound looks healthy.

V. S. ad xxx .

Taking away this quantity of blood caused syncope. The pain in the head was not relieved; the pupils are contracted.

1 P.M.—The pain in the head is very great; there is restlessness; he talks incoherently, and refuses to answer questions; the pulse is quick, but not full.

V. S. ad xxij .

when syncope again come on: eighteen leeches to be applied to the temples. Cold continued to the head. Turpentine enema to be administered every three or four hours, and a blister to be applied between the shoulders.

11 P.M.—The symptoms are aggravated; he is more delirious; the pupils are dilated, but act to the stimulus of light; the breathing is more difficult; he has constant throwing about of the limbs; the pulse has risen in strength, and is frequent; the bowels have been well relieved.

V. S. ad xxiv .

He fainted after this bleeding, and for a time appeared easier; the blister is beginning to rise; to continue the enema.

20th.—The breathing is more laborious; the pupils have ceased to act; the right pupil is much more dilated than the left; the pulse is quick and firm. Blood has been taken twice from the temporal arteries; after each bleed-

ing he fainted. The stertor became greater, and in the evening he died.

Post-mortem Examination.—On removing the skull cap, there was seen a laceration of the dura mater, two inches in length. The membranes of the brain were in a high state of inflammation; the whole upper surface of the left hemisphere was covered with coagulating lymph; there was a wound in the brain, under that, in the dura mater: the substance of the brain under the wound was very vascular, and might be said to be in a state of inflammation: this was not the case with the other parts of the brain. The right hemisphere was healthy, except a slight effusion of blood on the anterior lobe. The cerebellum was healthy.

CASE V.—Mortification of the Toes—Tetanus—Death.

Thomas Davies, æt. 31, was admitted on the 21st October, 1836, with mortification of the toes of both feet. He is a native of Africa, a sailor. He states, that a few days after his arrival here from Africa, he had been washing the deck of his vessel barefooted; the cold water had produced great pain and numbness in the feet. On his return to his lodgings in the evening, he sat near a large fire, the pain in the feet increased, and the next morning he observed blisters containing water. It is five weeks since this occurred; now there is complete mortification of the toes, and it is extending to the instep; the skin which is diseased is of a leaden colour, that which is healthy is perfectly black; the pulse is quick and feeble; the tongue white. A turpentine dressing to be applied to the feet, and over this poultices.

26th.—The line of separation is quite perfect on the top of the feet. The cuticle, which was loose, was removed from the soles; the commencement of the line of separation could be seen close to the toes.

30th.—This morning the patient complained he could not swallow; whenever he attempted to do so, he had a spasm in the muscles of deglutition. In fact, the tetanic symptoms became, each hour, more aggravated, and the following day he died.

Various medicines were tried, such as opium and tobacco; kreosote was tried in large doses; in the first instance five grains were given, rubbed down in

mucilage: this was increased to fifteen grains every hour: this was found to produce so much irritation, that it became necessary to administer small quantities of arrow-root: this treatment was persevered in until six p.m., during which time he had taken one drachm and a half, when, finding that it did not produce the slightest mitigation of the symptoms, tobacco enema were administered. The greatest relief was given by rubbing a strong ointment of belladonna in the neck and spine; this was only temporary, for the symptoms returned with greater violence, and on the third day of the attack he died.

On dissection nothing particular presented itself.

CASE VI.—Fracture of the Second Lumbar Vertebra—Death.

William Benson, ætat. 28, was admitted into the hospital, November 4th, 1836, with compound dislocation of the left ankle, fracture of the calcis and fibula, and fracture of the calcis of the right foot. There was also fracture of the second lumbar vertebra. He is a native of Ireland, and up to the period of the accident had been in good health. From his statement, it appears that he fell from the outer wall of the graving dock, a height of at least 10 yards; he alighted on his feet, and then fell backwards: he had great difficulty in getting on his feet, but found he could not stand. He then crept on his hands and knees a distance of more than half a mile to a brick-field; he was here discovered by a policeman, and brought to the hospital. He confessed after his admission that he had been stealing copper nails, and on seeing the police had dropped from the wall.

The accident occurred at one o'clock in the morning: in consequence of the extreme pain he suffered from the back, it was determined not to amputate the leg. He complains of great pain at the lower part of the abdomen and in the right hypochondrium. He had not any difficulty in parting with his urine and feces, and could move the lower extremities. He lived only three days after the accident.

On examination after death, we found the second lumbar vertebra completely fractured; the transverse processes were both broken off, and the body was broken from the spine; the ligaments were torn: and there was considerable

displacement of several portions of the bone: the cord, however, was uninjured, and although the bony canal was fractured across, yet the spinal cord was not in the least pressed upon. This accounts for the want of the usual symptoms attendant on such injuries: he had full power over the bladder and rectum to the last, and could move his lower limbs. Some time before death he had priapism.

CASE VII.—Ununited Fracture of the Femur—Operation—Cure.

John McCay was admitted into the hospital in September, with an ununited fracture of the femur. He is a ship carpenter; before the accident had always good health; he is 30 years of age. He states that he fell from a mast in Londonderry, and fractured the left femur; he was immediately conveyed to the Infirmary there; the bone protruded through the skin. The accident happened nine months before his admission into the Liverpool Northern Hospital; the usual means were had recourse to, (such as blistering, friction, and motion,) without any benefit; it was at last determined to cut down and remove the ends of the broken bone.

The patient was placed on his right side, and the left leg was flexed; an incision of 7 or 8 inches in length was made at the outer side of the thigh, and continued down to the bone; a second incision was made in the centre of the first, commencing at the front of the thigh and extending it to behind: this incision was four inches and a half long, and the integuments were dissected back, and the muscles cut through, until the bone was completely exposed: the lower fractured portion was more prominent than the upper one: there was a middle piece of about two inches and a half in length: this portion of bone was firmly united to the upper shaft, and lodged under the lower one, which caused it (the lower portion) to project; there was not any union of the lower shaft to this middle piece of bone or to the upper shaft. A strong piece of leather was passed under the lower shaft, and about an inch of the end was sawn off; there was very great difficulty in removing the end of the upper shaft and middle piece of bone; it was, however, at last accomplished by means of Hey's saw and the chisel. The edges of the bones were brought together. The

limb could not be much extended: this arose from the contracted state of the muscles: it was placed on the double-inclined plane. It is unnecessary to continue this case through the cure; suffice it to say, that in six weeks the wound had healed, and the bone was firmly united. He is now able to follow his work, as a ship carpenter; the leg is two inches shorter than the other, and he is obliged to walk with a stick and a high-heeled shoe. The ends of the bone which were removed were rounded and covered by a membrane which appeared to be a continuation of the periosteum. I was not able to obtain the nature of the treatment employed before his admission into the Liverpool Hospital. I thought of applying the seton in this case, but was of opinion that after a fracture had remained ununited so long, as was the case in this instance, that nothing but the operation of removing the fractured extremities could do any good.

CASE VIII.—Sloughing of portion of the Testicle—Cure.

Thomas Reece, æt. 30, was admitted into the Liverpool Northern Hospital, on the 19th of May, 1837, with disease of the testicle.

He states that about eight months since he perceived a swelling of the right testicle, which was not accompanied with much pain; occasionally it ached, and at times was quite free from pain; it was the aching which first drew his attention to the part, for while it was present it was accompanied with nausea. He has never taken any medicine, or used any application for this disease. About two weeks before his admission, the scrotum became very hard, and looked red; there was little or no pain at this time, and in a few days several small lumps formed in the scrotum and broke: this occurred in five places, and there was a slight watery discharge from them. He never had an attack of gonorrhœa or syphilis, nor does he recollect having hurt the testicle at any time. The patient now looks pallid and unhealthy; he is a sailor; he has lost flesh. The testicle is enlarged and very hard, and there is a rough uneven surface. The scrotum appears to be ulcerated through in four or five places, and at the bottom of each ulcer a white slough is perceptible. The probe can pass between the scrotum and the

sloughs for a considerable distance. A director was introduced into one of the openings, and an incision was carried through the others so as to connect them. The testicle could now be seen; it was uneven, and looked in a fungous state, and sloughy. A consultation was held on this case, when my friend and colleague, Mr. Wainwright, suggested that a mercurial course should be prescribed, and to dress the testicle with finely levigated red oxide of mercury. This plan was agreed to.

R Pil. Hydrarg. ʒi. ; Pulv. Opii, gr. iv.
M. et div. in pil. xij. capt. ij. mane
nocteqe quotidie.

28th.—He is under the influence of the mercury. The mouth is sore, and the face swelled; the slough is beginning to separate.

30th.—The slough has come away. That part of the testicle which remains looks healthy; it is, however, a very small portion of the organ: the mouth is very sore; the scrotum is much reduced.

Omit the pills.

July 1st.—He was dismissed cured. When the parts had quite healed very little of the testicle could be felt.

CASE IX.—*Fractured Skull, with depression—Operation—Cure.*

Edward Cogan, an Irish labourer, æt. 28, was admitted into the hospital on the 2d June, 1837, with fracture of the skull. It occurred in consequence of a blow from a block; he was standing on a stage to receive a heavy cask which had been hoisted out of a ship's hold; when the block was disengaged from the cask it came with great force against his head, and knocked him senseless. He remained in a state of insensibility for some time; on his recovering, he vomited several times, and parted with some blood from the mouth. On his admission, which was about an hour after the accident, he was in a state of stupor; the breathing was slow; the pulse slow and irregular, but full; the pupils are contracted; over the right parietal bone there is a considerable swelling, produced by effused blood, and there is a distinct feeling of fracture of the skull. There is not any wound of the scalp.

V. S. ad ʒxx. ; cold lotion to the head; saline purgatives.

June 3d.—He felt faint after the bleeding, and vomited. He has continued to vomit during the night.

The pulse this morning is 58, and very full; the pupils are dilated; the breathing slow, 18 inspirations in a minute; he answers questions on being roused, and will, indeed, hold a conversation: the bowels have acted; the tongue is furred.

Eighteen leeches to be applied to the temples; the cold to be continued to the head.

R Hydrarg. Submuriat. gr. x. statim.
sum.

Jan. 4th.—The stupor continues, and the breathing is laborious; the pulse is 55, and not full; the pupils are dilated, the left more than the right; the bowels have acted by the use of an enema of turpentine which had been administered every three or four hours. There is slight paralysis of the left arm and leg.

Hirudines xviii.; cold to the head; and the enema to be repeated.

In the evening 18 leeches were again applied to the head, in consequence of the patient complaining of pain, which subsided after their application.

5th.—The breathing is much better, and the pulse has risen to 63 beats in the minute; the stupor is still present, though much less; the tongue is furred, and brown in the centre; he is more restless; the paralysis is more complete; he has power over the bladder and rectum.

Apply 18 leeches to the head; continue the turpentine injections; continue the lotion to the head.

June 7th.—The stupor is nearly gone, and the breathing free; the pulse is at 78, and regular; he complains of a throbbing pain in the seat of injury; the pupils continue dilated; they act on the admission of light; the paralysis is much the same; the tongue furred and dry; the bowels have not acted since yesterday morning; he has had two injections, and 3 drops of castor oil.

8th.—The bowels have acted freely, and he is in every respect better.

10th.—He complains of more pain in the head; the pulse is at 80, and full, and there is a state of stupor about him which has not been present for two days.

Hirud. xij.

11th.—He is much relieved.—Pergat.

14th.—The swelling over the parietal bone looks red, and is painful to the touch: he has complained for some days of a throbbing pain in it; in other respects he is better. An opening was made through the scalp, and a large quantity of dark coagulated blood escaped with matter: the skull was felt denuded of the periosteum, and much depressed.

16th.—There is a dark-coloured offensive discharge from the opening in the scalp; he is much easier, and in other respects better; the paralysis of the arm is less.

26th.—He continued to do well until to-day: yesterday he had slight pain under the frontal sinuses, and felt languid. An aperient draught had been given, which relieved his bowels. In the night he had a severe rigor, and vomited twice; the tongue is brown, and dry in the centre; the pulse is frequent; he complains of great pain in the head, and there is more stupor about him. In consequence of these symptoms, a consultation was held, and it was determined to raise the depressed bone. A crucial incision was made of considerable extent, and the scalp dissected back. There was a very great depression of bone; the fracture extended in several directions: several large pieces of bone were raised and removed: on taking away a lower piece, about two drachms and a half of pus escaped. The greater part of the bone which was removed was quite bare, and looked unhealthy: the remaining portions were completely raised; the dura mater looked dark: the bone which was removed was of a triangular form; it measured at the base two inches, and on each side three inches. There was considerable hæmorrhage from the meningeal artery; the patient fainted, and the hæmorrhage ceased. The edges of the wound were brought together and a roller applied. At the time of the operation the paralysis of the leg was complete, and that of the arm considerable, though less than it had been. The strict antiphlogistic treatment was enjoined after the operation.

On the third day after the operation the paralysis of the arm had nearly disappeared, and that of the leg was much less; indeed, the difference was so great,

that before the operation he could not stand; on the third day after it he was able to walk, with assistance, into another ward to which he was removing for quietness.

August 21, a piece of bone came away through the wound.

September 4th.—The wound is quite healed, and the patient is perfectly recovered, being quite free from paralysis, and has all his faculties perfect. He was this day dismissed cured.

MR. MILLER ON EMBRYOLOGY.

To the Editor of the Medical Gazette.

SIR,

I THROW myself for publication on the liberality of your journal, which has been so often boasted as the vehicle of the communications of science.

In the year 1827 I attended Dr. Birkbeck's lectures on anatomy, at the Mechanics' Institution. I was then following the trade of a plasterer and modeller. Those lectures kindled in my bosom the love of anatomy: I determined to leave my trade and follow it. I offered my services as porter to the dissecting-rooms, but could not get an engagement. Mr. Green, of St. Thomas's, gave me employment in casting cases for his museum, for about twelve months. I then commenced forming a collection of my own. The subject I chose to investigate was embryology, and the connexion of the placenta to the uterus. I have examined the uteri of sixteen women who have died undelivered. In 1835 there was no preparation in London to prove the connexion of the uterus to the placenta, or the placenta to the uterus, except the preparation marked No. 3535 in the Hunterian Museum. The account of it was drawn up by Mr. Mayo, in the presence of Mr. Stanley and Mr. Owen; and all three concurred in thinking that this preparation established all the points of structure which are described in it. I would beg the attention of midwifery teachers to that preparation.

The paper on the Structure of the Uterus and Placenta, by Dr. Robert Lee, in the Philosophical Transactions,

drew my attention to another method of injecting, by making two injections to run at one time—the one in the hypogastric arteries and veins, and the other in the arteries of the funis, with different coloured injections, so that it might be seen where each terminated. I have succeeded in it. An artery as large as a crow-quill is seen to pass, filled with injection, from the uterus into the placenta. The large decidual vein round the margin of the placenta was filled with injection. In this vein there are three openings, as large as goose-quills, into the veins of the uterus. These three veins return the whole of the blood from the placenta into the uterus. In the greater number of uteri that I have examined, I have never seen more than four venous openings, and never less than three. There are also another set of vessels, called the decidual arteries and the decidual veins. The arteries go to the uterine layer of the decidua, and stop there; so do the veins; they form the short curling vessels of Hunter. Separate portions of this uterus were dissected by Mr. Green, Mr. North, and the late Dr. Hugh Ley, who saw the same appearances. I have the preparation now. I have also been successful in injecting other uteri.

In Magendie's *Physiology* (1829, p. 509), he says, "In animals, among the small vessels which go from the uterus to the placenta, there is not one which has the appearance of a vein." In my collection I have what shews the reverse—the veins and arteries are injected. In London there are twenty teachers of midwifery, and there is not one preparation to prove the connexion of the placenta with the uterus. The painter is known by his paintings, and the sculptor by his works. Why not the physiologist? Have they nothing to discover? What have the teachers in London done for the last ten years? Have they no preparation to produce that we never saw before? All is for money, and not science.

There ever will be in this, as in other callings, some few who, even in silence and solitude, and among the most disheartening difficulties, will travel the rugged up-hill path of science, urged by no other stimulus than the love of truth—seeking no other reward than that which results from the nobleness of their pur-

suits. Allow me to prove this:—Sir Astley Cooper can receive no more honours than he has got; behold him in his old age working every day; his preparations prove what he has done the last ten years. Go to Mr. Kiernan, and see his splendid collection; that shews what he has done the last ten years. Go to Mr. Langstaff's collection; that shews what he has done (his kindness to me I can never forget). Go to Mr. Salmon's collection; that shews what he has done. There is also Mr. King's (of Guy's) collection of the heart, which I have not seen. I grant that museums have been made in that period, but what discoveries are there in them? Allow a poor mechanic to tell what he has done.

I have dissected 127 human abortions, from two grains in weight to that of nine months, shewing the vesicula umbilicalis—shewing the omphalo-mesenteric arteries and veins injected—also, the germinal disk in the human ovum, I believe the only one known—the preparation at nine months in the series of the omphalo-mesenteric vessels; their existence at nine months is denied by Velpeau. I have a preparation shewing the first formation of the amnion and allantois in the human female; also in animals, shewing the amnion and allantois to be one membrane, so that the fluid in the one is the same as in the other.

I have dissected, in a series of animals, the vesicula umbilicalis, with its duct; also with the omphalo-mesenteric artery and vein. I have dissected four kangaroos, from two to four months, shewing that there is no urachus in that animal at that time. I have dissected and prepared also the whole series of the chick, at every second day of incubation. These dissections prove the following facts:—

1. The general causes of early abortion.
2. By looking at a placenta, if normal, it will shew where the fœtus was first formed.
3. How best to remove the placenta from the uterus, in order to prevent hæmorrhage.
4. The existence of a series of cells in the placenta, like a honeycomb.
5. The manner in which the first circulation takes place from the vesicula

umbilicalis, and the first blood-vessels formed in the embryo.

6. The second circulation. Do the vessels proceed from the *fœtus* to the placenta, or from the placenta to the *fœtus*?

7. In case of malformation without a bladder, the cause?

I could enumerate a few more, but shall soon publish them in my work. Taking them as a series of preparations, I am told they are unrivalled in Europe. In 1835 they were in part exhibited at St. George's, by Dr. Robert Lee; in his lectures at St. Bartholomew's, by the late Dr. Hugh Ley; and at the Westminster School of Medicine, by Mr. North; and since then by some other teachers. I think of placing them in some public institution, so that all persons may have access to them,—perhaps the British Museum.

Dr. Robert Lee introduced me to M. Coste, from Paris, a short time ago. This gentleman obtained a gold medal from the Institute of France for his researches on embryology. M. Serres is the reporter on the memoir to the Institute. I told M. Coste in the presence of Dr. R. Lee, that all he had written on the embryo of animals was wrong. Dr. Lee had seen my preparations in the sheep and the rabbit, with the vesicula umbilicalis between the amnion and chorion, twelve months ago, and might have confirmed me, but chose to be silent. I told him to call in three Frenchmen, and I three English—to put his book on the table, and I would put my preparations, and let those six report. Dr. Lee wished me to show him my preparations, and then to trust to the honour of M. Coste. This I refused. I at different times compared his plates with the preparations before Dr. Ferguson, of King's College, Dr. Ryan, Mr. Kiernan, Mr. Skey, and other gentlemen. All are convinced that he never dissected out the vesicula umbilicalis, but had figured the allantois for the vesicle. Mr. Owen, the Hunterian Professor, has fallen into the same error in his papers on the kangaroo, in the *Philosophical Transactions*, 1834, and *Loudon's Magazine of Natural History*, 1837; and is also wrong in saying a urachus exists in that animal. What could M. Coste, M. Serres, and Mr. Owen, be doing with their eyes?

This is the way that errors are propagated. Because they are great men, it is taken for granted that whatever they say is true, without being called upon to make preparations in proof, and the student never thinks it is necessary to investigate nature for himself. The plan I have adopted is the following:—I examine the uterus of animals at eight days after copulation, at fifteen, and at thirty days, or the calendar month. The cow goes nine calendar months, so does woman; and as I have found only four normal ova out of 127 human, I will see what, by analogy, I can make out from the cow. I am making a fetal preparation of each organ when formed, dating also the period when it is completed. There is one, an ovum of thirty days after copulation, in the mare, (in the Royal Veterinary Museum). I asked Mr. Sewell to allow me to examine it, but he denied me. I told him I was not to be beat, and would beg money to buy an ass to destroy it thirty days after copulation. I invited him and Mr. Colman to see my preparation, and see what I was doing. They have never come. Mr. Sewell asked me where I got my knowledge. I told him in an attic—for that was the best light for dissecting. What have they done at the Royal Veterinary College for the last ten years? I told Mr. Sewell that if he would teach physiology in the College, he must learn it from a mechanic. I beg that some one would intercede for me to examine that ovum, or procure for me a similar one: as I have got the ovum of the cow at thirty days, their comparison would be useful. If I was a great man I should not be denied; but I am poor—a pupil of a Mechanics' Institute, and not of a College. One high in the church told me to my face that I had drunk deep out of Dr. Birkbeck's cup—that the schoolmaster was getting too much knowledge. I pitied his ignorance. I know that those who have written on this subject do not like to be criticised by a mechanic; but by the truth I will stand or fall.

I will now give a description of the smallest human ovum in my series.

The first formation of the human ovum, with the germinal disk or spot. The ovum weighed four grains, and is five lines in diameter.

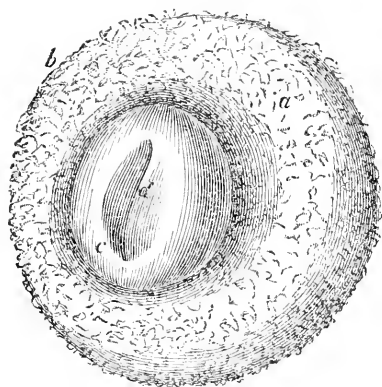


FIG. 1.—The Exterior of the Ovum.

- a. The chorion membrane.
- b. The floeculent chorion.
- c. The germinal disk, three lines in diameter.
- d. The puckering of the disk in the centre.

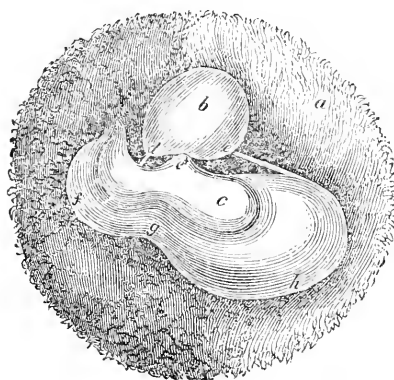


FIG. 2.—The Interior of the Ovum.

- a. The chorion membrane.
- b. The vesicula umbilicalis, two lines in diameter.
- c. The germ, three lines in length.
- d. The duct from the vesicle into the germ.
- e*. A vessel from the vesicula, in a direct line to the nearest part of the membrane at the head of the germ.
- e. A vessel from the vesicula to the membrane that forms the allantois of the germ, at the inferior part.
- f. The membrane that forms the amnion.
- g. The first formation of the urachus.
- h. The membrane that forms the allantois.

The membrane by which the amnion and allantois are formed is only one, as follows. It is an oval membrane, with the germ at the end of it, *i. e.* at *e**, where the shortest vessel proceeds from the vesicle to the membrane, so that the membrane becomes like the figure ∞ , or hour-glass; the large half is the allantois, and the turning in the centre becomes the urachus, *g*, and the fluid in the one is the same as the other. I have preparations showing the opening through the urachus (which can be seen with the naked eye) into the amnion.

As the allantois shoots to the membrane of the chorion, and the space being confined, it becomes depressed in the middle to suit the circle of of the ovum with the urachus along-side of the duct, from the vesicula umbilicalis to the germ. The interior of the ovum was filled with pellucid gelatine, which is always to be found in the normal ovum. I do not know the date of this ovum, as the woman met with an accident, and aborted in twelve hours.

By comparing this ovum with plate 1, fig. 4, in Velpeau's Embryology (which preparation is from Pockel), Velpeau doubts the accuracy of it, and asserts he (Pockel) must have made it with its long vessel from the vesicle to the allantois. I believe that part to be correct, although a great part of the preparation is abnormal. It is the vessel from the vesicula umbilicalis to that part of the membrane which forms the allantois.

In Dr. Barry's translation of Professor Wagner's *Inquiries concerning the Germinal Vessels*, in 1835, he states as follows:—"That in the human ovum there is present a germinal spot, appears to me probable. What may be its meaning? Does it stand in a certain relation to the embryo?" Also, in his translation of Dr. Valentine's *Development of the Human Ovum*, he states the following:—"It appears to be almost certain that the mammalia, before impregnation, possess no germinal disk, but merely a germinal vessel; the same is the case in the ovum of birds also, in the early stages of its development within the ovary."

I must confess that this subject still remains defective and dark: although the brightest names in the page of science have thought it worthy of their

researches, yet their reports contradict each other in many essential points.

The microscopic examinations of most men suit their own imaginations—a very unpardonable error. Papers written without preparations to prove the facts stated, should not be received in any Society. I grant that some appearances cannot always be preserved, but six men can always be found to prove the truth of these appearances.

There exists too much mystery in schools and colleges on this subject. There should be no mystical lore; knowledge is not mystical—it is only mystified to conceal something wrong. I have ova in my collection from six days and upwards after conception. Every man that has got eyes can see them as well as I can. I have got a diagram of the human ovum with the germinal disk, made by Mr. Perry; which every teacher of midwifery may use until he has one of his own.

I have given Mr. Perry permission to make diagrams of it; at the same time, no teacher shall use the diagram without having seen the preparation for himself.

Through the kindness of Dr. Ure and his son, Mr. Perry and myself had an opportunity of examining the ovum through his powerful lens.—I am, sir,

Your obedient servant,

JOHN MILLER,
Mechanic.

No. 3, Theobald's Road,
London, Nov. 6, 1837.

COMMON TOBACCO-PIPE BROKEN IN THE URETHRA.

To the Editor of the Medical Gazette.

SIR,

If you think the following case worthy a place in your valuable publication it is at your service.—I am, sir,

Your obedient servant,

THOMAS HAIGH MARTIN,
Surgeon.

Holmfurth, Yorkshire,
Oct. 28, 1837.

B. A. of Newcloses, in this neighbourhood, having been subject to occasional attacks of retention of urine for many years, was attacked with complete retention on the morning of the 19th June, 1828, for which I used the catheter. My father saw her on the 20th, prescribed a little medicine, and employed

the catheter sometimes once and sometimes twice a day, until the 2d August. But she being a pauper, and the overseers objecting to the expense of a medical man constantly attending, gave orders to the woman not to send to her medical attendant any more; from which time to the 15th September she employed a common tobacco-pipe three times a day; but, unfortunately, it being so brittle, she broke it in the urethra; and with the endeavours she made to extract it, pushed it into the bladder; from which time to the 27th April, 1829, it produced no inconvenience, she being provided by the overseer with a silver catheter immediately after the above accident, which she used two and three times a day. On the 27th April she was attacked with pains, which she described as similar to labour pains, but much more severe. My father was again requested to see her on the 28th, and found by examination *per vaginam* the pipe lodged in the cavity of the uterus, with one end protruding through the os uteri; but it was so strongly embraced by that viscus as to require the aid of a pair of dressing forceps to extract it, and so much force as to break the end of the pipe several times.

What I consider most remarkable is her never losing a single drop of urine but what passed through the catheter, which she continued to use three or four times daily up to the time of her death, which took place on the 30th November, 1834, after being confined to her bed entirely for three years or better, she having sunk at last from phthisis pulmonalis.

I was able to procure only a partial examination. The bladder was thicker than natural, but not the smallest particle of calcareous matter in it. On its left side, not far from the ureter, was a patch about the size of sixpence, which was much softer than any other part of the mucous membrane, and tore upon the slightest touch of the finger. I therefore concluded that to have been the place which had given passage to the pipe into the vagina, from whence it found its way into the uterus by the os uteri; as I should think it quite impossible it could have passed through the body of the uterus without leaving some sign of a cicatrix, which I was not able to find, although I brought away the uterus and bladder to examine them more leisurely.

The pipe is three inches in length, and was three inches round the middle part at the time it was extracted (from calcareous matter, which I believe to be the triple phosphate); but at present it measures only two inches and a half. It is now in my possession.

ON SCURVY.

From the Annual Report of

DR. MURRAY,

Principal Medical Officer at the Cape of Good Hope, for 1836.

Communicated by Sir James M. Grigor, Bart.

[Continued from p. 164.]

Report of Assistant-Surgeon Ford,—continued.

CASES VI. and VII. — The first patients with scurvy I admitted at Fort Willshire, were privates — Samuel Christie, æt. 28, and Thomas Atkinson, æt. 28, both of the 75th regiment, on the 12th of August, 1836, from Fort White; the disease being then of about three weeks' standing. They had had their usual rations, and half a gill of brandy daily, but had got no medicines. When admitted they were much exhausted, and unable to stand, from pain in the limbs and contraction of their knee-joints; the skin of their thighs and legs was extensively ecchymosed, and there was hardness and rigidity of the subjacent parts. Their general appearance denoted apathy and debility; countenance sallow and bloated. Christie suffered most from pain and straitness across the chest, and a catching in his breathing. Their gums were spongy and purple; teeth loose; breath offensive; urine scanty and high-coloured, and soon emitted a rank odour after being passed. Bowels regular; appetite good.

They slept better the night after their admission than before, probably from having comfortable beds. On the following morning they had a small dose of Epsom salts in bitter infusion, and their bowels were kept regular afterwards by occasionally drinking infusion of tamarinds. Their diet was as stated in the foregoing report, and after a few days indications of amendment were visible, and on the 28th of August, Christie left the hospital cured, but Atkinson remained eleven days longer, when he was also discharged cured.

CASE VIII. — Private James Henderson, 72d regt., æt. 34, was admitted Sept. 15th from Fort Cox, having evinced symptoms of the scorbutic diathesis since the middle of the preceding month. He had a soft, bloody, fungoid tumor, on the outside of the right leg, which was considered an indolent furunculus in the first instance, by the surgeon at Fort Cox; and an incision was made into it, and poultices applied, when a fungoid growth shot forth from it, known by the name of "*bullock's liver*," by seamen. The general symptoms were similar to those described in the two preceding cases, except that there was not so much debility. He had got nitrate of potass, sulphate of magnesia, and a daily allowance of diluted brandy, at Fort Cox, but without any appearance of advantage.

I employed the same general practice as before stated, and applied lint soaked in a saturated solution of chloride of lime to the fungoid ulcer; and on the 7th October (having been twenty-three days under treatment), he left the hospital perfectly recovered.

CASE IX. — Mathew Fyans, æt. 30, 75th regt., two months ill, was transferred to my hospital on the 6th Nov. The most urgent symptoms were constant pain, and frequent spasms in the region of the heart, especially during sleep, rendering respiration difficult and catching; great feebleness, particularly in the knees and calves of the legs; extensive ecchymosis and induration of the integuments of the legs, and pain in the ankles; gums not so spongy and livid as in the preceding cases; skin dry, *anserine*, and very itchy; countenance dejected, sallow, and puffy; pulse slow and feeble; bowels rather torpid; urine high coloured; appetite tolerably good. Former treatment unknown.

He had the same diet given as the other scorbutic cases; but had *two* gills of Cape wine daily, except during a few days that wine could not be procured, when he got one gill of brandy, with water, sugar, and fresh lemon juice, in lieu; 5ij. of Epsom salts, with tonic infusion, and tincture, were given every second or third morning, and a lotion of the solution of chloride of lime applied to the skin. He soon began to improve; but the pain in the heart, palpitation, and straitened breathing, still continued, when I handed him over

to Dr. Graham, 75th regt., on the 14th December, seven weeks after admission.

REMARKS.—It has occurred to me more than on e, that in this case moderate blood-letting would have relieved the cardiac symptoms, and raised the pulse, the depression of which I consider to depend on impeded and oppressed circulation through the lungs; yet, not being then aware of the introduction of this mode of treatment, and the success reported to attend its adoption, and having the opinion of physicians experienced in the treatment of scurvy before me, "that the disease by no means bears bleeding, even although the most acute pains upon the membranes, a high degree of fever, and dangerous hæmorrhages, would seem to indicate it," and that "the patient generally dies soon after the operation;" together with the consideration that this patient, being in an advanced stage of the disease, suffered from feebleness, and depression of energy and spirits, for which tonic treatment was required, I was deterred from using the lancet.

CASE X.—Alexander Liddle, æt. 36, 75th regt., received from the New Province on the 6th November, affected with enormous enlargement, intense ecchymosis, unyielding hardness, and much pain of the whole of the right lower extremity, and contraction of the knee. The gums, soft palate, velum palati, and uvula, were tumid, spongy, and livid; and purple-coloured fungi rose from the gums to a level with the edges of the teeth, interfering with mastication. He did not suffer much from pain of the chest, nor oppression of breathing, nor did he evince any tendency to faintness.

The same line of treatment was followed as in the preceding cases; and when I transferred him to Dr. Graham on the 14th December, he was hardly convalescent. He was soon after sent to Bathurst, and I have since seen him in the performance of his duties, wholly free from the disease.

CASE XI.—The worst case of scurvy that has fallen under my observation was that of private Huff Gavin, æt. 26, of the 75th regiment. This man first shewed symptoms of scorbutic affection at one of the out-posts in the New Province, about the latter end of August, and was treated in the 75th Regimental Hospital, at King William's Town.

In October he was removed with his company to Fort Cox, where he remained under treatment, chiefly of nitrate of potass, sulphate of magnesia, with brandy and water, until sent to Willshire, on the 8th of November.

At this period he was exceedingly exhausted, and fainted while being removed from the waggon to the ward. Countenance sallow, emaciated, and dejected; pulse small, frequent, and feeble; respiration short, quick, and oppressed (from pain in the chest); urine passed in small quantities, and high coloured; bowels rather torpid; appetite tolerably good, but has no desire for animal food; thighs and legs ecchymosed, stiff, and painful, with contraction of the knee-joints; pain and stiffness of the arms, and inability to use the hands; the pain of his loins almost prevent his moving in bed. The gums presented their usual state of sponginess. His debility was such, that even on the ninth day after his admission, while being carried on his mattress through the open air, from the old to the new hospital, he became nearly in a state of perfect syncope.

In the treatment, I deviated a little from my usual practice, but still it was conducted on the same principles, viz. that of exciting vascular action, restoring the healthy condition of the blood, and re-establishing the strength, by means of nutritious and corrective diet, and tonic, cordial, and saline remedies. During the first twelve days (having a distaste for meat) he had rice with milk and sugar, soup containing fresh vegetables, bread and milk, two gills of Port wine, and an orange, for his daily aliment; but afterwards he used the ordinary diet, with vinegar, two gills of Cape wine, and an orange, daily.

An aperient of Epsom salts, with infusion and tincture of gentian, when the bowels required assistance; 5 grains of carbonate of ammonia in 5ij. of camphor mixture, twice a day; friction of the extremities with soap liniment, and a gargle of solution of chloride of lime, comprised the medical means employed.

On the 21st November he was able to leave his bed; in two days more he moved about the ward; and after the 26th he enjoyed exercise in the open air daily. All the symptoms successively disappeared, and on the 9th December (just one month from the date

of his admission,) he left the hospital as a convalescent, merely labouring under some remaining debility.

In detailing the foregoing cases, I have candidly stated them to the extent of the information I acquired; I noted the symptoms of the disease, and the progress towards recovery, which I observed, to the best of my ability and without prejudice. I have also laid down the line of practice I followed, with all those affected with scurvy under my charge, and my reasons for its adoption; and from the favourable result in all who had been sufficiently long subjected to it, I conceive that my choice of it, and continuance in this mode of treatment (bearing in mind the class and state of the patients in whom it was employed,) are fully justified.

I am not prejudiced against, nor in favour of, any particular treatment; this, however, is the first opportunity I have had for the clinical investigation of scurvy; and it has been but partial, those only in advanced stages having fallen under my observation; but should I have an opportunity of investigating its nature at any future time, and of essaying the antiphlogistic treatment now recommended in its early stages, I shall not fail to avail myself of it, to give the practice a fair trial, and I shall feel happy if able to add my testimony to its beneficial effects.

P.S. I should decidedly say, that the men of the 75th regiment had not more, nay, not so much, hard work and fatigue in the New Province, in marching, exposure, felling wood, and hut and fort building, as the men of the 72d regiment; and as to "the huts left by our men," which I understand are now complained of, and brought forward among the causes of scurvy in the 75th regiment, there certainly were three or four of an expeditiously-built description among them, called "*Hartebeest huts*," which Colonel Smith caused to be completed; but all the rest were similar to those that were and are still usually erected at the different posts; and, indeed, at Fort Waterloo, where scurvy appeared very early, and prevailed extensively, I have learned that huts built there, under the superintendence of Lieut. Brown, 75th regiment, were the most comfortable in the New Province.

On having my memory refreshed, I

now perfectly recollect the "*anomalous neuralgic cases*," as they used to be called, in the 75th regimental hospital, at Cape Town, some years ago, which were certainly scorbutic. They occurred, I think, chiefly in men confined in the prison cells. I well remember their spongy gums, the great discoloration and swelling of their limbs, and the contraction and rigidity of their knee-joints, which Dr. Graham pointed out when I occasionally visited his wards; and which, by the by, he was treating *unsuccessfully* with tonic and stimulant remedies; among others, with large doses of carbonate of iron, as recommended in neuralgia. The 75th hospital was then seldom without one or more cases, so that the men of that corps even then evinced a tendency to this disease; while, in our regiment, I cannot call to mind a single instance, during the whole time we were in Cape Town, in which advanced symptoms of a scorbutic character shewed themselves, although I have a recollection of two or three being removed to hospital from the cells with spongy gums. If more of them were affected, it must have been in so slight a degree as not to render their admission into hospital necessary, as they never reported themselves.

IV. *Report of Staff Assistant-Surgeon Minto.*

February 1837.

During the year 1836 that I have been in charge of Fort Armstrong, in the Kat River Settlement, and the adjacent posts, it will be seen on a reference to my annual return, that the diseases of the detachments from the 27th regiment and Hottentot corps, which occupied them, have been of the most simple nature. The men were employed during the whole year in the construction of public works, and idleness and inactivity were guarded against. The European soldiers were quartered in a comfortable, well-ventilated, barrack-room, and were able very generally (but not always) to procure vegetables, and had very little patrol duty. They arrived from England in September 1835, mostly all young soldiers, with constitutions which have not been impaired by the immoderate use of Cape brandy, as we have had no canteens; than which I conceive there cannot be a greater evil at any out-post, whether in a mili-

tary, medical, or moral point of view : hence they enjoyed good health.

The Hottentot provisional troops erected for themselves such huts as they usually dwell in, which secured them against the inclemency of the weather ; and their food and clothing were infinitely better than that they had been accustomed to.

These circumstances may perhaps afford a sufficient reason why the men under my charge have not been at all affected with scurvy, which prevailed so much among the men of the 75th regiment, in the Caffer Province of Adelaide.

In order to ascertain if scurvy was known to the natives of this part of the country, I set an investigation on foot throughout the Hottentots of this settlement ; and found that none of them had ever seen or heard of any disease approximating to it among them ; and further, from all I could learn, it seems to be one to which none of the coloured tribes of this part of Africa are subject ; at least so far as the travels of the oldest people here have extended.

In regard to prevention, I would say, that when there are no public works going forward at military out-posts, I think that much good may be done, and much evil prevented, by introducing active exercises and exhilarating amusements among the men. None but those who have lived at a small military out-post can conceive the state of apathy into which soldiers generally get when unoccupied.

[To be continued.]

SALTS OF LEAD IN CHOLERA.

To the Editor of the Medical Gazette.

SIR,

IN one of your former numbers I observed some remarks of Dr. Graves', relative to the use of acetate of lead in cholera, in which disease it was found by him eminently successful. I have, however, to observe, that I gave the acetate of lead a very extensive trial in the cholera hospital of Bethnal Green, and found it equally inert with most other remedies. I would also observe, that the acetate is not the best formula ; for the evacuations from the stomach and bowels are usually *alkaline*, and

consequently the oxide will be precipitated. Free carbonic acid will also precipitate carbonate of lead from the acetate, which may, however, be obviated by the addition of a sufficient quantity of free acetic acid.

The late Sir D. Barry requested me, as physician to the Bethnal Green district, and the cholera hospital there established, to try the salt of lead. His observations were, "that he considered the disease a *serous hæmorrhagy* (flux ?) from the bowels, and that acetate of lead, therefore, seemed to be the most appropriate astringent." But although administered in large doses combined with opium, it proved inert, and frequently seemed to aggravate the cramps. It is true that the diarrhoea was occasionally suppressed during its use ; but I much question if this were not as much owing to kino or catechu, which upon such occasions were administered with it. Finding the different acetates so inert, I still determined to give lead an additional chance, and therefore exhibited the *nitrate* of lead, as a less *decomposable* salt ; but I still found this preparation, if not inert, at least of but equivocal powers ; administered, too, under every variety of form and mode. It was most successful when aided by kino or catechu.

Such is the result of my experience. But although I feel bound to make this statement, yet I am willing to believe that there may be something of change perhaps since, in the nature of the disease, and that possibly cases now may prove to be more under the influence of the metallic preparations, of the varieties of which, the nitrate, for obvious reasons, must be the preferable ; and therefore I agree with you, sir, that should occasionally, unfortunately occur, practitioners should give due attention to Dr. Graves' suggestions.

But I must say that, upon reviewing the accounts, I do not find a less mortality than has been published in the tables appended to my pamphlet on Cholera ; and in which the practice adopted will be found detailed at sufficient length.—I remain, sir,

Your obedient servant,

R. VENABLES.

17, Ely Place,
Nov. 7, 1837.

MEDICAL GAZETTE.

Saturday, November 11, 1837.

“*Licet omnibus, licet etiam mihi, dignitatem
Artis Medicæ tucri; potestas modo veniendi in
publicum sit, dicendi periculum non recuso.*”

CICERO.

DISPENSARIES IN LONDON.

DURING the discussions upon the Reform Bill, the case was often put of an intelligent foreigner being the spectator of our political proceedings; and it was asked by either party, alternately, what would such an educated stranger say if he saw the eccentricities of our opponents? What would he say to an election carried on by Sir Mark Wood's servants at Gatton, or to a torrent of cabbage-stalks on the hustings at Covent-Garden? Now let us suppose the “intelligent foreigner” to be a physician, and therefore sympathizing with the more confined circle of our medical interests. Let us imagine him to take up a newspaper, and find an advertisement for a dispensary physician, who must, as usual, be a member of the Royal College, and possess satisfactory testimonials. He might learn, on farther inquiry, that it was a common thing to spend a deal of time, and some money, in canvassing for such a situation; and that there have been occasions when candidates, stimulated by the desire of high preferment, have laid down Bank notes against each other—that is, have paid in subscriptions in the names of imaginary governors, and have thus gained their election at the Grub-street Dispensary, in the same way that they might their election as M. P. at Leicester or Liverpool. The intelligent but not naturalized foreigner, knowing that a professed philanthropist cannot always be had for advertising, and that a *nation boutiquière* must contain many cautious men who are unwilling to exchange solid sovereigns

for empty titles, necessarily supposes that the office gained with much expense, and held with infinite trouble, is very lucrative. What would be his surprise on hearing that the place is almost always a gratuitous one? Or, to drop the foreigner, how significant are the quiet inquiries of sensible people, when they see their medical friends canvassing for a place of nothing per annum! They naturally want to know why the candidate is so eager to obtain zero. Because it leads to practice, replies the abashed canvasser. But then, rejoins the friendly doubter, we do not see why this snatching at a shadow should be more necessary in physic than in the other learned professions. In the law, for instance, we sometimes hear of a cause being pleaded gratis; legal opinions, like medical advice, are no doubt occasionally given without fee to the needy client; but the barrister who should set up a legal dispensary where he doled forth his advice to all comers without a fee, with the avowed intention of getting into practice, would, we more than suspect, be speedily sent to Coventry by his brethren. It may be replied, of course, that medical advice is more frequently necessary than legal counsel to the indigent, and should therefore be provided for them gratis at the expense of the community. This we readily grant—at the expense of the community; but that this be done, the medical officers should be well paid, for at present the charitable bestowal is chiefly at their expense.

In the church — But here the newspapers of the day contain something quite *à propos* to our subject, and we must begin by detailing it. It appears that in the opinion of many friends of the church of England, there is a deficiency of curates in populous places, and an attempt is at present being made to supply the want. Now if the

competition were equal, as some erroneously suppose it to be, in all the learned professions—if divinity were reduced to the same straits as physic, the method of obtaining one hundred or five hundred additional curates would be obvious enough; it would suggest itself, without our mentioning it, to every medical man. Suppose an additional curate to be wanted in the parish of St. Giles's. The vestry would advertise on the 1st of the month, that candidates for the appointment, who must be graduates of Oxford or Cambridge, were to send in their testimonials on or before the 15th, and that the election would take place on the 29th; adding, that particulars of the duty required (among others, of course, that of being content with a stipend of 0) might be learned at the Vestry-Clerk's office. Testimonials would accordingly arrive in shoals, bearing witness to the long experience, excellent character, and unwearied zeal, of the respective candidates, and stating, that if there was any office for which they were more fit than another, it was that of assistant-curate to the parish of St. Giles'. Many also would possess certificates, showing, that, in addition to the required degree of B.A., they had taken those of M.A. and B.D. We need not describe to medical readers the advertising and letter-writing, and testimonial-soliciting; nor the going over the ground, as the phrase is, twice at least, by the candidate and his friends, which would necessarily occur. Yet more; we should hear the congratulations of the friends of the fortunate candidate, and their assurances that he might consider himself as within ten years of something comfortable—of a benefice of 80*l.* a year at the very least—if he behaved with zeal and discretion; at any rate, that he might be quite sure, if he was getting nothing else, that he was “getting known.”

Such things might easily be; but as

yet they are not. The friends of the church, in the present instance, are not giving votes and certificates, but solid cash; and the long lists of large subscriptions for providing additional curates, would seem to show that it is thought that, in one profession at least, hard work deserves something better than empty praise.

In the case of the dispensaries, it must be confessed that if the medical officers are somewhat slow in discerning certain unpalatable truths before their election, they find them out quickly enough afterwards; a few months are usually sufficient to teach that no pay is attended by another stern negative—no increase of practice; to which are added the uncomfortable positives—much work, and plenty of squabbling. The consequence is, that places are thrown up with wonderful readiness: service is found to be no inheritance. At a worn-out dispensary (a *ci-devant* respectable one), we have known four vacancies to occur, in the post of physician, in less than three years.

This dispensary, like others of the old school, once enjoyed the services of stipendiary officers; but when its income declined, and it became necessary to curtail its expenditure, the most obvious way of doing so was to cut off their salaries; for as druggists, leech-venders, messengers, and the like, insist upon being paid, and as this is a matter of indifference to physicians and surgeons, the natural conclusion was, stop these useless salaries, and thereby add so many hundreds to the income, and consequently to the efficiency, of the dispensary.

Let us be allowed to illustrate this very natural argument by an anecdote. Apicius Smith, Esq. kept one of the best tables in London. As almost every one knows, at least theoretically, what a good table is, we will not enter into details, nor speak of the *patés de Périgord*, the *rognons au vin de Champagne*,

or the curious *Johannisberg*, to be found upon his over-hospitable board; but we will just mention that his cook had a salary, not at all resembling the wages of a dispensary doctor, but a very comfortable thing indeed. Fortune, however, at last frowned upon Apicius Smith; Spanish bonds and South American mines rapidly reduced his capital, and it became necessary to retrench. One of the easiest retrenchments to make seemed to be in the stipends of the officers of the kitchen: indeed, after a few changes, he found it ultimately practicable to obtain their services gratuitously; for many thought that the fame of having served so great a gastronome would be a certificate of competency. But the hero of our tale was not slow to discover that he had better have economized at his table than in his kitchen; that what was gratuitously done was ill done; and that he was virtually, though unintentionally, keeping a school for cooks, as most of his new officers of the mouth were satisfied with a little improvement, and six or eight months' work without pay.

This is the short-sighted principle on which almost all dispensaries are now conducted—or miscondacted; but we confess our astonishment that so respectable an institution as the St. Mary-le-Bone Dispensary should have lately fallen into the common error, and passed a law that “no physician or surgeon hereafter appointed will receive any salary or gratuity from the funds of this charity.”

Any one who is acquainted with the subject, and has the interest of the poor really at heart, will allow that all that is done in London by twenty or thirty ill-conducted dispensaries, might be infinitely better done by six or eight good ones, with ample funds, and well-paid medical officers, devoting the greater part of their time to the institutions. The saving in house-rent,

taxes, and other items, which would be effected by such a coalition, would at least balance the increased expense caused by the salaries.

One of the most obvious improvements which would necessarily follow a reform of this kind, would be in the arrangement of the houses intended to be used as dispensaries. At present they are commonly unfit, architecturally, for the use to which they are put. Then it would be absolutely requisite that the hours of attendance should be considerably extended. How inconvenient it would be for the public if a medical practitioner were to be found at home for one hour only every day, or if a government office were to be open only from twelve to one. It continually happens, too, that the patients are thrown out by the accidental punctuality of the medical officer. Suppose that, as usually happens, he is habitually half an hour later than his printed hour; to-day he comes to the minute; his tail, or late patients, presuming upon his regular lack of punctuality, miss him altogether. But it is often said, all the patients ought to be there at the hour printed on the letters; if so, many will have to wait an hour, and some much more,—a serious loss for a patient who, though labouring under a chronic disease, still continues to work; while it is besides an injury to the health of many.

As, in consequence of such a coalition, the remaining institutions would be of far greater importance, we should expect to see persons of more weight in society, both lay and medical, interest themselves in their interior economy. It is almost needless to particularise the reforms to which this valuable superintendence must lead. The quality of the drugs would be improved, when the druggists were informed, quite in earnest, that trash and *pulveres redacti* must be sent elsewhere, and not to persons competent to judge of them.

The same cause would quite alter the system of dispensing medicines. No more substitution of gentian for cascarrilla—no more verbal directions—no more lumping of six powders into one*. We must conclude with the painful reflection, that most of these charitable institutions in London, as at present managed, are in bad odour among the poor, and produce more discontent than gratitude.

As to their supposed effects upon the fortunes of the physicians and surgeons connected with them, the truth must come out in this age of expositions, confessions, and *éclaircissements* of all kinds: the bubble must soon burst, and we shall not be sorry to see it do so.

LECTURES ON FORENSIC MEDICINE.

WHEN the course on Forensic Medicine, the publication of which we commenced last year, was interrupted by the lamented death of Dr. Cummin, we promised to complete the lectures on this subject from another source, "as soon as we could do so in a manner consistent with the very high character of those already given." We are happy in now having the prospect of doing this, and we this week insert the first of a set of lectures, which, by a remarkable coincidence, are delivered at the same school as the course a portion of which we published. We shall confine ourselves as nearly as pos-

sible to the subjects not treated of by Dr. Cummin, and have reason to believe that the pledge to our readers above quoted will be now amply redeemed by the lectures of Dr. Southwood Smith.

INDECENT MODE OF ADVERTISING.

A SURGEON in the city, who has contrived to establish a dispensary in Charterhouse Square, is annoying all the inhabitants of that district by the exposure of a board in the front of the house, on which is inscribed, in large letters, "Infirmary for Diseases of the Rectum." Notwithstanding numerous remonstrances, both friendly and hostile, he refuses to remove the nuisance; and, we understand, even threatens to insert words which shall make it more offensive. So indecent a mode of attracting attention we never before heard of from any one who occupied the place of a regular practitioner.

REFUSAL OF A CERTIFICATE TO A PUPIL.

WE are informed of the following case having occurred at Guy's Hospital:—A pupil who on the occasion of the late disputes between that hospital and St. Thomas's had been subpoenaed to give evidence in favour of the latter, on applying for his certificates was refused them by two of the lecturers, and referred by them to the treasurer. This gentleman also declined complying with his request, but returned him his money.

The case, as we are informed, is aggravated by the general opinion, which is in favour of the pupil's having been a diligent attendant on the lectures, and on all his studies, and by the circumstance of his being obliged to give evidence against his own school having been the consequence of an expression which he unintentionally made use of, and which seemed to make it probable that the "row" was premeditated by the pupils of Guy's Hospital.

* We once knew of a singular curing by mistake, which took place in consequence of the conglomeration of half a dozen powders. A practitioner had prescribed some tatar emetic for a woman labouring under a fit of indigestion. From whatever cause, the medicine did not operate, and the patient went to a Dispensary. Leeches were ordered to be applied to the right side, and a mass of Dover's powder was given her, to be taken at intervals, in small doses. The directions being given *dispensarially*—that is, in words alone—and being misunderstood or forgotten, the woman took the powder so freely as to produce a state of narcotism, with occasional fits of vomiting. The patient was extremely indignant, but was very much benefited by the vomiting—a vomiting not produced when intended, but which followed one of the most unusual of 11. emetics, the pulv. ipecac. comp.

If the story be untrue, the officers of Guy's ought immediately to contradict it, and we shall be happy to give publicity to such denial; but if otherwise, and if this gentleman can prove his attendance at the lectures of which his certificates are now refused, we should recommend him to have recourse to legal proceedings to save himself from the loss of twelve months or more of study.

CLINICAL LECTURE

ON

VARICOSE VEINS AND ULCERS OF THE LEGS;

Delivered at St. George's Hospital, Oct. 24, 1837,

By SIR B. C. BRODIE, BART.

Varicose Veins continued — Necessity of Rest — Bleeding — Various Applications — Varicose Ulcers — Different kinds of Plaster; mode of applying them — Application of Ligatures — Cutting the Vein across — Sir Benjamin's method of dividing the smaller Veins — M. Velpéau's plan.

I HAD not an opportunity of completing, in the last lecture, my observations on varicose veins of the leg. I explained to you the pathology, the symptoms, and the consequences of the disease; and I began to speak of the treatment which it requires: I shall continue the latter subject in the present lecture.

In those cases in which, from long neglect of varicose veins, the skin of the leg becomes red and irritable, you will be able to render the patient no service so long as he is going about, standing and walking as usual. The first thing to be done is, to confine him to his bed, or at all events to a sofa; but the safest method is to confine him to his bed, and the horizontal posture, so that the blood may not have to rise up in the leg against its own gravity. In many cases nothing more is necessary than this; but, in some instances, this will afford but very slow relief, and in all cases you may hasten the patient's recovery by adopting other methods in addition: I have frequently, in these cases, bled the patient in the vena saphena major, in the lower part of the thigh, near the inner condyle; and it is astonishing what relief that gives. It is not worth while to adopt this practice in all cases, but where you find the patient suffering more than usual from the inflamed state of the skin you may very properly have recourse to it.

Bleeding in the vena saphena major is performed very easily in persons who are not very fat: place a bandage round the lower part of the thigh, let the patient put his leg into a pail of warm water, and what with the warm water below and the bandage above, the vena saphena swells; you then open it with a lancet, and take away any quantity of blood you please. But, in a very fat person, bleeding from the vena saphena is not very easy to be accomplished, and as a substitute for it you may apply leeches to the inside of the thigh, or you may apply them in this situation in other cases where you do not think that actual bleeding in the vena saphena is required. And here I must call to your recollection what I said respecting the application of leeches, under these circumstances, in my last lecture. Never apply leeches to the inflamed part, but always at some distance above it. If the whole skin of the leg be inflamed, then apply them on the inside of the thigh; if the leg be inflamed in the lower part and not in the upper, then apply them in the leg, but above the inflammation. Besides the application of leeches, you may, in the first instance, apply a rag, wetted with cold spirituous or saturnine lotion. When the inflammation of the skin has subsided, you may begin the use of bandages in the way which I described in the last lecture.

In some cases, as I formerly told you, the skin is not only inflamed, but more or less excoriated, the cuticle being abraded to a greater or less extent, while the surface of the cutis secretes an ichorous fluid. Here, also, you may take away blood from the vena saphena major, or from the inside of the thigh by leeches, and the patient will also derive benefit in these cases from the application of a saturnine lotion, though, for the most part, some mild cerate answers the purpose better. The zinc ointment or calamine cerate answers very well; but we use, in the hospital, a preparation known with us by the name of compound chalk ointment, which is much preferable. It is, if I am not mistaken, now introduced into the Pharmacopœia under the name of Ung. plumbi compositum. It is an excellent application in these and other cases where the surface of the cutis is deprived of the cuticle. This ointment was invented by Dr. Kirkland, a celebrated practitioner many years ago in Leicestershire, and I believe it was commonly known under the name of Kirkland's neutral cerate. It is composed of diachylon plaster, olive oil, chalk, and distilled vinegar. How it should have ever entered into any man's head to make such a composition as this I do not know, but the composition having been invented I must say it is a very useful one. The

ointment should be spread on linen rag, and applied in stripes round the leg, each stripe over-lapping the one below. In some cases, in addition to the use of chalk ointment, you will find advantage from washing the surface with a weak solution of nitrate of silver, in the proportion of two or three grains to an ounce of rose water. A strong solution would here be improper, but a weak solution is very useful.

I told you that in some cases there was œdema, a swelling of the leg and foot, in consequence of the inflammation of the cellular membrane, causing it to be infiltrated with coagulated lymph and serum. The treatment that is required under these circumstances is very nearly the same as that which is necessary where there is the inflammation of which I have just spoken. The patient should be kept in the horizontal posture; blood may be taken either from the vena saphena major, or by leeches from the thigh, and generally you will find the latter quite sufficient. You may apply a cold lotion in the first instance, but very soon, in these cases, you should begin to apply a bandage, such as will give an uniform support to the leg from the toes to the knee.

In cases of varicose ulcers of the leg, if you find that the patient has neglected himself, that the ulcer is in a state of inflammation, foul and painful, as it often is, and the surrounding skin being in a state of inflammation also, you must keep the patient in bed, and treat him as if the leg were inflamed without the existence of the ulcer. But as soon as the inflammation of the ulcer and the surrounding parts has been relieved, you may begin the application of pressure. The pressure of a common roller will do a great deal of good, and formerly nothing else was recommended. But we find, now, that in cases of varicose ulcer, as in cases of indolent ulcer of the leg, you may very much assist the common roller by the addition of other means. One very good way of making pressure on a varicose ulcer is to interpose between it and the bandage a piece of sheet lead, such as is used in anatomical museums for covering preparations. The lead should be made quite smooth, and larger than the ulcer, extending some way beyond its margin. This makes a very uniform pressure, and really does very well. But for the most part we are in the habit of using pressure by means of plaster applied in a circular manner round the limb. It is common to employ stripes of linen spread with soap or adhesive plaster, but I own that I very much prefer diachylon plaster, for both soap plaster and adhesive plaster will frequently irritate the skin, and bring on inflammation and pustules,

but diachylon plaster scarcely ever produces this effect.

You have an opportunity of seeing stripes of diachylon plaster applied every day, and over and over again every day, in the wards of the hospital; and, therefore, it might seem almost superfluous for me to make any observations on the mode of applying them. But I find that new dressers very seldom apply them in the manner that I believe to be proper, and therefore I shall offer to you some observations on that subject.

In the first place the stripes should be applied round the limb, the two ends crossing each other in front, the application beginning below the ulcer, and extending some way above it. Each of the stripes ought to overlap the one below by one half of its diameter. Thus every part has a double piece of plaster over it, and you secure more equal pressure than you could otherwise obtain. It is of great consequence that the plaster should be tight enough to give comfortable support to the limb, and at the same time not so tight as to make the limb swell below; for if it does produce this effect, it is very likely that it will bring on a sloughing of the sore. The plasters ought to make uniform pressure — that is, the pressure should be equal throughout; or if there be any difference in the degree of pressure, it ought to be greater below than above. If you do not attend to this point, the plaster above operates as a tight garter, and makes the parts below swell.

When you apply the plaster, it should always be with the heel raised, the patient lying flat on his back, so that the vessels of the leg may be emptied of their blood. The same plan should be adopted when the plaster is taken off. If the leg be hanging down at the time the plaster is applied, the veins are full of blood, and the plaster becomes too loose as soon as the patient puts his leg up.

The plaster, if there be much discharge, should be changed daily; but as the discharge becomes less in quantity, it may be changed every other day, or once in three days, and in some cases it may be left on even longer than that.

Frequently, in cases of varicose ulcer, you find the veins on each side of the leg just above the heel, and behind the ankles, formed into a varicose cluster. A bandage applied in the common manner does not, sufficiently support these veins. The ulcer may be above, and you may cover it with a bandage; but if there be such veins, as I have mentioned, below, you must not, for obvious reasons, leave them uncovered.

In order to support these veins, some stripes of plaster should be applied round the lower part of the heel, ex-

tending upwards in a longitudinal direction on each side of the leg. Let these be held firmly on while you apply the circular stripes over them, in order to keep them in their place. In this case also, in the application of the bandage, you ought to pursue the same course: a longitudinal bandage, extending under the heel and up each side of the leg, should be applied first, and this covered by a circular bandage afterwards. These may appear matters of little importance, but a great deal of your success in practice will depend on attention to such minutiae. It is not enough to understand the case, to make a good diagnosis, and to know what remedies are to be employed; you should also take pains to apply these remedies in the best possible manner, otherwise they may fail in producing their effect. In some cases of varicose ulcer you will promote the healing of the ulcer by touching it every other day with a strong solution of nitrate of silver in water, beginning with five or six grains to an ounce, and increasing the strength gradually. But I do not advise you as a general rule to put any application in the way of dressing under the plaster. I find a new dresser frequently interposing a piece of lint, with or without simple ointment, between the plaster and the sore. It is a very injurious practice; it keeps the sore slopped with its own discharge; it prevents the plaster from making that uniform and regular pressure which is required. When the sore has been healed, the patient should continue to wear the plaster for *some time* afterwards, otherwise the cicatrix will give way, and for the same reason he should *ever* afterwards wear the bandage.

Other methods of treating patients labouring under varicose veins have been proposed by surgeons in former times, and also of late years. They have proposed to relieve or cure the disease by performing operations upon the affected veins. I need not carry you back to the propositions of Celsus on this subject, nor even to those of Heister. I shall only speak to you of methods that have been suggested within the last 30 or 40 years.

Sir Everard Home recommended the application of a ligature, where the veins of the leg were varicose, to the vena saphena major. He performed this operation in a great number of cases, and in a few cases he applied it to the vena saphena minor. When I was a student, nothing was more common than to see a patient with varicose veins standing on a table, and leaning over the back of a chair, to have this operation performed. The skin was divided; a silver needle, armed with a ligature, was passed under the

vein, and the vein was tied. In many instances, at first, no ill consequences ensued; but by and by a private patient of Sir Everard Home became affected with venous inflammation, and died. The same thing then occurred in another patient. When I was house-surgeon here, there were two women on whom the operation was performed, in each of whom venous inflammation, attended by typhoid symptoms, supervened. Fortunately they did not die, but they had a very narrow escape. The operation was performed by other surgeons, and in their hands also it was found that every now and then venous inflammation was brought on, which ended fatally. The operation was then generally abandoned. Mr. Abernethy remarked,—“I dare say it is only the ligature that brings on the inflammation. You divide veins when you amputate, and they do not become inflamed; why should you not merely cut across the vena saphena, and put on pressure?” He was mistaken in his view of the matter, which was not indeed much understood by surgeons at that time. We now know that the veins after amputation not unfrequently inflame, and that this is one of the most common causes of death after amputation. When I was first assistant-surgeon there was a man with very bad varicose veins; such a case as those in which the vena saphena would formerly have been tied. I did not tie the vein, however, but I followed Mr. Abernethy's advice, cutting it across, and applying a compress and bandage. The patient had venous inflammation, attended with very severe typhoid symptoms, and died within four days after the operation. Since then, as you may suppose, no operation has been performed on the vena saphena, either by ligature or in any other way. There are no circumstances here to justify the performance of a dangerous operation. You may perform dangerous operations to get rid of a disease still more dangerous, but you have no right to perform an operation attended with such a degree of danger as can be appreciated, in order to get rid of a disease which is not dangerous; and no one can say that varicose veins belong to the class of dangerous diseases. But still there is another reason against having recourse to this operation. I do not believe, from any thing that I have formerly seen, that the operation permanently benefited the patients. It is true that they appeared to go away a great deal better, but I now and then saw one of them a year or two afterwards, and I always found them as bad as ever. Indeed I am by no means certain that the benefit which the patient appeared to derive, in the first instance, was the result of the operation; and I am more inclined

to believe that it arose from his having been necessarily kept for some time in bed in the horizontal posture. Patients always appear to get better under these circumstances. But I may observe further, that there appears to be no reason why in ordinary cases of varicose veins the obliteration of the saphena major should do any good, and that there are better grounds for believing that it will do harm. If you stop the vena saphena major you prevent the due return of blood to the heart, so that it is likely that the veins will become worse than they were before. Have I not shewn to you that pressure on large venous trunks causes an obstruction of the blood in passing through them? that this is one common cause of varicose veins? In *very bad cases*, however, of this disease, I can understand why the patient should derive benefit from trying the vena saphena major; and in order that you should understand what I now state, I must explain to you the different condition of the parts where the veins are very much dilated, and where the disease has only proceeded to a limited extent.

If the veins are but little dilated, or dilated only in particular places, the valves can still continue to answer the purpose for which they are designed. If the vena saphena major be not at all dilated, while the smaller veins of the leg are dilated, the valves of the vena saphena major act perfectly, and take off the weight of the column of blood pressing on the veins below; but if the vena saphena major be itself considerably dilated, its valves then are of no use. I have sometimes seen a very curious result from this. I had a patient, for example, in whom there was an unusually large cluster of varicose veins on the inside of the leg, while the vena saphena major was of enormous diameter, so that the valves could evidently be of no use. If I put on a bandage and squeezed the blood out of the veins below, and then put my thumb on the vena saphena major above, so as to stop the circulation through it, I found, on taking off the bandage, the patient being in the erect posture, that the cluster of veins below filled very slowly from the capillary vessels. But if, the patient being in the erect posture, I took off my thumb from the vena saphena major, the valves being of no use, the blood seemed to flow down from the trunk of the vena saphena major, contrary to the circulation, and filled the varicose cluster below almost instantaneously. I can understand that a ligature upon the vena saphena major, under these circumstances, would in a great degree lessen the inconvenience arising from the distension of varicose veins below. It would answer the same purpose as the pressure of my

thumb, but still it is not to be supposed that the good thus obtained would counterbalance the chance of mischief resulting from the operation.

I was occupied, many years ago, in making experiments on the obliteration, not of the vena saphena, but of the veins themselves. I applied caustic so as to penetrate through the skin to the veins, and in this way I cured many varicose ulcers. Mr. Mayo has, as I have been informed, employed the same practice lately, with this difference: he has not gone far enough to make a slough of the vein, but brought on some inflammation which has caused the vein to become obliterated. I tried this method in many cases, but I cannot say that I have found it answer sufficiently to make it worth the patient's while to submit to it. The application of the caustic was very painful, the slough took a long time to separate, the sore took a considerable time to heal, and though one cluster was cured, other clusters appeared. Altogether it was a very tedious process, and my own experience does not lead me to recommend it.

Then I contrived another method. Though there is danger in cutting across large veins, or in tying them, there does not appear to be any danger which can be appreciated from the ligature of smaller veins. Piles are nothing originally but varicose veins; now I have performed operations for internal piles, I cannot tell you how often, for there is nothing in the practice of surgery more common; but I have never yet seen a patient have venous inflammation arising in consequence.

We frequently cut across small veins in operations, and they are divided by accident, but we never find venous inflammation supervening. Although there may be danger from operations on the vena saphena major, we have no right to expect danger from operations on the smaller veins. I contrived, then, the following method. Supposing that I intend to cure a particular cluster of veins, I use a sharp-pointed bistoury, which cuts, not like a common bistoury, on the concave, but on the convex edge. I puncture the skin with this instrument on one side of the varicose cluster; I carry the blade under the skin, between it and the varicose veins, over to the other side of the cluster; and having carefully performed this part of the operation, the skin over it remaining entire, except where the first puncture was made, I turn the edge of the instrument backwards, and drawing it out, cut across the cluster. A good deal of hæmorrhage follows, but the pressure of a compress commands it, and a bandage is applied afterwards. The wound, in most instances, heals by the first intention. The varicose

veins are obliterated, and usually in a few days the patient suffers no inconvenience from the operation. However, in some cases, the wound suppurates, instead of healing by the first intention, which protracts the cure. Then, in other cases, a remarkable occurrence took place. Although I was satisfied that the cluster was divided, the disease was not cured. It seemed as if the veins healed without being closed. As the ductus choledochus, or the intestinal canal, will heal after the application of a ligature, without the continuity of the canal being destroyed, so it appeared that the continuity of the canal of the veins was not in every instance obliterated.

This was a very easy and a very safe method of curing varicose veins, yet we hardly ever perform this operation now; for, with my present stock of experience, it really seems to me that there are very few cases in which it is worth the patient's while to submit to it. I have always observed that if I have cured one cluster, two smaller ones have appeared, one on each side, so that ultimately I left the patient no better than I found him.

The operation, however, is proper where there is a varicose cluster much distended, and liable to burst and bleed. Here you may actually save the patient's life by having recourse to it; and you may do so without considering whether fresh clusters are or are not likely to form afterwards. Sometimes when there is a varicose cluster above and below on which a varicose ulcer depends, you get the ulcer to heal sooner than it otherwise would by dividing the cluster. I do not recommend this generally in cases of varicose ulcer, but only every now and then where there is unusual difficulty in getting it to heal. I generally observe that it heals sooner if you divide the cluster below than the cluster above. Then there are some cases where a varicose cluster is productive of an unusual quantity of pain, apparently in consequence of there being some nervous filament lying over it which is kept on the stretch. There you may relieve the patient from the pain of the particular cluster by the division of it. But these occasions are of rare occurrence; and under other circumstances I really do not think that it is worth the while of any patient to submit to the operation.

I ought not to take leave of the subject which is before us, without referring to a very ingenious method of obliterating varicose veins, which has been lately adopted by M. Velpeau, of Paris. He introduces a pin or needle through the skin, which is passed underneath the vein, and at right angles to it. A twisted suture is then applied round the two ends of the pin, so

as to compress the vein sufficiently to produce its obliteration. I cannot, from my own experience of this practice, say any thing of its advantages or disadvantages; but must acknowledge that it seems not improbable that it may be preferable to the other methods of which I have given you a description. Still, the observations which I have made as to these other methods, apply equally to this. It may be useful in certain cases, and under peculiar circumstances; but I can see no reason to believe that you would be justified in having recourse to it on ordinary occasions.

GUY'S HOSPITAL PHYSICAL SOCIETY.

November 4, 1837.

DR. HUGHES IN THE CHAIR.

DR. ASHWELL read a paper on—

Hæmorrhage from the unimpregnated Uterus, associated with Tumors of varying degrees of Induration and Malignancy.

Dr. A. remarked that the following points, in reference to these growths, deserved especial attention. First, They commenced in the parenchyma of the organ, or, in other words, from the tissue or structure inclosed within its peritoneal and mucous coverings, in closer proximity to the mucous than to the peritoneal coat. Hard tumors have more commonly been thought to commence between the muscular and peritoneal coats.

2dly. When they produced the hæmorrhage now alluded to, they grew internally, not imbedding themselves in the walls of the uterus and advancing towards the peritoneum, but by their increase in size partially distending the uterine cavity, and not only stretching and irritating the mucous membrane, thereby altering its condition and deranging its functions, but also gave rise to morbid growth of this tissue; in some measure proportionate to their own bulk.

3dly. They had generally the white lines and the hardness indicative of schirrus, or they possessed a more decided degree of malignancy.

In answer to various questions,

Dr. A. alluded to the cases adduced (four in number), to the preparations, and the drawings; stating that this location of the growth was very singular, and not, as supposed by some, "a matter of such common observation as to be familiar to every one."

In Guy's Museum, out of thirty examples of hard fibrous or schirrous tumors,

twenty-six were situated in some parts of the walls, while only three, or at the most four, were lodged within the cavity. Dr. A. also pointed to the difference between the growths partially and morbidly distending the cavity, as now described, and those entirely filling it up: these latter would probably prevent hæmorrhage by their pressure.

A section of one of the tumors, with a drawing executed by Canton, was shown to the Society.

The tumor was distinctly of a cystiform character, about the consistence of firm steatoma, encroaching on the uterine cavity, which was thus rendered uneven, but still capacious. The mucous lining itself was free from any visible lesion, but was highly vascular, and a web of vessels was seen on that part which was stretched across the tumor. The vascularity of the mucous lining, and the total absence of vascularity in the tumor itself, led Dr. A. to regard the former as the source of the hæmorrhage in question, while the latter, by its mechanical action, operated as the exciting cause of such loss of blood. This difference in the source of hæmorrhage, as observed in polypus and in the tumors alluded to, was a fact to which Dr. A. called the particular attention of the Society.

In the course of the discussion that followed, the question of the malignant or non-malignant character of these hard tumors of the uterus came under the consideration of the Society. It was contended by Mr. Greenwood, Mr. Bransby Cooper, Mr. Roberts, and others, that the evidence and arguments adduced by Dr. Ashwell tended rather to shew that these tumors were non-malignant. Their long-continued quiescent state—the absence of local pain, or constitutional suffering—the exemption of other parts, such as the lumbar and absorbent glands of the pelvis, from a like affection—the age at which they usually occur—and the alleged curability of these structures by means of iodine, as stated by Dr. A., all went to prove that they were at least different from truly malignant diseases elsewhere. To all this it was replied by Mr. Oldham, Dr. Ashwell, and others, that the indolent state of these tumors depended very much upon their situation. When placed in the parietes of the womb there was much space for their growth, and in the absence of exciting causes they remained sometimes years without change. Under the influence, however, of an exciting cause, such as pregnancy, their malignant character was fully developed, and hence Dr. Ashwell had been induced to recommend the adoption of premature delivery in such cases. In glandular parts, too, as in

the cervix uteri, malignant ulceration quickly attacked these tumors. Examination of their structure also shewed that they were of the cystiform character supposed to belong to malignant disease. With regard to age, cancer often occurred in very young persons, and several cases were adduced whose age at the time of death was under thirty years. Dr. Ashwell further strongly vindicated the use of iodine in resolving these tumors, especially when situated in the cervix uteri, by which remedy he was satisfied that ulceration was often prevented, and their increase arrested to an obvious extent.

Other topics of discussion also arose in the course of the evening, in which the proposal to induce premature delivery in cases of pregnancy associated with hard tumors, was canvassed by Mr. Law, Mr. Lever, Mr. J. H. Roberts, &c.

The opinion of Sir Charles Clarke upon the nature, history, and treatment of these diseases, were introduced by Mr. Gazelee into the discussion; but a difference of opinion occurred between that gentleman and Dr. Ashwell as to the true interpretation of Sir C. Clarke's expression. At the termination of the discussion (which was highly animated), a vote of thanks to Dr. Ashwell was unanimously accorded by the Society, upon the motion of Mr. Greenwood, seconded by Mr. B. Cooper.

At the next meeting of the Society, Saturday, Nov. 18, at eight, p.m., Mr. EDWARD COCK in the Chair, Mr. EDEN will read a paper on "Normal and Abnormal Dentition."

PROVINCIAL MEDICAL ASSOCIATION.

PETITION AGAINST THE POOR-LAWS.

In the course of their proceedings at Cheltenham, the "Provincial Medical and Surgical Association" agreed upon the following petition, to be presented to both houses of parliament immediately on their assembling. This was done on the motion of Dr. Webster, of Dulwich, seconded by Mr. Addison.

COPY OF THE PETITION.

The Petition of the President, Council, and Members of the Provincial Medical and Surgical Association, assembled at the Fifth Anniversary Meeting, at Cheltenham,

HUMBLY SHEWETH—

That your Petitioners deprecate the system of medical relief adopted by the Poor-Law Authorities.

That the greatly diminished and insuffi-

cient supply of medical officers must lead to the neglect and injury of the sick poor, and to the increase of those monopolies of parochial practice which were among the acknowledged evils of the old system.

That the extent of medical districts, in general, precludes that prompt and convenient performance of medical duties which is essential to the proper treatment and the safety of the sick paupers.

That the procuring of medical officers by public advertisement and "tender" (a practice degrading to the profession), is injurious to the public, because it cannot afford a sufficient test of the qualifications and practical skill of the candidates, and, consequently, is not calculated to provide humane and efficient attendance for the sick poor.

That dietating, either with or without tender, stipends far below the conventional value of the services required, and any rational calculation of their cost, with the alternative of the introduction of strangers, is another mode of competition equally unfair to the resident and experienced practitioners, and unjust and injurious to the poor.

That vesting in the hands of the relieving and parish officers the power of deciding whether the sick pauper requires medical aid, is calculated to prolong disease and endanger life.

That entrusting to non-professional persons, such as Poor-Law Commissioners, and Boards of Guardians, the power of superintending and controlling, justifying and condemning the conduct and proceedings of the medical officers, is neither just nor judicious, and is not in accordance with the practice pursued in other departments of the public service.

That as the entire system is unjust and derogatory to the medical profession, detrimental to science, injurious to the community, and cruel and delusive to the sick poor, your Petitioners humbly but earnestly hope that your Honourable House will be pleased to direct a special, complete, and impartial inquiry into the subject of medical relief, and grant an order for the production of official returns of all medical contracts made under the Poor-Law Amendment Act; of the number of medical officers appointed, as compared with the former system—of the extent of their respective districts—of the amount of their salaries—of the mode of their appointment—of the number of patients attended, and visits performed by each medical officer; and, finally, enact such remedies as to your wisdom shall seem fit.

And your Petitioners, as in duty bound, will ever pray, &c.

(Signed) H. C. BOISRAGON, President.

ACCIDENTS ADMITTED AT THE LONDON HOSPITAL.

Mr. LUKE, Surgeon.

Mr. CURLING, Assistant-Surgeon.

Oct.	Sex.	Age.	Case.
24.	M.	56	Fractured ribs.
	M.	29	Contused foot.
	F.	40	Sprained ankle.
25.	F.	16	Bite on the ankle.
	M.	72	Large scrotal hernia (reduced.)
	M.	40	Concussion.
	M.	37	Sprained ankle.
	M.	56	Fractured femur.
26.	M.	28	Contused back.
	F.	33	Fractured femur.
27.	M.	30	Fractured ribs.
	M.	68	Contused wound of the eye, with slight abrasion of the cornea.
	M.	32	Contused leg.
28.	M.	17	Contused shoulder and back.
29.	F.	48	Small femoral hernia (reduced.)
	F.	37	Femoral hernia (operated on, (a.))
	F.	18	Cut hand, with division of the radial indicis.
30.	M.	50	Cut head.
	M.	28	Sprained ankle.
31.	M.	82	Fractured neck of the femur.
	M.	45	Compound fracture of the right tibia and fibula, and simple fracture of both bones of the left leg. A slight wound at the back of the head, (b.)

In-patients 21
Out-patients 42

Total 63

(a.) In this case strangulation had existed for twenty-eight hours, and a large portion of omentum contained in the sac was in a state of sphacelus; it was therefore excised. The bowels were not moved till twenty hours after the operation; but all inflammatory symptoms have subsided, and the patient is doing well.

(b.) This patient, a drayman, was thrown from the horse on which he was riding sideways upon some gravel. He was brought to the hospital in a state of great prostration. The lower part of the right tibia was fractured obliquely, and there was considerable displacement of the ends of the bone, which it was impossible to reduce. There was a wound three inches in extent about the middle of the tibia, and a small one of about half an inch at

the fractured part. From both wounds venous hæmorrhage ensued, and, notwithstanding the employment of pressure, continued till the evening, during which time the man must have lost about thirteen ounces of blood.

On the 31st the femoral artery was tied in a spare unhealthy-looking man, aged 39, on account of the sudden rupture of the true sac of a popliteal aneurism. The patient is supposed to have hypertrophy of the left ventricle of the heart; and in the evening the impulse at the seat of the ligature was so violent, that it was thought advisable to bleed him, which was repeated the following day. At present, Nov. 5, he is going on well.

BORATE OF SODA IN DYSMENORRHOEA

DR. DANIEL STAHL, of Vincennes, Indiana, highly extols, in a communication to the *Western Journal of the Medical and Physical Sciences*, (April, 1837,) the efficacy of Borax in the treatment of Dysmenorrhœa. It is not, however, recommended to be given indiscriminately, or without preparing the system for its reception.

"In lymphatic constitutions, and with patients who are debilitated by other diseases," says Dr. S., "the effect is often doubtful, at least not very prompt; whereas in patients of sanguineous or sanguinonervous constitutions, and where there is an actual plethora, it will always be found effectual if venesection be premised. We should never give it without bringing all the systems of the organism to a kind of equilibrium; therefore bleed and give antiphlogistics in plethoric constitutions, raise the energy of the nervous system in 'nervous subjects,' and give tonics to the debilitated.

"After many trials with borax, I determined on the following plan of treatment, and follow it now exclusively.

"In plethoric habits*, I take from four to six ounces of blood from the veins of one of the lower extremities, and repeat the venesection if necessary, and keep the bowels open by Epsom salts. For two days previous to the appearance of the menses, I order a warm foot bath every evening and morning, and give borax in the following formula:

"R. Natri. borac, ʒii.; infusi semini lini. ʒvii. Misce: every two hours a table-spoonful to be taken.

"The pain I relieve by extractum

hyoseyami*, one grain every fifteen or thirty minutes.

"In nervous habits, where I find this complaint in four cases out of seven. I commence my treatment with the exhibition of tinctura valerianæ, teaspoonful doses thrice a day, and previous to the appearance of the menses, I give borax, &c. as above.

"As soon as the menstruation ceases for the time, I treat plethoric habits with slight antiphlogistics, and with the nervous I resume the use of tinctura valerianæ until a day or two previous to menstruation, when I act as before. After the second appearance of the menses, after following that plan, I will generally hear the patient relate with gratification, that the pain was less and of shorter duration, that she menstruated more than usual, and that she feels more contented and cheerful†, and by the third time, the complaint, the mere idea of which haunted the poor woman from one period to the other, is generally removed."

MR. BAKER'S SUBSCRIPTION.

To the Editor of the Medical Gazette.

SIR,

As one of the subscribers to the fund raised under the auspices of that clever and industrious gentleman, Mr. James Baker, for defraying the expenses attending the petitions to parliament respecting the "Medical Witnesses' Bill," I beg to inquire, through the medium of your excellent GAZETTE, how it happens that no public statement whatever of the disbursements has yet appeared, and the more so, as I was induced to advance my money solely on condition that a faithful account of all receipts and payments should be rendered immediately after the affair should be completed. Perhaps, Mr. Editor, some of your numerous readers can inform me whether any such document is likely very shortly to be produced; because if I do not obtain, through your widely circulated GAZETTE, an immediate reply in the affirmative, I shall most certainly take the proper steps to convene a public meeting of the subscribers. Really, Mr. Editor, I have been so repeatedly and shamefully bamboozled out of my money,

* In some cases the extr. hyoseyami produced nausea, and if there was no contraindication I gave Dover's Powder, or what was the most effectual, Aqua Laurocerasi.

† In most cases of dysmenorrhœa menstruation is scanty, and symptoms of hysteria are present.

* It is very rare to find dysmenorrhœa in plethoric habits.

under one pretence or another, that I am positively half inclined to view this matter as a mere job, notwithstanding all the fine professions of disinterestedness made to me by Mr. Baker, when he so earnestly solicited my pecuniary aid in this good cause; and therefore I shall not choose to remain any longer quiet, unless the business is cleared up to my entire satisfaction without loss of time.

Rest assured, that in these sentiments I am joined by many most respectable and influential members of the profession, who parted with their money under the same impression as myself, and who now feel exceedingly indignant at the non-production of any account from Mr. James Baker, who, from his standing as a medical practitioner, can have no pretensions to act in so disrespectful a manner towards his colleagues. Under these circumstances, I feel well persuaded that the immediate insertion of the present communication in your *GAZETTE* will be appreciated as a favour conferred on the whole profession.—I am, sir,

Your most obedient servant,

AN ENEMY TO HUMBUG.

Tuesday Morning,
Nov. 7, 1837.

P.S.—Your estimate of Mr. Baker, as expressed in your journal about October 1835, and the opinion of the Apothecaries' Company, as shewn by their legal proceedings against him, still pending, have most clearly demonstrated that your severe strictures on his conduct were richly merited by him.

BOOKS RECEIVED FOR REVIEW.

A Conspectus of the Pharmacopœias of the London, Edinburgh, and Dublin Colleges of Physicians. By A. T. Thomson, M.D., &c. Tenth Edition, containing the New French and American Remedies.

The Elements of Practical Obstetrics. By Thomas Denman, M.D. New Edition, with Notes, &c., by B. K. Brydes.

The Student's Companion to Apothecaries' Hall. By E. Oliver.

Literal Translation of the Pharmacopœia of the Royal College of Physicians of London. By G. Futroey.

Memoranda on difficult Subjects in Anatomy and Surgery. By R. Druiitt.

COLLEGE OF SURGEONS.

LIST OF GENTLEMEN WHO RECEIVED DIPLOMAS IN OCTOBER.

J. Birkett, Upper Clapton.—D. Reek, Birmingham.—G. Moss, Whiteby, Lincolnshire.—J. Ardagh, Madras.—E. Hetherington, Ropley, Hants.—C. D. Fitzpatrick, E. I.—H. Shaw, Blackpool, Lancashire.—H. D. Croker, Cork.—

J. Parsons, Laventon, Somerset.—J. Harvey, Penzance.—H. Bidwell, Albrighton, Salop.—J. Hunter, Jun., Islington.—J. O'Shaughnessy, Limerick.—T. S. Murphy, Dublin.—J. Meredith, Leominster.—J. MacIise, Russell Place, Fitzroy Square.—H. W. Garde, Middleton, Co. Cork.—D. Sweeny, Cork.—F. Whitwell, Shrewsbury.—W. Francis, Exeter.—J. Houseman, Richmond, Yorkshire.—H. Montgomery, Dublin.—W. W. Baynes, Adstock, Bucks.—W. M. Transdale, Sheffield.—J. Davis, London.—N. Rundle, Calington.—John Paghe, Cochybug, Cornwall.—C. Chapman, Tooting.—J. E. Robertson, Hendred, Berks.—R. V. Gorham, Hollesley, Suffolk.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, Nov. 9, 1837.

John Delaroche Bragge, Clifton, Bristol.—Thos. Bourne, Alford, Lincolnshire.—John Frankard, Langport, Somersetshire.—Charles Millar, Kuntstford, Cheshire.—Henry Johnson Hemingway, Dowsbury, Yorkshire.—John Meredith, Leominster.—Wm. Duckworth, Addingham.—Frederick Gastineau Harrison.—Edward Hunt Butler, Leggatestone, Essex.—John Hope Wraith, Blackburn, Lancashire.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, Nov. 7, 1837.

Age and Debility . . .	15	Heart, diseased . . .	5
Apoplexy . . .	4	Hooping Cough . . .	4
Asthma . . .	6	Inflammation . . .	22
Cancer . . .	3	Bowels & Stomach . . .	6
Childbirth . . .	2	Brain . . .	1
Consumption . . .	41	Lungs and Pleura . . .	5
Convulsions . . .	10	Influenza . . .	1
Croup . . .	1	Insanity . . .	2
Dentition or Teething . . .	5	Liver, diseased . . .	2
Dropsy . . .	5	Measles . . .	5
Drops, in the Brain . . .	7	Mortification . . .	1
Epilepsy . . .	1	Rheumatism . . .	1
Erysipelas . . .	1	Small-pox . . .	5
Fever . . .	13	Unknown Causes . . .	29
Fever, Scarlet . . .	2		
Fever, Typhus . . .	5	Casualties . . .	4
Gout . . .	1		

Decrease of Burials, as compared with }
the preceding week . . . } 266

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.

	OCT.		THERMOMETER.		BAROMETER	
			from 37 to 47		29.04 to 29.14	
Thursday . . .	2		37	46	29.04	29.14
Friday . . .	3		37	46	29.20	29.36
Saturday . . .	4		29	47	29.40	29.91
Sunday . . .	5		31	49	29.95	30.05
Monday . . .	6		28	45	30.17	30.29
Tuesday . . .	7		23	44	30.32	30.29
Wednesday . . .	8		20	39	30.23	30.15

Prevailing wind, S.W.

Except the 3d, 5th, and 8th, generally clear; rain and hail on the 2d, lightning during the evenings of the 2d and 3d, and foggy in the morning and evening of the 8th.

Rain fallen, .15 of an inch.

CHARLES HENRY ADAMS.

WILSON & SON, Printers, 57, Skinner-st., London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, NOVEMBER 18, 1837.

LECTURES

ON THE

PHYSIOLOGY AND DISEASES OF THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL
AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of
1836 and 1837,

At the Anatomical School, Kinnerton-Street,
near St George's Hospital,

By CHARLES J. B. WILLIAMS, M.D. F.R.S.

LECTURE VII.

Physical Examination of the Chest (continued)
—*Sounds of Respiration (continued)*—*The Rhonchi*—Dry Rhonchi, Sibilant, Sonorous, Dry Mucous—Humid Rhonchi, Mucous, Cavernous, Submucous, Subcrepitant, Crepitant—Importance of the Rhonchi as Signs—*Sounds of the Voice*—Natural Sounds, their causes and regions—*Morbid Sounds*: Bronchophony, Ægophony, Pectoriloquy, Metallic and Amphoric Resonance, Pectoral Frenitus—*Sound of Friction*—*Tabular View of the Sounds of the Organs of Respiration.*

WE have hitherto considered the sounds produced by the passage of the air to and fro in the lungs, and we found that the varieties of these sounds depend on the size of the tubes and on the force with which the air strikes against their sides and angles, and that they may be shortened or stopped by various kinds of obstruction.

We have now to attend a little to a class of novel sounds which arise from partial obstructions to the passage of air,—obstructions which permit the air to pass, but not without such a resistance as causes an increased and modified sound. Thus,

suppose a bronchial tube to be narrowed by the swelling of its membrane, or by mucus secreted by it, the air must pass through the narrowed portion with increased velocity and increased resistance, and hence the sound is changed from a simple breathing or blowing, to a louder wheezing, bubbling, whistling, or snoring, according to the nature of the obstruction. These new sounds Laennec called *râles*, or rattles; I have preferred the Latin term *rhonchus* (which is from the Greek *ῥογχος*), as more expressive, and this has been adopted by most English writers. I wish we had an English word that would do as well; for nothing injures the purity of a language more than the introduction of foreign words: and it is only pedantic to use them when English terms will express the same thing. And here, in relation to our subject, I enter my protest against the prevalent use of French words, in English speaking or writing, such as *bruit de soufflet*, *bruit de râpe*, *bruit de pot fêlé*, *frôtement*, *gargouillement*, *tintement métallique*, &c.;—the simple translation of which would do just as well, and convey much more precise and intelligible ideas than these mystifying terms. We cannot, however, do well without the word *rhonchus*, or some other to be used conventionally in the same sense, as a generic term applicable to all those sounds which are produced by an increased resistance to the air moving through the lungs.

Let us attend to some of the species of rhonchus. I divide them into the *dry* and the *humid*, according to whether the impediments that produce them are solid or liquid. Of the dry rhonchi there is the *sibilant* or *whistling rhonchus*, which is sufficiently described by its name, and may be generally imitated by whistling between the teeth. It is produced by the passage of air through a small and rather circular aperture; and this aperture may be formed by a slight obstruction of a small tube, or

by a greater obstruction in tubes of larger size. It generally occurs in tubes narrowed by swelling of their mucous and submucous coats, such as occurs in the early stage of acute bronchitis; but you have it also in asthma, where the tubes are congested and constricted by the spasmodic contraction of their circular fibres; and you may have it also when viscid mucus clings to and diminishes the calibre of the tubes.

The *sonorous rhonchus* is a snoring, humming, or droning sound, and may vary in loudness and key from an acute note like that of a gnat, down to the grave tone of a violoncello or bassoon. It must be produced by obstruction leaving a flattened aperture, the lips of which, or the moisture on them, yield to the passing air with a vibrating resistance. Partial swelling of the sides of a tube, particularly at its bifurcation, a pellet of tough mucus in it, or external pressure on it, may cause such a flattened opening within the tube; and the sound in question therefore occurs in various forms of bronchitis, and often accompanies tumors which press on the bronchial tubes. When caused by tough phlegm, coughing will generally change or remove it; when from the other causes, it is generally more permanent. When quite permanent, it usually depends on the pressure of a tumor or some deposit outside the tube. The key of this note depends chiefly on the size of the aperture left; when this is small the note is high, when large the note more of a bass: from this we may infer that the latter can only have its seat in the larger tubes; but as a more considerable obstruction may flatten their calibre to the smallest size, these may also be the seat of the acute notes. You may imitate almost every variety of this rhonchus by blowing between the lips moistened with saliva and almost closed. This is the mode in which M. Alexandre imitated the buzzing of a fly.

There is another rhonchus which I call the *dry mucous*, because it is produced by a pellet of tough mucus obstructing a tube, and yielding to the air only in successive jerks, which cause a ticking sound, like that of a click wheel. When the air is driven very fast, these, as in the case of other click-sounds, pass into a continuous note, and constitute the *sonorous rhonchus*. Sometimes, again, particularly in inspiration, the click-sound suddenly stops; the tough mucus being forced into a smaller tube which it completely closes, and may not be dislodged again, but by dint of forcible coughing.

Now, as any of these rhonchi may be produced in only one tube and yet make a great deal of noise, you are not to suppose that they are important in proportion to

the noise they make. It is rather when they are very permanent, or when several of them are heard at once in different parts of the lungs, that they bespeak disorder which may be serious, either from its permanency or its extent.

The *humid rhonchi* all depend on the passage of air, in bubbles, through a liquid in the lungs, and their species are produced by differences in the size of the tubes, and the nature and quantity of the liquid, which cause varieties in the bubbling sound. Do not suppose I am trifling with your time because I ask your attention to some of these *bubbles*. Newton learnt much by studying the colour of a soap bubble, and he still left much in it for others to explain: perchance, we may learn somewhat by studying the sound of the bubbles in the lungs; at least we should lose nothing, as others do, by their *bursting*. A bubble is a portion of air contained and slightly compressed by a thin film of liquid, which preserves its continuity by its molecular or aggregative attraction: when this attraction is overcome, by the gravitation of the liquid, the motion of the air, or any other disturbing cause, the bubble bursts: as it bursts, the air from it, slightly expanding, gives to the adjacent air an impulse, which, if forcible enough, produces sound. In the bubbling passage of air through a liquid, the air is the moving body; the liquid gives the resistance, and in proportion as these are strongly and suddenly opposed to each other, the louder will be the sound produced. If the air pass with force it will make most noise in a liquid of some tenacity which offers to it most resistance; but if it move slowly, such tenacity may retard the breaking of the bubbles, and therefore diminish the sound. Again, air passing through liquid in large tubes will give most sound when the liquid is thin, because the bubbles form and burst quickly; but in passing through very small tubes air will cause more sound with a rather viscid liquid, which, adhering to the tubes, is not carried before the air so readily as one of a thinner nature. Now, let us apply this rule to the bubbling sounds or rhonchi heard in the chest.

The *mucous rhonchus* may be heard in the large and smaller bronchi down to the size of a crow's quill, and in these different situations its gurgling or crackling presents different degrees of coarseness. It is an irregular and varying sound composed of unequal bubbles, and generally interspersed with some whistling, chirping, or hissing notes. Its most common cause is acute bronchitis, which after its onset is attended with a secretion of liquid mucus into the bronchial tubes; and the passing of the sibilant and sonorous rhonchi of the

first or dry stage into the bubbling of the second or secreting stage is often marked by a curious combination of chirruping and cooing notes, like those of birds in a bush. When the bronchial tubes become unnaturally enlarged by disease, or when morbid cavities are formed by the destruction of portions of lung, the bubbling of air through liquid in these, is of the coarsest kind; it is quite *gurgling*, and if the liquid be scanty, has a hollow character, and is called the *cavernous rhonchus*.

When there is a little liquid in the smaller bronchi the bubbling or crackling is more regular, although the sound is weaker, and is sometimes only a roughness added to the ordinary respiratory murmur. This is the *submucous rhonchus*. It may result from slight degrees of bronchitis, and owes its importance only to its being permanently present when such slight inflammation is constantly kept up by the irritation of adjacent tubercles in an incipient state.

When there is more liquid, not viscid, in the smallest tubes and terminal cells, the rhonchus has a still more crepitant character, and resembles that heard on applying the ear near the surface of a liquid slightly efferverscing, such as bottled cyder or champagne. This is the *subcrepitant rhonchus* which is heard in œdema of the lungs, humid bronchitis, and other affections in which liquid and air occupy the extreme tubes, and are forced through each other in the motions of breathing.

But the most perfect and equal crepitation is that of peripneumony, called the *crepitant rhonchus*. It exactly resembles the sound which you can produce by rubbing slowly and firmly between your finger and thumb a lock of hair near your ear. I believe that this sound depends on the forcible passage of air through a little viscid liquid in the finest tubes narrowed by congestion and deposit around them: but we shall speak more of this when we come to treat of peripneumony.

Of all these different rhonchi we may say what we did of the morbid sounds of respiration, that they may occupy the whole of the respiratory movements, or be confined to part of them. Thus an obstruction which is sufficient at the commencement of inspiration to cause a rhonchus, may be insufficient when the tubes are dilated by the distension of a full breath; and you may have the converse, an obstruction which is total in low degrees of respiration and stops all sound, in forced and extensive efforts, as in coughing, occasions a rhonchus. This suggests to us the propriety of using these different degrees of respiration to test the nature and extent of bronchial obstructions. You may also

gather from what has been said that the different stages and degrees of force in respiration may change the note of the different rhonchi, and thus produce a variety of music, which we do really hear in the chests of some catarrhal and asthmatic patients. Laennec used to call this piping sound, *rhonchus canorus*. After the explanations which I have given, you may easily perceive that these several rhonchi may be variously combined, or exist at the same time in different parts of the lung, and give rise to numerous combinations that I need not dwell on. I before observed that the loudness of a sonorous or sibilant rhonchus is no proof of the severity of the disease, nor is the fact of its being audible over the whole chest, unless the respiratory murmur be at the same time absent, or very feeble in parts. But the presence of the bubbling or crepitant rhonchi does imply mischief proportioned to its extent; and if they are heard over a large space, and accompanying the whole act of respiration, diminishing or destroying the natural respiratory murmur, they denote disease of a very serious character, because, as your hearing informs you, there is an obstructing liquid in the tubes, where there ought to be only air.

So much for the sounds produced by the passage of air.

We have now another class of sounds to examine—the sounds of the voice as transmitted through the chest. We have found that sounds produced by air passing in the lungs are transmitted to the ear on the surface of the chest; and why should not the sounds of the voice, which are strongly communicated to the same air, be transmitted in like manner, modified by the size of the tubes, and the nature of the substance through which they pass? So in truth they are, and if carefully attended to, they may thus become signs of the condition of the organs that transmit them.

If you apply your ear to the throat, or upper part of the sternum, of a person whilst he is speaking, you will hear the voice so loud that you might fancy he was speaking into your ear, only the articulation is not as distinct. The reason of this is plain enough. The sound of the voice, although originating in the vibration of the glottis, is propagated to the air above and below it; that below being pent up is not heard without bringing the ear into contact with the parts where the tubes run, and it there resounds with all its force. This is called *tracheophony*, or the natural tracheal voice.

But when the trachea divides and subdivides, there is not only a splitting of the

vocal sound into smaller pipes, and a consequent diffusion of it, and a reduction of its volume, but at this division the tubes plunge into the spongy tissue of the lung, which, as we have before found, is a bad conductor, and tends to stop the sound. Hence, over the chief bronchial ramifications, as on each side of the upper part of the sternum, at and between the scapulae and in the axillae, you still hear the voice, but it is less in a body, more diffused and more distinct, than in the other parts, and the articulation is still less distinct. This is natural *bronchophony*, or bronchial resonance.

What becomes of the voice in other parts of the chest? Why, as it gets into the finer tubes with their more flaccid coats and minute cells, its vibrations are either choked and destroyed, or in some parts may be transmitted across the tissue to the parietes in merely an obscure diffused *fremitus*. This I call the *pectoral fremitus*. It may be also felt by the hand applied on the chest.

Now before we touch on the modifications of these sounds by disease, I must say a few words on some natural varieties and their physical causes.

Natural *bronchophony*, or the vocal resonance in the bronchial tubes, is most distinct in thin persons with a high or treble voice, as in females and children. Shrill or treble notes penetrate further into the small tubes, because their vibrations are less excursive and need less room than those of a deeper tone. You remember how we noticed this difference in the cords of the guitar. The motions of the treble cords were so short and quick that you could scarcely see them, whilst those of the bass were long and comparatively slow. Now what will be the effect in a person with a bass voice? Why, the voice will scarcely go into the subdivisions of the tubes, and there will be little or no *bronchophony*; but if the voice be strong it will not be entirely lost, for it will pass *across* the whole spongy tissue, and throw it all more or less into a diffused vibration, which will be heard and felt in many parts of the chest, in the character of *pectoral fremitus*. We find, then, that treble tones of the voice give more of *bronchophony*, and bass ones more of the *pectoral fremitus*; and the same occurs with the morbid sounds: if we can get our patients sometimes to change their tone of voice, we may thereby more effectually test the condition of their pectoral organs.

Now, as with the corresponding varieties of respiratory sound, so with these sounds of the voice, they become signs of disease when they are heard out of their

proper places. But you will say, as there is some natural variety in different individuals, how shall we know what are their proper places? The diagrams which you see here, and in my work on *Diagnosis*, before mentioned, and your own anatomical knowledge, will guide you in a general way on this point, by pointing out in what regions the different sized tubes lie; but there is another standard more applicable to individuals, and that is, comparison between the two sides of the chest. As there is an approach to symmetry in the structure on the two sides, so there is in health a general correspondence between their sounds; and as disease scarcely ever affects both sides at the same time, or in the same degree, it will make the phenomena of one side to differ from those of the other. For example, if under one clavicle you hear the voice resound loudly, whilst it is scarcely heard under the other, you may be sure that there is some physical difference between the two sides, which does not exist naturally; or if you hear below the third rib in front the tubular or bronchial voice, which is generally confined to the immediate neighbourhood of the large bronchi, you may also infer that there is an altered condition of the parts. And now let us see what alterations will change the natural disposition of the sounds.

Increased density of the pulmonary tissue by solid or liquid effusion, or even great sanguineous congestion in it, will improve its conducting power, and will enable it to transmit from the bronchial tubes within, the vocal sounds which they receive from the trachea. This is *morbid bronchophony*, and it is usually accompanied with bronchial respiration. Well, then, if you hear the voice resounding in a part of the chest where it is not usually heard, you may suspect the lung there to be somehow increased in density; but you cannot be sure until you shall have tested this by other means, for there is another change which may have a similar effect. If, instead of the sound being better conducted from within, it is increased in strength and extent by an enlargement of the bronchial tubes, it may then be heard in situations where it does not naturally reach the walls of the chest. In both cases it may more or less resemble the natural *bronchophony* heard near the top of the sternum and between the scapulae; but it often presents remarkable modifications. Thus, when transmitted from the middle-sized bronchi, it comes rather as *diminutived bits of voice* than as articulate words; and for reasons before mentioned low tones are not transmitted; so that if the patient varies his cadence, some words

are heard, and others not. When arising from dilated air-tubes, or when transmitted from the larger tubes, the resonance is more noisy and continued, varying less with the tone of the voice. If the air-cells over the resonant tubes are still open, the sound will be diminished when they are dilated by a full inspiration, because they then tend to intercept it more. The loudest bronchophony is caused where the middle or upper parts of the lung are pressed against some part of the walls of the chest by a liquid effusion in the pleura, which cannot displace the lung from that part, because it is bound to it by old adhesions.

But what modifies the transmitted voice in the most remarkable manner is a thin layer of liquid between the lung and the walls of the chest. The liquid is thrown by the vocal resonance of the lung into a state of irregular vibration, which causes it to transmit the voice in a broken tremulous manner, so that it sounds to the ear outside like the bleating of the goat. Hence Laennec called it *agophony*. It may be produced simply by liquid in the pleural sac, without disease of the lung; for the compression of the pulmonary tissue caused by the liquid is enough to enable the lung to transmit the voice from the bronchial tubes within it. When the lung is consolidated also by disease, the vocal resonance is stronger, and there is a loud bronchophony mixed with the bleating voice, constituting a kind of buzzing voice, which Laennec compared to that of *Punch*.

But there is another kind, which may be called the perfection of vocal resonance, in the chest. When a cavity is formed in the lung by the emptying of a vomica or abscess through the air-tubes, the voice will pass from these tubes into it; and if the communication is free, the voice may, by the ear applied outside, be heard in the cavity as loud and distinct as it is in the trachea. This is *pectoriloquy*—not only *voice*, but *speaking*, in the chest. When the cavity is near the surface, of moderate size, and opens freely into a large air-tube, the phenomenon is most perfect, and then sounds exactly as if the patient spoke into one's ear: this is limited to the spot where the cavity lies, which is thus, as it were, a little *island* of voice, and when so it is a sure sign of a cavity. The sound of bronchophony is often louder, but then it is more diffused, and there is less distinctness in the words. But I shall not now dwell on these distinctions, because we shall speak of them when we come to treat of the diseases which cause these phenomena.

When the cavity is large, and the opening into it small, the voice may not fully get into it, but there may be a hollow or tinkling reverberation in it, like that in a phial, which is one of the modified echoes from repeated reflection that I told you of in my lecture on sound. This is called *amphoric resonance*, or *metallic tinkling*, according to the character of the sound. It may be produced in the cavity left by a large vomica or abscess, or by several of these running together; but its more common seat is the sac of the pleura, into which air has entered through a fistulous opening from the lung. Now, this being the resonant or echoing cavity, you can easily perceive that not the voice only, but the breathing and cough also, especially if they be accompanied by a bubbling through the fistula, will have more or less of this tinkling or bottle sound.

Besides these various positive phenomena of the voice, the absence of the vocal fremitus is sometimes a valuable sign. I have said this pectoral fremitus can be felt as well as heard; and if you apply your hands, one on each side of a healthy chest, you will feel the vibrations on both sides alike. Liquid in the pleura will generally, more or less, destroy this fremitus, and the difference which it produces is often a very valuable sign of the presence of liquid. Consolidation of the lung, again, will increase the vibrations, or make them stronger over the bronchial tubes. In cases where one side is quite dull on percussion, you may often thus easily distinguish whether the dullness is caused by consolidated lung, or liquid in the pleura—a point of considerable importance in practice.

Besides these sounds produced by the air and the voice, you may sometimes have a sound produced by the motions of the lungs against the ribs. You know that the lungs, although they nearly follow the motions of the chest, do not move quite with it, especially in the lower parts, where the descent of the diaphragm draws the lungs downwards, whilst the ribs are rising. But in the natural condition, the surfaces of the pulmonary and costal pleura are so smooth, and so well lubricated with serum, that, although there is motion, there is not resistance enough to that motion to cause sound. But suppose these surfaces to become uneven by the deposit of rough matter on them, or by an irregular distension of the tissue by solids or air under them, you may then have a *rubbing sound* with the motions of respiration; and this does really occur in pleurisy and emphysema of the lung. This rubbing sound is often the more evi-

dent in these cases, because the same disease, by preventing the proper expansion of the lung, causes less harmony than usual between its motions and those of the chest.

Now, gentlemen, I have described and explained to you most of the acoustic phenomena of the chest connected with respiration. Those of the heart will be considered in the latter part of this course. Perhaps you may have thought me somewhat minute; I do not expect you to remember the details of what I have said; but if your understanding has accompanied me as I proceeded, you must have become pretty familiar with the *principles*, of which the various phenomena are illustrations. I would rather not encumber your minds with too many details, but I do wish to give you a sound knowledge of

the laws under which they occur; for from these, and a good knowledge of pathology, you may understand and deduce, not only the phenomena hitherto observed, but new forms and combinations of signs which observation and practice may bring before you. In our description of diseases we shall meet with these phenomena again; and from the principles which we have now developed, we shall be prepared for them wherever they may occur. In the mean time, I will leave for your inspection these tabular views of the acoustic phenomena of the chest, and the names by which they have been distinguished. They should be studied in conjunction with others which you will find at the end my treatise on the Signs and Pathology of Diseases of the Chest.

SOUNDS PRODUCED BY THE PASSAGE OF AIR IN RESPIRATION.

SOUND OF RESPIRATION.—*Natural*: produced by collision of the air against the sides and angles of the air tubes.

Tracheal; heard in the neck and at the top of the sternum.

Brachial; near the upper parts of the sternum, between the scapula, &c.

Vesicular; in most other parts of the chest.

Morbid, modified in production or transmission.

Bronchial, or whiffling; transmitted from the bronchi by condensed tissue of the lung.

Cavernous } produced in morbid cavities communicating
Amphoric } with the bronchi.

Amphoric } with the bronchi.

RHONCH, produced by increased resistance to the air moving through the lungs.

Dry; Sibillant Sonorous Dry mucous	} produced by viscid mucus in the bronchi, or by swelling of the membranes, or by pressure upon them.
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Sonorous

Dry mucous

Moist; Mucous . . { produced by a bubbling } liquid in the bronchi.
 { passage of air through }

Submucous :

Subcrepitant	liquid in the smallest bronchi.
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Creepant viscid liquid in compress

elephant } smallest bronchi.

Cavernous liquid in a morbid cavity.

SOUNDS OF THE VOICE TRANSMITTED THROUGH THE CHEST.

NATURAL SOUNDS, *heard in a healthy chest.*

Tracheophony, in the neck and at the top of the sternum.

Branchophony, near top of the sternum, between the scapulæ, in the axilla, &c.

Pectoral fremitus, in many parts of the chest.

MORBID SOUNDS, transmitted or produced by a diseased chest.

Bronchophony, transmitted by condensed pulmonary tissue.

Ægophony, the same vibrating through a thin layer of liquid.

Pectoriloquy, resounding in a cavity in the lung.

Tinkling, a changed echo of the voice or cough in a large cavity.

SOUNDS PRODUCED BY THE MOTIONS OF THE LUNGS.

Sounds of friction, when the pleurae are dry, or rough from deposits.

Emphysematous crackling, by the irregular passage of air between the lobules.

LECTURES
ON
RHEUMATISM AND ITS EFFECTS;

BY R. MACLEOD, M.D.

Physician to St. George's Hospital.

LECTURE IV.

Synovial Rheumatism; its seat—Difference between it and the preceding form—Permanent Changes produced in the affected Joints—Nature of the Deposit—Carbonate of Lime, and Lithate of Soda—Suppuration in the Joints an occasional consequence of Synovial Rheumatism—Causes and Symptoms of the Disease.

If being different in their seat, in their symptoms, in their terminations, in the affections with which they are complicated, and in their treatment, be sufficient to prove that a distinction ought to be made between two diseases, then is rheumatism affecting the synovial membranes essentially distinct from the form of rheumatism already considered.

The seat of synovial rheumatism (as, indeed, the name implies) is in the lining membrane of the joints; being, in this respect, the same as gout, to which it is, in various respects, very closely allied. The parts most liable to its attacks are the feet and hands, where it is, for the most part, easily recognized by the enlargement of the small joints; but the peculiar characters of the disease are, perhaps, most strikingly displayed when it attacks the knees. At first there is pain and heat, which, in the more severe cases, are attended by external redness; and after a time, varying from about twenty-four hours to several days, tumefaction of the parts takes place. The appearance of the swelling, which is limited to the immediate vicinity of the joint, points out the nature of the affection. The effusion is within the synovial capsule, which is consequently distended, and projects at those points where it meets with least resistance. This gives a peculiarity to the shape, which at once strikes the eye. In the knee, the fulness is most conspicuous on either side of the patella and across the lower part of the thigh, just above the joint: frequently also it projects backwards, and may be seen or felt between the hamstrings. Even where the inflammation extends to the surrounding textures, they participate much less in this than in the preceding form of the disease, and the swelling is consequently much less diffused. In the synovial rheumatism the swelling is fluctuating, and bound down by the ligaments of the joint; in the common acute rheumatism, the effusion is external to the capsular ligament—chiefly in the cellular membrane—is not fluctuating, but occasionally pits very slightly, like inflammatory œdema. The shape and aspect of the two thus differ strikingly from each other.

Nor are the histories of the two diseases less remarkable for their points of difference. The synovial rheumatism is infinitely more persistent—affecting, for the most part, several joints, but generally becoming more especially fixed in a limited number, to be ultimately completely localized, and in a certain proportion of them to produce permanent changes of structure, or even disorganization of the parts. In general, it is probable that the effusion into the joint consists merely of an increased quantity of synovia, produced by the excited condition of the secreting surface. Such, at least, was the case in an instance wherein I very lately had an opportunity of examining the body of a patient cut off by another disease, while labouring under a first attack of synovial rheumatism. The joint chiefly affected was the knee; it contained an increased quantity of synovia, and the lining membrane, except where reflected over the cartilages, was of rather a deep red colour, and tumefied. But where the attacks have been repeated, or have expended themselves upon particular joints, much more considerable changes take place.

When the disease recurs several times, the recovery after each attack becomes less and less complete; the joint remains perceptibly swollen; the ligaments which bind the extremities of the bones together become stretched; the relative situations of the articulating surfaces are altered, and more or less distortion is the result. Sometimes the quantity of synovia in the joint becomes permanently, though not very considerably, increased, the disease assuming an entirely chronic character; and in several such cases, where I have examined the joints after death, I have found little or nothing which could be called disease, with the exception of a preternatural quantity of synovia. In others, the synovia, which, during the acuter attacks, becomes increased in quantity, is again absorbed as these subside; but about such joints, particularly after repeated attacks, some degree of thickening remains. This is most remarkable about the small joints, particularly those of the fingers and toes, which are often considerably distorted by it; and when, as sometimes happens, it extends to the larger joints also, it is diffi-

cult to imagine any thing more helpless than the subject of such disease ultimately becomes. Fortunately it has almost invariably a chronic character, and is attended with comparatively little pain. This is what Dr. Haygarth described, many years ago, as *nodosities* of the joints, but apparently without understanding its nature.

Another appearance which sometimes presents itself is peculiar: the surface of the cartilages of the joint seems as if sprinkled over, in spots of greater or less extent, with white powder, or smoothly painted of the same colour. When we try to scrape this off, we perceive that it is not merely superficial, but pervades the substance of the cartilage. The form of this deposit is thus considerably different from that which takes place in acute and well-marked gout, wherein it exists in small, irregular, detached portions, or as a kind of exudation which may even escape externally. But another difference, and one which would be very important if it were general, has been pointed out as existing between them. The chalk-stones, as they have been called, of gout, are well known to consist of lithate of soda; but in several cases of old synovial rheumatism which occurred to Dr. Chambers, the white deposit was found to consist of carbonate of lime. This is a curious pathological fact; but I am satisfied, nevertheless, that the difference between the deposit in such cases and in true gout, is not such as to constitute a safe ground of distinction. I have several times examined this smooth white deposit on the articular cartilages of those who have had the synovial form of rheumatism, and in all such instances it answered to the tests of lithate of soda; and on mentioning the subject to Dr. Prout, he informed me that, in the course of his experience (which we know in such cases to have been very extensive), he had never met with a deposit of carbonate of lime in the joints. I think, therefore, we may safely infer that this, though an occasional, is still but a rare occurrence.

But there is yet another change sometimes produced by this form of rheumatism—I mean suppurative disorganization of the joint in which it is situated. This is contrary to what seems to be very generally believed, and almost invariably laid down in books; but I believe, nevertheless, that it will be found, on more extended observation, to be a more frequent occurrence than has been suspected. Less, indeed, is known concerning the morbid changes which take place in the primary seats of rheumatism, than of any other portion of its history; nor is it difficult to explain

this, for very few die in the early stage of the disease; and when the patient is cut off at a more advanced period, it is in consequence of the affection of some internal organ, which either absorbs all the attention of the practitioner, or it may be, that long before the fatal event the joints have become free from the disease, as happens in the great majority of cases where the heart has been so damaged as to prove incompatible with life. Nor must I neglect to mention another circumstance—viz. that in cases which have ended in suppuration, the very fact of such termination is assumed as *ipso facto*, proving that the disease had not been rheumatism, but ordinary inflammation.

I have never seen any form of rheumatism except the synovial, in which suppuration has occurred; but of this I have seen several instances:—

A lady, about 55 years of age, of delicate constitution (residing at Chelsea), became affected with the ordinary symptoms of synovial rheumatism, affecting principally the shoulders and hips. In about a week the right shoulder joint became distended, and after a few days the articulation of the clavicle with the sternum was swollen and fluctuating. The right hip next became the chief seat of pain; and it was thought that deep-seated fluctuation, apparently from distension of the capsular ligament, could be felt. Sir Benjamin Brodie at this period saw the patient with me, and regarded it, as I had done, as a severe case of synovial rheumatism. She died at the end of a month from the commencement of the attack, without any internal organ participating in the disease, but apparently exhausted by paroxysms of fever, attended by copious perspirations. No examination was allowed; but the impression produced upon my mind at the time was, that the inflammation had run so high, as to give rise not merely to an increased secretion of synovia, but that purulent matter had been formed in the affected joints.

A man was admitted into St. George's Hospital, under my care, in 1835, affected with pain and swelling of the knees and hips. The pains next attacked the wrists and shoulders, the former of which were swollen from distension of the synovial membranes, and slightly red externally. The pain was also severe and long-continued in the left shoulder. But the chief affection from the onset consisted of severe pain in the right hip-joint, with a considerable tense elastic fulness in the groin, apparently connected with the joint. The pain and swelling of the other parts subsided in the course of a few weeks, while that of the hip joint continued.

The patient lingered for several months; and on examining the body after death, matter was found in and around the joint, with ulceration of the cartilages, and destruction of the ligaments.

Now this I regard as a case of synovial rheumatism, in which, owing to the concentration of the action on one joint, its disorganization was produced. In the first instance no doubt was entertained, or could have arisen, as to the nature of the case, its history being that of common synovial rheumatism; but had the patient not been seen till the successive affection of one or more joints had passed away, while that of the hip alone remained, the case might have been regarded merely as one of common disease of the hip-joint unconnected with rheumatism.

A woman was brought into St. George's Hospital, who was stated to have had rheumatism of a very acute character, and was treated for that complaint by a medical practitioner at Kensington. She complained of pains in her limbs generally, but particularly in the right hand and left hip. The knuckles were swollen, the tumefaction taking the shape of the joints. The first joint of the fore-finger was especially tumefied and red. She had been ill a month, and several joints had been affected in succession. She was admitted in a moribund state, and died next day. The affected knuckle contained a tea-spoonful of thin pus, with redness of the membrane round the edge of the cartilages. The left hip-joint had a thin layer of pus infiltrating the cellular membrane, external to the capsular ligament, and within it was filled with thin pus mixed with flakes: there was redness of the capsular ligament, but no ulceration of the cartilages.

George Coombs, ætat. 31, was admitted into St. George's Hospital the 23d of November, 1836, at which time he had suffered for a month from pain in various joints. He had then, however, no pain remaining, except in the left knee, where it was still severe. There was no swelling or redness, and he could bend it without difficulty, but complained much when the limb was straightened. There was little general disturbance.

Nov. 30th.—Having exposed himself to cold, he had a rigor, followed by heat and copious acid sweating. To-day has pain in the limbs generally, but particularly in the knees and shoulders, accompanied by a good deal of fever.

By the 15th December he was much better, and especially with respect to the affection of the left knee, for which he was originally admitted; but on the 19th he had a return of pain in both knees, parti-

cularly the right, the synovial membrane of which was distended.

On the 24th the left ankle was painful and swollen, and also the left elbow, which was red externally. His pulse was 116, and his countenance anxious.

26th.—Knuckle of left middle finger swollen, with perceptible distension of the joint, and redness of the skin. The copious acid perspirations continue.

31st.—The swelling of finger and elbow very much diminished; and chief complaint now is of right knee, which continued much swollen, the tumefaction being dependent on distension of the capsular ligament, accompanied by fluctuation. The limb retained habitually in a half-bent position.

In this state he continued, with little change, till the 12th Jan., when he died.

The body was examined next day. The right knee was found to contain about half a pint of thin purulent matter, with some flocculi. The cartilages were entire, and of healthy appearance; but the synovial membrane was distended, pushed up beneath the quadriceps muscle, red, and thickened. The metacarpal joint of the finger which had suffered was next examined. The flexor tendon was softened, and seemed sodden, being deprived of its silvery lustre; the articular surfaces red, roughened, and smeared with a thin layer of pus. The left ankle, which seemed to have been third in the degree of suffering, was examined, but presented no morbid appearance beyond a little redness. Neither the pericardium nor the heart presented any morbid change, unless a slight, apparently old, thickening of the mitral valves. There were between two and three ounces of fluid in the ventricles of the brain.

Now it appears to me impossible to separate such cases as the preceding from rheumatism, unless we admit change of structure in the part affected to be in itself sufficient proof that the disease had been something else—a mode of reasoning both unphilosophical, and calculated to establish an embarrassing, and I believe imaginary distinction, between those cases in which disorganization is, and those in which it is not, the result of those affections of the joints commonly regarded as rheumatism.

Again, abundant evidence will be found in the writings of those who have attended most minutely to the changes of structure which the joints undergo, to convince us that rheumatism which becomes fixed in a part may there give rise to palpable changes of structure. The fourth case, which occurs in Sir Benjamin Brodie's work on the Joints, is conclusive on the subject:—

“Henry Payne, 30 years of age, was ad-

mitted into St. George's Hospital, under the care of Mr. Hawkins, on the 7th of October, 1829.

He had suffered formerly from repeated attacks of rheumatism.

About twelve weeks ago, after exposure to damp and cold, he was seized with inflammation in nearly all his joints. In the course of a few days, the disease in the other joints had abated; but the right knee became more painful and swollen. At the time of his admission, this knee was tender, painful, and much disended with fluid, and there was a good deal of febrile excitement of the system.

Blood was taken from the neighbourhood of the knee by cupping; and this was followed by the application of blisters. The *vinum colchici*, and afterwards colomel, combined with opium, were administered internally. Under this treatment the pain and swelling of the knee subsided.

On the 27th of October he was attacked with severe inflammation of the fauces and larynx; which, however, soon yielded to the remedies employed.

On the 31st he complained of severe pain in the right side, with great difficulty of breathing; and on the 3d of November he died.

On examining the body after death, the pleurae were found inflamed, and incrustated with lymph, and serum had been effused into that of the right side. The lungs, also, were inflamed, and some portions of them were in a state of gangrene. The heart was affected with hypertrophy, and the pericardium was inflamed, with flakes of lymph adhering to it. The synovial membrane of the right knee was full of a dark-coloured fluid; not purulent, but having the appearance of a thick synovia, tinged with blood. The synovial membrane was every where of a red colour, as if stained by this secretion, and the cartilages of the joint had the appearance of having been stained in the same manner. There were some small extravasations of blood in the cellular membrane external to the joint."

But I find also unequivocal evidence of the same general nature in the works of some of the continental writers. Thus, so long ago as the time of Stoll, the occasional termination of rheumatism in suppuration of the joints, seem to have been known. Indeed, he expressly says, "in some patients these cases of rheumatism, after having caused great suffering, give rise to suppuration;" unfortunately, he does not inform us as to whether he had examined the joints after death, — but adds, a little farther on, "I treated two young girls, one of whom had the hand, and the other

the foot, affected with this kind of rheumatism. All my efforts, though they were varied and long-continued, were unable to overcome the obstinate character of the malady*."

M. Bouillaud, who strongly advocates the idea of rheumatism being analogous in its effects to other inflammations, states that he has seen three cases of the disease ending in suppuration of the joints; but in these there existed inflammation of some of the veins, and as we know that phlebitis is frequently productive of deposits of matter in the joints and elsewhere, I am not disposed to attach so much importance to his observations as to those of some others, particularly of M. Chomel. This distinguished physician maintains that rheumatism is not a true inflammation, and consequently his facts are not open to the suspicion of any bias in favour of the doctrine for which I argue. Now, in his Essay on Rheumatism, he relates the case of a patient who died at La Charité; both shoulders had been in succession the seat of acute pain, with tumefaction; and on examination after death, a purulent effusion was found in the affected joints. But that the previous history of this case had been that of rheumatism is proved by this—that Chomel asks in reference to it, whether it can be that *rheumatism* should have its seat in the synovial membranes? He then goes on to state, that in two other patients whom he had seen at the Hôtel Dieu, several joints of different limbs became painful and swollen, so that they could not perform their movements. The patients died, when increased redness of the synovial membranes, with effusion of pus, was found in the articulations alluded to. The inference which he deduces is not, as we might have expected, that rheumatism may produce these organic changes, but that inflammation of the synovial membranes may very much resemble that disease. This, and numerous other passages in the works of modern French writers, shew that they are not aware of the great distinction between synovial and common fibrous rheumatism, their reasoning being almost exclusively applicable to the latter.

In the Journal Hebdomadaire for April 1834, is the following case: — Thérèse Vellamé, aged 27, a cook, in the eighth month of her pregnancy, was exposed to wet and to the vicissitudes of the atmosphere, and experienced on the 27th of January an attack of pain in the loins, with shivering. The knees, feet, and wrists, were successively affected, and she was admitted on the 1st of February, under the care of M. Piorry. There was

* Practice of Medicine, vol. iii.

then swelling, with heat and redness of almost all the joints, and *hydarthrosis* of the left knee. She was ordered to be bled on several successive days, but two venesections only (through mistake or negligence) were practised, and those with an interval of several days. The patient remained, with little change, till the 6th of March, when she was put to bed of a dead child; she herself expired on the 9th, symptoms of peritonitis having previously declared themselves. The left knee presented externally a marked degree of swelling. On opening the joint, a considerable quantity of pus was evacuated. The cartilages were softened, and the fibro-cartilages at some points destroyed. The synovial capsule was of a deep red colour, with lymph deposited on its surface. Both ankles presented analogous appearances, but in a less degree.

In referring to the four fatal cases I have myself seen, and in analysing the others which I have adduced, I find that, in all the affection had apparently originated in the synovial membranes, evinced by pain of the joint, accompanied by a swelling, the shape, (and for the most, the fluctuation also) of which indicated it to depend upon effusion within the capsule; that at first the pains had been more or less erratic, implicating several joints simultaneously, or in rather rapid succession; but that either from the commencement some one or two articulations had suffered more than the rest, or that after a time the pain and swelling had become, as it were, fixed in certain joints, and continued there after the others had got well. As might be expected, the joints on which the force of the disease was thus concentrated, were those in which the destructive effects of the inflammation were most conspicuous.

The phenomena here are strikingly different from those which occur in rheumatism affecting the ligamentous textures; for in this last, as we have seen, however considerable the external appearances of inflammation, yet the joint internally may present on examination not a single trace by which its presence can be recognised.

It seems clear, from the considerations already offered, that the form of rheumatism which more especially attacks the synovial membranes has a greater tendency than the preceding to produce disorganization in the primary seat of the disease. This might, perhaps, be explained by the fact that the internal lining of the joints is, under ordinary circumstances, more prone to change of structure than the common ligamentous textures. There is, however, another important point to be taken into the ac-

count—namely, that the synovial form of rheumatism is more connected with a cachectic or other morbid condition of the general system. It conspicuously occurs in persons who are of feeble and debilitated constitution; or if it appear in the more robust, it is generally after some considerable and continued source of mental or bodily exhaustion. Thus it may supervene upon a long journey in cold weather—upon severe study, or acute distress, particularly when to these are superadded causes productive of ordinary rheumatism, such as sitting up during the night by a sick bed, or in the accomplishment of some trying intellectual task.

Again, the evidence of a morbid condition is sometimes yet more unequivocal, as where we see the attack supervening upon, or alternating with, gonorrhœa—an event by no means very uncommon, and presenting one of the most obstinate forms of the disease. It also is met with in connexion with other venereal affections; but as this occurs almost exclusively where long-continued courses of mercury have been adopted, there is reason to suspect that the remedy is in this respect as much to blame as the syphilitic disease.

The first indication of this form of rheumatism generally consists in a dull aching and stiffness of certain joints, particularly those which are most superficial. After a time the pain becomes of a more acute or burning character, and the parts begin to swell, the swelling presenting a model of the synovial or bursal cavity, into which the effusion has taken place: the suffering is increased either by stretching the parts much, or by corrugating them, and hence a semi-flexed position of the joint is the one usually assumed in severe cases. Motion aggravates the pain, particularly after rest of some continuance, as in the morning, at which time there is often a great deal of stiffness about the parts; but after a little exercise this subsides, and the limb assumes more of its wonted suppleness.

In the most acute cases there is smart fever, and some redness about the joints; but much more usually there is no external discoloration, and little fever; that is to say, the fever is inconsiderable as compared to that which attends the ligamentous form of rheumatism. When fever is present to a considerable extent, it generally presents the remittent type, with copious acid perspirations; and I have seen a pulverulent deposit on the skin which admitted of being scraped off, in quantity minute indeed, but sufficient to admit of being tested, and which was found to consist of the lithate of soda. Where the fever runs rather high the tongue becomes foul, and the bowels

constipated; but there is much less disposition to these states than in common rheumatic fever. Indeed, of the secretions, the perspiration and urine are those most uniformly affected, the latter being loaded with the lithates, and sometimes producing a sense of scalding as it is voided. In one case, which lately occurred to me, this was attended with slight discharge from the urethra, although I have the strongest reason to be assured that there was no gonorrhoeal affection. This phenomenon would seem to bring still more close the connexion between the urethra and synovial membranes.

The course of this disease is altogether considerably different from that of rheumatic fever; it more speedily becomes mitigated in severity when both are treated in the ordinary manner, or when both are left to the unaided efforts of nature. But it is rarely if ever cut short at once, and the subacute, or chronic condition, is much more enduring. In a first attack, perhaps six weeks may be stated to be the average duration of the illness, and in such case little, or perhaps no perceptible damage may have been done to the affected joints. But it is very apt to recur, and with each relapse its obstinacy seems to increase, and, in fact, the inflammation acting repeatedly on textures already diseased, at length may be said never to be wholly absent, giving rise to permanent swelling and stiffness about the joints, constituting the "nodosities," already described. Sometimes, after years of suffering, a truce is at length entered into, and the patient enjoys a long respite, without our art having any apparent share in the favourable change.

When the superficial joints are affected with this form of rheumatism, it is generally very easily recognised; but when it attacks those which are deep-seated and most covered, considerable difficulty may exist: for instance, with respect to the hip joint. In such cases there is usually some external part implicated, which gives us a clue to the mystery. Thus, if the great toe, or knee, or but a single knuckle be simultaneously affected, we may infer that the nature of the deep and concealed disease is the same as that of the superficial. But sometimes the *joints* are all free, and some deep-seated *bursa* is alone affected. If in an individual who has been subject to this form of rheumatism, or even without this, if he has been exposed to causes likely to have produced it, should he complain of pain accompanied by stiffness, and much aggravated by motion, with some perceptible fulness, but without heat or much constitutional disturbance, such case will frequently be found to depend upon rheumatic inflammation, with effusion into

some deep-seated bursa; at least, such cases will frequently get well under the use of those remedies which are of most avail in less equivocal examples of synovial rheumatism.

ON THE
PRACTICAL INFERENCES
WHICH RESULT FROM
SOME LATE PHYSIOLOGICAL
INVESTIGATIONS
RESPECTING THE
LAWS OF THE VITAL FUNCTIONS
IN THE MORE PERFECT ANIMALS.

By A. P. W. PHILIP, M.D. F.R.S.L. & E.
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of London and Edinburgh, &c.

PART II.

To the Editor of the Medical Gazette.

Sir,

THE positions which, as far as I am capable of judging, are established by the experiments referred to in a paper published in the last number of the *MEDICAL GAZETTE* are, that besides the nerves of sensation and motion, there is a third set, the ganglionic nerves, the functions of which have not been understood, but which differ as essentially from the function of either of those sets of nerves, as their functions do from each other; that the functions of this class of nerves are to combine and convey an influence prepared by the brain and spinal marrow, and not by any particular part, but the whole of those organs, from the uppermost surfaces of the brain and cerebellum to the lowest portion of the spinal marrow, for the purpose of maintaining and regulating the functions of all vital organs, with the exception of those, the function of which depends simply on that of the muscular fibre—namely, the heart and blood-vessels; which organs, although deriving their power from a source wholly independent of the influence conveyed by the ganglionic nerves, are nevertheless placed under its immediate control*.

* We have reason to believe from many facts recapitulated in the *Philosophical Transactions* of last year, and republished, with some additions, in the numbers of the *MEDICAL GAZETTE* for the 18th and 25th of March last, that the ganglionic nerves possess no power of bestowing sen-

Thus it is that this influence, constituting the leading power in the vital system, that power to which all its other powers are subordinate, holds under its dominion every part concerned in the vital functions, whether it derives its power from another source, or from itself; on which therefore depends the formation and well-being of all our organs; and we cannot help observing with what care nature protects both the organs by which it is prepared, and those which convey it. The brain and spinal marrow are in all their parts defended by powerful bones; and the ganglionic nerves, in every instance, placed so deeply in

sibility on the parts to which they are supplied, and consequently, that those parts derive their sensibility from the same class of nerves which bestow it on other parts; nerves belonging to this class accompanying and being bound up in the same sheath with the ganglionic, as they are found to be with the nerves of motion. At first view it appeared to me probable that in those instances where the ganglionic nerves excite the muscular fibre, they were bound up with the same class of nerves which excite the muscles of voluntary motion. But there are several circumstances which seem to point out, and others which prove, that the ganglionic nerves themselves possess the power of exciting the muscular fibre. In the organs supplied by the ganglionic nerves, it is not excited in the same way as in the muscles of voluntary motion; the excitement of the muscles of involuntary motion neither being under the influence of the will nor capable of being produced, as in the case of the muscles of voluntary motion, by mechanically stimulating the nerves either of the living or newly dead animal, a fact which we have seen misled Haller in his inference respecting the relation which the nervous system bears to the muscles of involuntary motion. But the facts which leave no room to doubt that the ganglionic nerves possess the power, under certain circumstances, of exciting the muscular fibre, are, that the muscles of involuntary motion, although they cannot be excited by stimulants applied to their nerves, either in the living or newly-dead animal, can in both be excited by stimulants applied to any part of either the brain or spinal marrow; while the muscles of voluntary motion only obey stimulants applied to the particular parts of those organs from which their nerves arise: and while the muscles of involuntary motion are more powerfully excited by chemical than mechanical stimulants, the former of which, if we except electricity, have little effect in exciting the muscles of voluntary motion (my *Experimental Inquiry into the Laws of the Vital Organs*, Part II.); and lastly, we know from direct experiment, that the influence conveyed by the ganglionic nerves is the same as that conveyed by other nerves of motion, although wholly of a different nature from that conveyed by the nerves of sensation (*Philosophical Transactions* for 1836.) The most ready test, as I have elsewhere pointed out, by which we may determine whether any particular function depends on the ganglionic nerves, where the parts are too minute for the labours of the anatomist, is its being subject to all parts of the brain and spinal marrow, these being the only nerves which convey the influence of all parts of these organs. It was thus that the blood-vessels were proved to be supplied by ganglionic nerves, even to their minute ramifications. (*Philosophical Transactions*.)

the softer parts of our frame, as to be almost as well defended as if they also had been secured by bony cases.

WE are now to inquire into the nature of the morbid states of the influence conveyed by the ganglionic nerves, with a view to the improvement of the practical department of our profession; for it is impossible to conceive that a knowledge of, as far as life is concerned, the most important functions of the brain and spinal marrow; and of the only functions of the ganglionic nerves, functions of no less importance, those of combining and conveying the influence of the vital organs of the brain and spinal marrow, should not essentially influence that department: that the state of the leading power of the vital system should not be essentially concerned in its diseases.

As this is one of those powers of which the living animal partakes in common with inanimate nature, it cannot of course be subject to change by disease, but the organs which supply, and those which convey it, are as much so as other parts of our frame, and its effects must be regulated by the state of those organs.

Certain stages of their diseased states are familiar to every practical physician. But as we have neither been aware that the brain and spinal marrow supply, and the ganglionic nerves convey, the power which regulates the functions of life, our knowledge of the nature and immediate cause of many of those diseases has been extremely imperfect. From this defect of knowledge, and the want of sensibility in the parts concerned*, their early stages often excite little attention: and it not unfrequently happens that no serious attempt is made

* Neither the brain nor spinal marrow appear to possess any sensibility. Previous to our being aware of the distinction between the nerves of sensation and those of motion, certain parts of them appeared to possess sensibility, because the muscles of voluntary motion are thrown into contraction by irritating those parts. This we now know does not necessarily imply that those parts are endowed with sensibility. It is also a fact that many of the other vital organs are ill-supplied with nerves of sensation—the lungs, the heart, the liver, &c. These causes have greatly contributed to the obscurity of the diseases depending on a failure or irregular supply of the influence they convey. This may, at first view, appear to be a defect in the constitution of our bodies: but it is probably the cause of much less inconvenience than would have arisen from a high degree of sensibility in organs, the functions of which are constant and subject to frequent, and often sudden, causes of excitement.

to arrest their progress, till it has been such as to defy all our means.

Nothing can be more evident than the inference that if the organs of the leading power in the maintenance of the functions on which the healthy structure of every part depends, be distributed throughout every part of the brain and spinal marrow, those functions must be influenced by all causes which tend to impair the vigour of either, or any considerable portion of either of these organs; and no fact can be more notorious than that, in many of those instances where the nervous system has long suffered under causes of irritation, derangement of function in some organ essential to life is often at length established, which frequently resists the usual means, and terminates in a fatal derangement of structure; while in the earlier stages, from the insensibility of the vital organs, the patient's state appears to differ but little from that of others who are what is called nervous, and often continue so for a long life without any symptoms of danger supervening.

It is evident that these cases, however similar in their symptoms, must be of an essentially different nature.

The nature of the difference will be evident if we compare them with the facts detailed in the paper so frequently referred to in my last communication, on the Powers of Life, in the Philosophical Transactions for 1836. In the one case, the derangement has its seat in the central organs of the sensitive system; in which, therefore, however severe the suffering, life is not endangered, because the organs of the sensitive system have no share in maintaining it: in the other, it extends to the vital organs of the brain and spinal marrow; and the cause of the two cases bearing so near a resemblance to each other, arises from the sufferings being in both in the sensitive system; for the one system, for the reasons stated in the paper just referred to, never suffers without the other more or less partaking of the suffering; and all our feelings belonging to the sensitive system, the sufferings are nearly the same, whether the vital organs of the brain and spinal marrow partake of the disease or not: indeed, often less in the former case, as might be supposed, when the original disease is in the insensible parts of our frame; until the evil declares itself by the suf-

fering of some other part of the vital system, more or less supplied with nerves of sensation, and the functions of which consequently being more evident, have been better understood.

The first thing which suggests that the disease may not be wholly confined to the sensitive system, is the functions of this part being more prominently and constantly affected than is common for those of any particular organ to be, in what we call nervous complaints.

Even under such circumstances, however, we are not always alarmed; we have often before seen such affections of the same part arising from causes, the effects of which proved trivial, and yielded readily to the usual remedies; and, not being aware of the change which has been gradually going on in the vital organs of the brain and spinal marrow, we see no reason why the derangement of function should not yield as in other cases; the cough, or the headache, is a little more obstinate than usual, but we see no reason why the patient should not do well.

In such cases, however, unless we can trace the source of the disease, and remove the cause which is preying on the vital organs of the brain and spinal marrow, we generally find that he does not do well; and are at length awakened to his real state, by symptoms of change of structure supervening on those of deranged function, when, for the most part, the disease has advanced too far to be arrested.

Cases of this description are of two kinds. I have just had occasion to refer to a paper in which are detailed the various causes which maintain a powerful bond of union between the sensitive and vital systems; in consequence of which, it never happens that the central organs of the one are much affected without those of the other more or less partaking of their affection. Their vicinity alone, indeed, is sufficient to produce this effect, for there is no exception to the law which I have elsewhere considered at length*, that all neighbouring parts sympathise.

The degree in which this sympathetic affection of the central parts of either system tends to produce disease in the other organs of that system, depends not

* Gulstonian Lectures, delivered before the College of Physicians in 1835, and published in the same year.

merely on the degree in which its central parts are affected, but in a great degree on the liability to disease in other organs; and therefore on the particular constitution of the patient; which also determines the organ which will suffer most, unless some particular organ has been weakened by accidental causes, such as having suffered by previous disease; for the chief effect of the derangement, which influences the whole, will always fall on the weakest part; and it is a law of our frame, which I have had frequent occasion to illustrate, that the morbid affection of any one part being established, tends to divert the cause from all others.

Thus it is, that whether an organ, secondarily affected, belong to the sensitive or vital system, the cause may either originate in the other system, or it may be the effect of causes directly injuring the central parts of the system to which the organ in question belongs. Both cases, it is evident, are very different in their nature from those in which the cause of injury acts directly on this organ itself: these are simple cases, of which I do not speak, and in which both the cause of the disease and its treatment are well understood. Thus, on the one hand, we see cases in which the long continuance of the depressing passions, what is called a settled grief, for example, gradually impairs the vigour of the central organs of the sensitive system; by degrees those of the vital system partaking of this debility; and in consequence of this secondary affection, the function of some organ belonging to the latter system is deranged; and the derangement, if the cause continue to operate, maintained, till it terminates, as almost always sooner or later happens, in such derangement of structure, that the organ is no longer capable of any degree of its healthy function, and a fatal termination ensues.

On the other hand, we see such causes of injury as directly debilitate the central organs of the vital system, produce the same train of symptoms, except that they are not preceded by the affections of the sensitive system, which in the foregoing case lay the foundation of all that follows.

With respect to this case little need be said. The safety of the patient depends on removing the cause of derangement in the sensitive system, which, from the sensibility of the parts concerned, is generally sufficiently evident, before the

structure of some organ essential to life, in consequence of the central organs of the vital system partaking of the diseased state of those of the sensitive system, is seriously deranged; for after this has taken place, our means, although they may in some degree retard, can rarely prevent the fatal termination.

The object of this paper is to direct the attention of the physician to the former case, in which the cause of disease, in consequence of the insensibility of the organs of life belonging to the brain and spinal marrow, and many of the other vital organs being ill supplied with nerves of sensation, not unfrequently makes its advances in so insidious a manner, that, as we have just seen, the symptoms of derangement of structure in some organ essential to life is often the first thing which warns the physician of his patient's danger.

The only means of preventing such a result is to observe with care the states which precede it, that we may be enabled to distinguish the threatenings that should warn us of its approach; and thus learn to distinguish those cases in which the vital organ of the brain and spinal marrow are suffering in such a way as may sooner or later incapacitate them for maintaining the functions on which life depends.

The subject naturally divides itself into three parts: in the first place, to determine the nature of the immediate cause of the disease—the state of the organs from which it immediately arises; secondly, the remote causes—the causes which produce that state of the organs in question; and lastly, the means best calculated to obviate their effects. The first I shall consider in the remaining part of the present paper, reserving the second and third, with which the subject will close, for my next communication.

It appears from a paper to which I have had frequent occasion to refer, because it contains a recapitulation of all the more important facts relating to the subject*, that the leading power in each of the two systems, under which all the powers of the living animal arrange themselves, both influences every part, and is influenced by every part, of the

* Philosophical Transactions for 1836; and Medical Gazette for March 18 and 25, 1837.

system to which it belongs. This power, therefore, suffers by every cause of injury going on in any part of that system, all the functions of which depend upon it. I have already stated my reason for confining my attention to causes directly operating on the leading power of the vital system; namely, that when the affection of the sensitive system is such as, in consequence of the sympathy which exists between the central parts of the two systems, to injure the organs of the leading power of the vital system, the cause must, from the sensibility of every part of the sensitive system, always be such as to be evident to the most careless observer.

In the cases to which I wish to direct the reader's attention, on the contrary, the little sensibility of the parts concerned, from the vital organs of the brain and spinal marrow being devoid of sensibility, many of the other vital organs but ill supplied with nerves of sensation, and the cause in general operating slowly, the symptoms are often such, in the early stages, as appear to indicate no serious consequences; in general differing but little from those of what are called nervous complaints, which are, for the most part, free from any risk to life.

But it will easily be believed, that if a cause tending to debilitate the organs of the leading power in the vital system, however feeble, be long continued, its continuance compensating for the feebleness of the impression, these organs may so suffer as to interfere with the due preparation of the influence they supply, on which we know from direct experiment the functions of all vital organs depend. The apparent inadequacy of the cause, in the majority of cases, is a chief source of danger; for we shall find, that when the cause and its tendency are detected at an early period, its effects may almost always be obviated. As the central organs of the vital system feel the effects of all causes operating on any part of this system, such causes, it is evident, must operate most frequently, as well as powerfully, in those parts of the vital system which are most extensive, and most exposed to causes of injury.

Now the vital organs to which these observations in the greatest degree apply, are the alimentary canal and its appendages; and so habitual are causes of irritation in them, and so many of them

such as we find are unattended with any serious effects, that of all such causes, they are least calculated to excite alarm. We are accustomed to see them recur for a great length of time without producing any serious effects on the general health.

In some cases, however, in affections of these or other vital organs, the patient perceives that a sense of debility in some, in others what is called a fret of the nerves, which have only been temporary, becomes more permanent, and at length constant. Under such circumstances he soon feels his strength more or less permanently impaired; but the first thing which generally makes him anxious respecting his complaints, is finding that he is not equal to his usual mental efforts, for the sympathy which exists between the two systems is mutual. As affections of the sensitive at length influence the vital, those of the vital seldom fail more or less to influence the sensitive system.

In affections of the alimentary canal, however, a more direct cause is at the same time operating. This canal is one of the few vital organs copiously supplied with nerves of sensation, and consequently its affections directly, and to a great degree, impress the central organs of the sensitive system, and that to a much greater degree than happens with respect to those of any other vital organ; so that by causes of irritation in that canal, the central organs of the vital system are doubly assailed, by the direct effect on these organs of all impressions made on any vital organ, and by their immediate sympathy with the central organs of the sensitive system, which is but little influenced by the affection of the vital organs ill supplied with nerves of sensation.

Now as soon as this sense of debility or fret of the nerves becomes continued, there is a cause operating, which is constantly tending to debilitate the organs of the leading power of the vital system; and however slow its progress and little prominent its symptoms, in consequence of the vital organs of both the brain and spinal marrow being devoid of sensibility, its ultimate effects may be fatal; and it appears from experiments referred to in my last communication that they may be so, before the general debility of these organs is such as even to approach a total loss of power, because any considerable diminution of

the influence they supply was found sufficient not only to destroy the function, but even structure of other vital organs.

Life, therefore, must cease from this cause, before the functional disease of the central vital organs can approach that stage at which change of structure takes place in them. Wherever, therefore, change of structure in these organs is observed on inspection after death, their disease must have been partial, and such as originated in the part affected. Such a result cannot take place from a cause operating generally on the central organs, which all disease of other vital organs must do, as each is not only under the dominion of, but capable of influencing, the vital organs in every part of the brain and spinal marrow.

If any of those other organs happen, under such circumstances, either from peculiarity of constitution, previous disease, or other cause, to be more liable to disease than the rest; the cause which operates on all will on it make its chief impression: and according to a law of the animal frame to which I have had occasion to refer, when disease is established in one part, the effects of the cause are diverted from others; and then the functional disease—for in such cases the first deviation is only functional—if the cause be allowed to continue, is sooner or later, and in some cases rapidly, succeeded by disease of structure.

Such is the course of the diseases to which I am about to direct the attention of the reader. Their course is variously modified in different cases. The limits of the present papers do not permit of an attempt to enumerate all their modifications. I shall only mention a few of the more important, which cause the derangement in different cases to assume so different a form, that it is difficult to refer them to the same class of diseases.

In other essential respects, besides that just mentioned, diseases of this description are powerfully modified by peculiarity of constitution. In some, the vital organs of the brain and spinal marrow resist disease much more powerfully than in others. In others, the other vital organs are less than usually liable to disease. In the former, the debility or fret of nerves does not readily become permanent, for their permanence

seems always to depend on some affection, that is, some degree of debility, more or less, having taken place in the central organs of the vital system. In the latter, although their permanency is established, the other vital organs resist their effects on the central organs more powerfully than usual. The disease, from this cause is, as it were, arrested in its progress; the power of the central organs is impaired, but the organs which depend upon them feel less than usual slight diminutions of the nervous influence; and thus the functions of life going on, the state of the central organs themselves is better maintained than is usual under such circumstances.

In some, the affections of the central organs of the vital system affect the sensitive system in one way, in others in another; and often so differently, that the two cases appear to have nothing in common. One patient appears only languid, and what is called out of health. In others, the sense of languor and debility is extreme, or restlessness, and a painful sense of oppression, characterize the disease. In not a few, the leading symptom is a frequent recurrence of severe and sometimes agonizing muscular pains, not unfrequently even producing delirium.

It is a remarkable fact, that all such severe affections of the sensitive system, however tormenting, seem generally to be a means of safety, by diverting the tendency of the disease from more vital parts. I have uniformly found that in such cases, many of which I have seen, the organs of the vital system are less apt to suffer than where the disturbance of the sensitive system forms a less prominent part of the disease, and where consequently it attracts less attention.

The most deceitful of all this class of diseases are those in which the disease does not arise from irritation in distant parts, but originates in the vital organs of the brain or spinal marrow themselves. From the insensibility of these organs, the affections which originate in them are detected with much difficulty; and are often overlooked till they have made great progress, and even hardly betray themselves by any symptoms but those which attend the more common nervous derangements; their chief characteristic being the uniformity with which they advance to a fatal ter-

mination. In my *Gulstonian Lectures*, I have endeavoured to point out their diagnostic and illustrate their nature, by an account of two post-mortem examinations, compared with a detail of the symptoms.

When we consider the sympathy which exists between the organs of the leading powers in the sensitive and vital systems, and that the former are the source of all our feelings, while the latter appear to be wholly insensible, we cannot be surprised at the endless variety and striking dissimilarity of the affections of either, often depending as much on causes which influence the sympathy which exists between them, as on the morbid condition of the one originally affected.

It is hardly necessary to observe, that the symptoms which may arise from affections of these two sets of organs—the centres of the systems which regulate the whole of the functions of our complicated frame—must necessarily be of infinite variety; and when we take into account the powerful sympathy which exists between them, it is evident that the affection of either may produce, in different constitutions, almost every variety of affection of which our organs are capable.

My chief object, however, is to direct the attention to those cases the obscurity of which has produced the most fatal effects; and which may be obviated by a better understanding of their early and curable stages, which are marked by no prominent symptom; and sometimes by no regular set of symptoms belonging to the affection of any particular organ; because the symptoms which arise from disease in either set of central organs, or even both sets, when a diseased state of no other organ has supervened, so that certain parts of them remain the only seat of the disease, must, from their insensibility and the nature of the functions of both, be general, not local symptoms.

However varied the other phenomena in all the cases I now speak of, the diagnostic symptom of their earlier stages is still the same; a continued general debility or fret of nerves, without the symptoms which indicate a fixed disease in any particular organ, with the exception of those, by far the most common, in which the state of the central organs is the secondary disease, the affection of another part having pro-

duced it. The characteristic symptoms which arise from a diseased state of either set of the central organs, being, from their nature, necessarily general not local symptoms, greatly contribute to the obscurity of their early stages. But in most cases the disease gradually assumes a different form.

The functions of some other vital organ, we have seen, beside that, the disordered function of which had produced the general affection, begins particularly to suffer, in consequence of the failure of function in the central organs of the vital system. This almost always happens where the original affection is in an organ little inclined to change of structure, and the affection of which consequently long supports the irritation without proving fatal. When this change takes place, except the disease has arisen from an original affection of the central organs themselves, which is comparatively rare, it consists of three distinct affections; the disease of the vital organ in which the whole originated, the disease of the central organs of the vital system produced by it, and that of the organ, the affection of which arises from the affection of the central organs.

In addition to these, the essential symptoms of such cases, there are always more or less of two other trains of symptoms, having their origin in the central organs of the sensitive system, which are directly influenced by the disease of the part originally affected, more or less, according to the sensibility of that part; and sympathetically, by the affection of the central organs of the vital system.

Now the obscurity, and consequently the danger, of the early stages of such complicated, but we shall find very common cases, depending on the vital organ originally affected being ill supplied with nerves of sensation, and the insensibility of the central organs, often causing the affections of both to be overlooked; the first thing in general, as appears from what has been said, which commands serious attention, is the affection of the vital organ secondarily affected through the central organs of the system to which it belongs. And as the attention of the physician is often not attracted to the original affection, in consequence of the little sensibility of the organ in which it is seated, or is not aware of its connexion with the state

of the organ secondarily affected, which is generally the most prominent part of the disease, he is not prepared to find the latter either more obstinate and more apt to run to change of structure than the same affection of the same part when it is the only disease.

The means of preventing the fatal result are the discovery and removal of the original disease which caused and supports the diseased function of the central organs, and without which the cure of the secondary disease is impossible.

In one case of this kind, post-mortem dissections have directed our attention to the original disease, although its mode of operation has not been understood. I mean in the hydrocephalus internus; and what has been the consequence? That in that which was previously an almost uniformly fatal disease, now that dissection has proved that it derives its origin from diseased function of the liver, the proper treatment is so successful, that there are few serious diseases in which our practice is more uniformly efficient; and it will appear from what I am about to say in my next communication, that an equal improvement in the prognostic has already taken place in other cases, of a nature quite as important, in consequence of the application of the foregoing principles.

One observation applies to all such complicated cases; however little annoying to the patient, or formidable in the view of the bystanders, both of which generally arise less from the degree of the derangement than the insensibility of the organs affected, and however slow its progress, the case will certainly sooner or later prove fatal, if the cause which has induced and supports the debility or general fret of nerves (which, in its turn, is the cause of debility in the organs of the leading power of the vital system, and this of the disease which last supervenes on which the danger depends,) cannot be discovered and removed; for no disease can yield if the cause which produces it continues to operate.

It is therefore the original disease, however unobtrusive its symptoms, to which must be ascribed all that follows, and on the removal of which the safety of the patient wholly depends.

In the remaining communication I

shall consider the remote causes, those causes which tend to produce a debilitated state of the vital organs of the brain and spinal marrow, the immediate cause of the more prominent disease; the means of removing them, and as far as it has taken place, counteracting their effect.—I am, sir,

Your obedient servant,

A. P. W. PHILIP.

Cavendish-Square,
Nov. 13. 1837.

SOME ACCOUNT OF MESMERISM.

A Monsieur l'éditeur de la Gazette Médicale.

MONSIEUR,

QUELLES que soient vos opinions sur la science que je professe, que cette science soit ou non fondée, elle a maintenant trop de partisans pour ne pas être examinée avec attention. J'attends donc de votre impartialité que vous ne vous refuserez pas à l'insertion dans votre journal de quelques articles propres à faire connoître la découverte de Mesmer, et les travaux des hommes distingués qui s'en sont occupé.

Cette preuve honorable de votre esprit de justice, me permettra peut-être de faire cesser les préventions qui existent contre le Mesmérisme, et de préparer les esprits à un nouvel examen.

J'ai l'honneur de vous saluer,

BARON DU POTET DE SENNEVOY.

Londres, le 11 Novembre, 1837,
25, Orchard-street, Portman Square,

Anthony Mesmer, to whom we owe the discovery of animal magnetism, was born at Vienna, in the year 1740. He received his degree as Doctor of Medicine, from the faculty in that city, in 1766. His inaugural thesis is entitled, "On the Influence of the Planets upon the Human Body."

Led by the nature of his mind to deviate from the beaten track, and aware of the little certainty of his art, he was anxious to establish it upon incontrovertible principles. The yearning after truth took possession of his spirit; he followed up the search with enthusiasm: but a just idea of the character of Mesmer could perhaps only be given by quoting what he himself wrote, when most absorbed by his discovery.

It would be difficult to say how

Mesmer arrived at the detection of that agent with the study of which we are now occupied. Whether it was by the force of his own genius, or that he had read the authors who long before him had treated of this matter, is of little importance to us. However that may be, Mesmer is entitled to our utmost gratitude for having recalled our minds to the study of magnetism.

The doctrine of Mesmer first began to be practically applied towards the year 1775, about which time he published a portion of his system.

The principles of Mesmer were not fundamentally so different from those of other physicians as might be imagined. Mesmer thought that every motion of the body, external and internal, whether in health or disease, takes place by the agency of the nerves. Now this opinion of Mesmer was held by all other physicians. Mesmer thought that the action of the nerves itself depended on the action of a very subtle fluid; so thought all other physicians. Mesmer thought this fluid to be itself subject to various agents, some of which (such as the bodies by which we are surrounded) are external; and others (such as the various affections of our mind, our will, our passions, the organization of our frame, &c.) are internal; all other physicians thought the same. Mesmer thought that the normal state of our functions, on which health depends, is maintained by the regular action of the nerves; other physicians thought so too.

Mesmer believed that the cure of diseases is effected by crises; other physicians also considered this to be the case.

In what, then, did Mesmer differ from the physicians of his age? In this: Mesmer thought that he had discovered the secret of directing at will, and by very easy means, the fluid which sets our nerves in action, and thereby of imparting to them such action as might be requisite either for the preservation of health or for the cure of disease. Mesmer, in fact, laid claim to having arrived at a better knowledge of the laws of life than the physicians who had preceded him. It was this which they disputed.

His ideas were not favourably received. He was repulsed on all sides, although he had treated some patients by magnetism, and had obtained the most complete success. After having been exposed to several disgraceful

scenes, he quitted Vienna in the year 1777, thinking he had done enough for his ungrateful fellow citizens, and bearing with him the hope that the day would come when they would do him justice.

A few years only from this period had elapsed, when that which was denominated Mesmer's discovery was already producing the most vivid sensation amongst the neighbouring nations; and when he arrived in Paris, in 1778, preceded by his reputation, he found himself the object of public attention.

It was not long before he associated himself with the most distinguished medical men of the capital, and more particularly with Dr. Deslong, first physician to the Comte d'Artois. An eager desire was manifested on all sides to assure Mesmer that he would be more successful in France than in his native country, and he was persuaded to lay his system before the Academy of Science.

Unfavourably received by this Academy, the ideas of Mesmer met with no better success at the Société de Médecine. Notwithstanding this failure, however, the frankness and good faith of his proceedings obtained for him the good will of many medical men, who zealously devoted themselves to the practice of magnetism, and published its successful results in all directions.

Treated without consideration, I might say almost ignominiously, by the learned societies, Mesmer was, nevertheless, eagerly sought after by a great number of distinguished persons. This circumstance suggested to him the idea of opening an establishment, where patients hastened to be treated according to the new mode; and some cures which were effected there finally established his reputation.

If the first propagators of magnetism had followed the example of that ancient philosopher who contented himself with walking in the presence of one who denied motion—if they had restricted themselves to producing effects without endeavouring to account for them—the cause of magnetism would long ago have been won. But they did not follow this course. They were in too great haste to erect systems, and to bring them forward as containing all the secrets of their art. Carried away by enthusiasm, the partisans of Mesmer knew not how to set limits to their

faith; the cure of some diseases led them to believe in the possibility of curing all. They had the indiscretion to affirm in their writings that there was but one life, one health, one disease, and consequently but one remedy—and that this one remedy was magnetism.

Extending their views to the future, they thought themselves entitled to predict that the agent discovered by Mesmer would operate a considerable modification of our morals, and a complete modification of our organization.

And these promises were actually published, accompanied by reflections not less startling.

"What we have just announced," said they, "seems to savour of enthusiasm; but it will be known some day that we have been considerate towards received opinions, and that we have not done full justice to our subject."

Not less astonishing than the enthusiasm of the magnetisers was the conduct of the scientific societies, who were as incapable as they of preserving sufficient coolness to pronounce, without prejudice, their decisions respecting magnetism.

Numerous scientific committees were officially appointed by Louis XVI. to examine into the system of Mesmer. These committees included Lavoisier, Bailly, Franklin, Jussieu, and a multitude of other names not less illustrious. The public, therefore, had a right to expect every thing from such an Areopagus. There could now be no doubt that the subject would be placed in its true light; yet this did not happen. But the reader should be transported back to that period, and turn over the history of those transactions, in order to form an idea of the agitation into which France was plunged by this question of magnetism. On one side were to be seen the learned bodies, animated by party spirit, endeavouring to proscribe magnetism; on the other, courtiers and citizens warmly embracing the new doctrine, and espousing the cause of Mesmer, whose character and conduct had conciliated general esteem. The one party denied all the effects of magnetism, or explained them on erroneous grounds, which I shall hereafter relate; the other, on the contrary, adopted all that their leader had said and written, and thus carried their belief too far. The dispute was warmly

urged on both sides. Paris was deluged with publications (more than 500 appeared in the space of eighteen months;) wit and learning were alternately employed. It would be difficult, at the present day, to conceive the high pitch to which the excitement was carried, if we did not possess the most unquestionable testimonials to prove the bitterness of the two parties.

The dispute became fiercer every day, and the commissioners chose the very moment when men's minds were thus exasperated to pronounce on the question of magnetism. Their report, I regret to say, was in consequence marked throughout by prejudice; and in every part of it we may observe them resisting the truth, which they endeavour throughout to elude. All arguments are considered good by them for the explanation of facts which were then inexplicable, and they did not fear to compromise their well-earned reputation, and to leave to posterity a monument destined to commemorate the aberrations of genius when excited by passion and prejudice. Yet, notwithstanding the unfavourable reports of the Commissioners, effects continued to be produced, and to gain partisans to the new doctrine.

This circumstance reminds us that many questions have been decided without the concurrence of the learned; and often, indeed, notwithstanding their formal opposition. It is well known, that the first persons who asserted they had witnessed the fall of aerolites, found no credit for their recital; but at length similar facts were attested by others, and at this day no one disputes a phenomenon at that time denied by the most learned. What occurred respecting the aerolites will indubitably take place with animal magnetism, and the period, perhaps, is not far distant. But let us proceed with our narrative.

Intolerance is frequently allied with power. The learned societies of France, then firmly constituted, knew that they could impose, with impunity, their belief or their opinion on the public. They were also not ignorant of the injury they could do to Mesmer, and they therefore began, previously to making their report, to persecute the followers of the new doctrine, and a great number of physicians fell victims to their zeal for the propagation of magnetism.

They even went so far as to endeavour to make these men act against their consciences. Such things would appear to us, at the present day, incredible, if they were not established by a multitude of memoirs written at the time. Let me be permitted to cite a portion of one of these, which will give some idea of the animosity which then existed against Mesmer and his adherents.

I extract the following passage from a pamphlet, entitled "Report addressed to the Public upon some abuses caused by Magnetism," by M. Douglé, Docteur regent.*

"Thirty magnetising physicians were denounced, and each of them received a special summons. They almost all appeared, and were sent into a separate chamber from the assembly. Each waited with impatience the general summons, and walked up and down, wondering what was going to happen. I was informed that it was proposed to require us to sign a kind of formulary. We will see what it contains, said I, and then we shall either sign or not, as we may think proper.

At length the usher appeared, and I was first summoned, having that honour as the eldest of the company. I entered, much surprised at not finding myself followed by any of my companions. I was requested to be seated, and the Dean began by inquiring if I had paid any thing for the instruction I had received in magnetism. Still more surprised at this question, I answered that M. Deslong did not receive money; that he only admitted medical men to observe and to assist him; that he was in the highest degree respectable, modest, and obliging; and that, moreover, the faculty were not ignorant of this. I will not weary the reader by the detail of the other questions put to me. I was interrogated like a criminal, and I fancied myself transported into the great hall of La Tourmelles. They at length concluded by presenting me a paper, which I did not consider myself at liberty to subscribe. I declined signing it, and assured the Faculty, in order to prove my zeal and deference, that I had not yet discovered in this method a sufficient degree of utility to induce me to make any report on it; that I had witnessed some results

from it, which might be attributed to the effect produced by a man in health upon an invalid; that in order to magnetise patients in their beds, much courage, strength, and health were requisite, &c. I left the chamber, and was succeeded by another."

The following is the formula which they wished every Doctor-regent* to sign:—

"No physician shall declare himself a partizan of animal magnetism, either by his writings or by his practice, under penalty of being struck off the list of Docteurs regents."

This decree bears date, April 23d, 1784.

The many truths which have been rejected and persecuted when first brought forward, and which have been since established, ought to render us more circumspect, and should deter us from denying new facts, however strange they may appear to us.

But it is not thus that we proceed. The experience of past ages is lost upon us, or, at the utmost, we only derive from it barren lessons, which are of no service to us when the opportunity of putting them in practice presents itself; for such is the constitution of our minds, that the things which appear the clearest to us when we consider them abstractedly and in a general point of view, we can no longer apply in particular cases, when we are misled by our prejudices and partialities.

I return to the sentence pronounced by the ancient Faculty against some of its members accused of believing and practising magnetism.

Several of the Doctor-regents did not submit to the judgment pronounced against them; they would not tamper with their conscience. They were inexorably erased from the list of Doctor-regents, and deprived of the honours and emoluments attached to their function. Of this number were Dr. Deslong, and Varnier, another Doctor-regent; the latter, because he manifested by his practice, and in his writings, a too obstinate adherence to the pretended animal magnetism. This decree is dated Aug. 27th, 1784. It produced a memo-

* Doctor regent was a dignity or title belonging to those distinguished physicians who at that time constituted the Academy of Medicine. These regent Doctors had the right of judging the conduct of other physicians, and of citing them to appear before them.

rial from M. Varnier, who protested against it. This memorial, drawn up by M. Fournel, an advocate, is a valuable monument of sagacity and prudence, as opposed to the irritation of the Faculty. It is accompanied by the joint opinion of seventeen of the most distinguished advocates of the time, who concurred in censuring the conduct of the Faculty, especially for exacting an oath that any individual would never believe in magnetism, and never become a follower of the system.

This act of intolerance, by a body which should have known better how to respect itself, contributed much towards increasing the number of those who favoured the new doctrine; and that which gave still greater stability to the system of Mesmer, and shook the respect still preserved by some persons for the decision of those who had judged him, was the resolution of M. Jussieu, who refused to sign the report drawn up by Mauduyt, Andry, and Caille, with whom he had been associated in the investigation. M. Jussieu made a separate report, in which, without entirely admitting the system of Mesmer, he yet seems to acknowledge that there is some foundation for it.

M. Jussieu performed an act of great courage in thus separating himself from his colleagues, and did not fear to encounter the ridicule which, in the learned world, awaited at that time all the partizans of magnetism; for notwithstanding the striking and positive proofs of its existence, Mesmer was ridiculed on the stage, burlesque poems were published against his doctrine, and himself was travestied in songs which were circulated throughout Paris. Magnetism was the subject of every conversation.

Before he left France, Mesmer visited Spa, where ill health detained him for some time. Many of his patients distinguished by rank and fortune had followed him thither, in order not to interrupt the course of his treatment. These invalids sympathised warmly in the vexation of him to whom they ascribed the relief of their sufferings. Attached to magnetism, of which they had felt the beneficial effects, they resolved to insure its diffusion in France by means of a subscription, which should provide the author of the discovery with such an independent fortune as would enable him to propagate it in the most efficient manner. This subscription did take

place. Mesmer accepted the proposal made to him to instruct pupils; and the subscribers, who were to have been limited to a hundred, much exceeded that number, although each subscription was 100 Louis. The members of this society, instructed in the doctrine of Mesmer, executed the project they had conceived; they spread the knowledge of magnetism in the provinces, and Mesmer quitted France to return no more.

[To be continued.]

ON SCURVY.

From the Annual Report of

DR. MURRAY,

Principal Medical Officer at the Cape of Good Hope, for 1835.

Communicated by Sir James McCorgor, Bart.

[Concluded from p. 259.]

V. Report of Staff Assistant-Surgeon Morgan.

Bathurst, Feb. 1837.

ALTHOUGH scorbutus has been so rife among the troops stationed in the New Province, no case of it has originated in this sub-district; therefore, whatever desire I have to investigate its causes, I have had no opportunity of doing so from actual observation.

This village was selected as a place for the reception of the scorbutic cases, towards the end of last year, on account of the pleasantness of its site, and the amenity of its climate; and also because it afforded, at the same time, facility of obtaining hospital accommodation, and vegetables, for the patients.

Thirteen cases arrived on the 16th November, eighteen on the 2d of December, six on the 4th, five on the 15th, and eleven on the 22d of the same month,—making in all 53 during 1836; and I have instituted a minute inquiry among these individuals respecting the origin and causes of the disease, of which the following is the result:—

The men of the 75th regiment appear to be the chief subjects of its attack, as no men of any other corps have come under my notice. This regiment shared, with all the other troops on the frontier, in the fatigues of the early part of the late Caffre war; but, occupying the rear line in the latter part of it, the na-

ture of their duty was different from that of the 72d regiment, and the other troops, which advanced into Cafferland against the enemy.

The duty of the 75th regiment consisted chiefly in assisting to escort supplies to the front, which was one of constant watching and considerable fatigue, as they were surrounded by lurking enemies; and when the campaign was over, and a change was allotted to them, they had still very fatiguing duties, since, in addition to that of escorting waggons, they had the building of huts and forts at their new stations; and when they got into their huts, such was the material of which they were made, and the inexperience of the workmen, that they did not afford sufficient shelter against the heat, cold, or rain.

In the next place, the meat being inferior in quality, the quantity of it eaten, from the way they were obliged to cook it, did not afford proper nourishment. They had no change of diet—no vegetables—no condiments and stimulants sufficient to excite the stomach; salt and pepper were not always to be had, and wine and spirits were only issued by starts.

As the transport of supplies passed through King William's Town to the advanced posts in the New Province, that is, to Forts Wellington, Warden, and Waterloo, and as the 75th regiment was quartered at these, great part of the escort duty, which was still necessary in time of peace, fell on it, and pressed hard on the men, so as to make them draw comparisons between themselves and others, and to render them discontented. Thus a desire of change was created; the men grumbled, and soon a languor shewed itself, which proclaimed the unhealthy diathesis that had crept into their constitutions.

In the months of December, January, and February, the weather in the part of Cafferland that was occupied by the troops is sultry, with thunder-storms; and days of great heat are often succeeded by damp and cold nights, with fogs in the valleys, and dew on the high ground.

I understand that as early as January and February (of 1836), scurvy, under the name of *purpura*, began to make its appearance at King William's Town; that it commenced at Forts Warden and Waterloo in the beginning of July; but that at Fort Wellington, the site of

which is greatly more elevated (being on the Gounbe heights), it did not become developed till a later period. The position and aspect of the several posts in the New Province differed very much; yet the disease appeared at them all, except, I believe, at Forts Beresford and Murray, where it seems to have made little if any impression; which is ascribed to the men at these having better huts, and occasionally some vegetables. At Forts White and Cox, it appeared in July or August.

I will now attempt to describe the symptoms and mode of treatment, as I have been best able to collect information; and I shall adhere to the actual accounts given by the patients.

Prior to the attention of the men themselves being particularly called to any marked symptom of the disease about them, it was observed by their officers and comrades that they fell off in flesh, and had a haggard countenance. About the same time they felt unusually fatigued after any exertion, and had an unwillingness and aversion to take duties, or any active exercise. The appetite did not appear to fail, but, on the contrary, was good; and the taking of their meals was the only thing that roused them for a time from their apathy and listlessness. They had a sense of internal heat, but no particular thirst. They felt distension of the stomach, and a sense of weight and anxiety in the precordia, especially after meals; but they had no acid eructations. They had occasionally slight pains in the back, thighs, and legs, which at first were considered to be of a rheumatic nature. They had no disposition to perspiration, but, on the contrary, a feeling of dryness and chilliness of the surface. The extremities were cold, and they were particularly sensible of the want of sufficient bedding to keep them warm, which produced restlessness in the first part of the night. The state of the bowels and urine was not remarked by them to be unnatural.

These symptoms used at first to be considered the effect of cold, and in many were so slight, that they did not require to put themselves on the sick list for a considerable time; but others were obliged to apply very soon for medical aid. After some continuance of the above premonitory symptoms, which varied in length in different individuals (*constitutions?*), spots began

to appear about the ankles and legs, and the skin assumed a blue colour, similar to that from a blow, in patches or streaks, frequently extending from the ankle to the middle of the thigh. The ankle and knee-joints swelled, and became painful, stiff, and contracted; and the skin felt like a board.

During the progress of this "*black-leg*," as they term this disease, from the discoloration being one of the most prominent and early symptoms, the gums became very tender. Some state this as the very first indication that they perceived; others as coming on at the same time with the spots on the limbs; and some say that it only appeared after the livid and swollen state of the limbs got to the worst; and it would appear, from their account, that the earlier the gums became tender and spongy, the less violent were the other symptoms; and *vice versa*.

As the disease advanced the appetite became very irregular; pain in the chest, attended with a sensation of fluttering and palpitation of the heart, which was very troublesome. The bowels were often relaxed, alternating with constipation; and the stools were dark, offensive, and sometimes contained bloody, putrid-like matter. The pain of the chest, in particular, continued a symptom of complaint to the last, causing a shortness of breath, which was most distressing on lying down. The pain was chiefly in that part of the chest crossed by the strap of the knapsack; and cough was sometimes present. In cold and damp weather the symptoms always became aggravated; and I have sometimes remarked, when convalescence was slow and tedious, that recovery was hastened by the patients being sent to resume their regular duties and employments, so soon as their strength would permit.

The treatment of most of the cases that came under my care had been previously under the direction of several different medical officers, and was very indistinctly reported to me; but I ascertained that the principal remedies exhibited had been wine, porter, spirits, vegetable acids, quinine or decoction of bark, neutral salts, especially nitre, and generous diet, with vegetables and lemons, or oranges, when they could be procured.

Before the patients had arrived at Bathurst, they had generally recovered

from the regularly-marked symptoms of scorbutus; and my attention was principally called to the affection of the chest attempted to be described. The forms of it, however, were so various, as to cause me considerable embarrassment; and, moreover, the men were extremely irregular in their conduct, and gave themselves up to frequent intoxication, insomuch that their persisting disease appeared to arise more from their irregularities than to be the sequelæ of scurvy; therefore, I seldom did more than administer emetics and purgatives, of tartar emetic, calomel, and Epsom salts. Some I ordered to be blistered, or to have pustulation excited on the chest by tartar emetic ointment; but from the external applications little benefit was derived.

A few men who were suffering from other complaints before they got the scurvy, and some who contracted disease afterwards, chiefly from excess in drinking, remained long on hand; but, generally speaking, notwithstanding their irregularities, the scorbutic patients recovered favourably at Bathurst. The following is an outline of a few of their cases:—

CASE XII.—John Devine, 75th regiment, æt. 26, eleven years in the service, of healthy natural constitution, but given to drink, was taken ill at Fort Wellington, in August 1836, with sore and tender gums. He fell away in flesh, and felt weak, but was able to continue at his duty for five weeks after contracting these symptoms, when he was sent to regimental hospital at King William's Town. His left leg had become black; the knee, leg, and ankle swelled, and his mouth continued sore; and as he was not getting better, he was sent, in November, to Graham's Town. In his regimental hospital at King William's Town, he says that he got half diet, with wine and porter, and that when he was sent to Graham's Town his gums had grown over his teeth. The pain of his legs, and tenderness over his whole body, were excessive, and he had withal become so weak, that he was unable to rise, or to turn himself in bed. He gradually recovered there, and in the month of December was sent in a convalescent state to Bathurst, his only complaint being weakness.

CASE XIII.—Dennis Humes, 75th

regiment, *æt.* 30, twelve years in the service, states that he had been affected with a pulmonary affection, at Fort Cox, long before he got the "*black leg*," but was not medically treated for it. In July, at King William's Town, he was seized with pain in his ankles and legs, swelled and bleeding gums, shortness of breath, and pain in the back and chest. He was admitted into hospital there, kept his bed for fourteen days, and got some pills, brandy, and wine; and he was afterwards in quarters doing his duty for a month, when, in September, he was transferred as convalescent to Graham's Town, but was not taken into hospital, and in a short time was sent back to King William's Town, where he was soon obliged to go into hospital again, on account of pain in the chest. His mouth was not then sore, neither were his legs swollen nor black; but after being in hospital nine days, he was transferred to Bathurst in the beginning of December, his only complaint being pain of the chest, which appeared to me to be the effect of, or to be kept up by, intemperance. On the 23d February he was sent from Bathurst, on account of being an habitual drunkard.

CASE XIV.—Cornelius Ahem, 75th regiment, *æt.* 30, twelve years in the service. In 1834 had dysentery and eruptions on the skin. In October 1835, while at Fort Warden, in the New Province, he got a pulmonary attack; and in July 1836, he got the "*black leg*," with pains in his ankles and loins. He fell off in flesh, felt very weak, and his mouth became tender and sore. In September he was sent to King William's Town, was in hospital there only two days, and got half diet, with half a gill of brandy daily, and was then transferred to Fort Willshire, where he recovered from the black leg and soreness of the mouth; but as cough and pain of the chest continued, in the end of December he was sent to this place, where he has had no scorbutic symptoms; and he has been treated by venesection and blisters to the chest, together with antimonials and purgatives, for the pulmonary affection.

CASE XV.—William Patterson, 75th regiment, *æt.* 40, twenty years in the service. In June 1836, while stationed at Fort Waterloo, was seized with head-

ache, giddiness, and cough, lost his appetite, and could not sleep, but was still able to continue at duty for a fortnight, when his legs became swelled, hard, and black, and his mouth and gums tender and sore, upon which the pain of his head ceased. In July he was sent to King William's Town, to hospital, where he remained a month, got a pint of porter and two gills of wine daily, and had vinegar, salts, nitre, and other medicines, but did not get better, and was sent to Graham's Town in the latter end of August. He was not taken into hospital there, but remained in barracks, where he had wine daily; and in October he was well enough to be sent back to his regiment at King William's Town. On his journey thither he was exposed to wet and cold, and his gums again became sore; but there was no return of the discoloration of the legs. He was admitted into hospital, and remained in it a week, after which he was transferred to Fort Willshire, and in the end of December came to Bathurst, his symptoms being weakness, and pain in the back, shoulders, and chest, with a hard tumor on the sternum, and others of the same nature on the head, but which did not appear to be of a scorbutic character. He still remained in hospital.

CASE XVI.—John Sadler, 75th regiment, *æt.* 43, twenty-five years a soldier. In July, when at King William's Town, he got pain and swelling of the right foot, ankle, and whole of the limb, and was taken into hospital. The limb became black from the hip to the heel. His mouth had been sore from the commencement, and he had frequently felt sick and qualmish. He got half a bottle of porter and two gills of wine daily, with some lemons and vegetables, and often salts; and he had alum gargles for his mouth. He remained fifty days in hospital, but got weaker and weaker, until at last he was unable to leave his bed; and in this state he was removed in a waggon to Graham's Town. He was there taken into hospital, and got little medicine, but good diet, with plenty of vegetables; and after twenty-four days he was able to leave his bed. Soon after this his mouth got well, and the swelling and blackness of the limb subsided; but the contraction and stiffness of the knee-joint continued. In November he was

transferred to Bathurst, at which time he was not scorbutic, but still affected with this contraction, and also with pain and oppression of the chest, which he says he first felt at Fort Cox in 1835. He remains in hospital under treatment.

MEDICAL GAZETTE.

Saturday, November 18, 1837.

"Licet omnibus, licet etiam mihi, dignitatem
Artis Medicæ tueri; potestas modo veniendi in
publicum sit, dicendi periculum non recuso."
CICERO.

CERTIFICATES FOR INSURANCE OFFICES.

WE are glad to see that the certificates which medical men sign for patients who want to insure their lives, have become the subject of warm and repeated discussion; for the common-sense conclusions to which such an investigation must inevitably lead, will be beneficial, in the first place to the profession, and secondarily to the public at large.

The first point mooted is, whether the medical practitioner who signs such a certificate, ought to be paid at all? In any profession but ours, such a question would be thought to savour of insanity. If a man were to expect a surveyor to draw up a detailed account of the dilapidations of his house gratis, or if he were to hope to get a written opinion on some legal subtlety from a barrister, for nothing, we suspect that it would go rather hard with him on a writ *de lunatico inquirendo*. The jury would infer, with some reason, that a man so grossly ignorant of the ordinary affairs of the world was incompetent to take care of his own property. With us, unluckily, matters are very different. There is a large heretical sect in society who seem to think that in the higher walks of our profession, at any rate, gratuitous services should be the rule, and payment the exception; that peers of the realm, and

great landholders, may pay; but that friends, and friends' friends, should be privileged to the fourth link of connexion: bits of paper must, of course, be signed gratis; to take a fee for such a thing as a certificate when offered, far more to demand it, would be "shabby." We begin, however, to have a glimpse of better days; the dictates of common sense are listened to, and practitioners in various quarters have declared that they must be paid for filling up these same documents, however much interested parties may disapprove of such proceeding.

We think it is very obvious, also, that these certificates deserve more than a common fee. A written opinion necessarily involves a greater responsibility than the ordinary revocable advice given to a patient—*littera scripta manet*; besides, it is really a much more difficult task to strike the balance of a man's constitution—to weigh his headaches in one scale and his equable pulse in the other, and from many minute circumstances to decide whether he is a good life, than to judge from present symptoms what medicine or diet is likely to relieve him. Hence we should think a fee of two guineas a very moderate recompense for such a service.

But a far more difficult question remains to be decided, namely, by whom ought the practitioner to be paid?

At first sight it might seem that the insurance office ought to pay; partly because the formal application to the medical man comes from the office, and partly because the insurance company gains by the bargain. The former of these reasons is rather a trifling one; for if in equity the patient ought to pay, it might easily be arranged that the application should come from him, or that he should repay the company for the fee given to his medical attendant.

That it is the company which gains by the transaction, if not a more substantial, is a more plausible argument;

for as the tables on which their policies are founded are formed from the duration of ordinary lives, and the company takes none but the most select lives at the ordinary premium, it requires but a glance to see that the balance of policies must be extremely advantageous to them; and accordingly we find that any great insurance office which has lasted for fifty or a hundred years, counts its gains by thousands and hundreds of thousands. Of late, indeed, we have had some insurance companies whose tables are founded on calculations taken from more picked lives, and who consequently give more favourable terms. Still, however, their tables do not come up to the actual average of life among the classes who insure, and the resulting profit is considerable. As the mass of policies, therefore, produce a profit, each insurer may be said to lose something; for it would be illusory to take, as some do, the case of a man who dies immediately after having insured his life, and contrasting the large gain which accrues to his family with the small gains derived by the company from a number of policies, to exclaim, see how much is gained by the insurer in one instance, and how little by the company in each of the others. This is as inconclusive as if we were to reason on a single case of the contrary kind, where the insurer continues to pay his annual contribution into the joint-stock treasury for fifty or sixty years, till he has paid double the amount of his policy, and till, perhaps, all whom he originally intended to benefit have disappeared from the stage of life.

When the problem is fairly stated, and accurately solved—that is, when the results of a large number of policies are balanced, the company always turns out to have been the gainer; and therefore each insurer may be considered a small loser. But this trifling loss is much more than counterbalanced by two great

advantages; namely, the freedom from anxiety, and the forced economy, which attend it. Supposing it to be certain that some one out of a thousand persons would be subject every year to a loss of 500*l.*, how great an advantage it would be if each person could be secured against the possible calamity by an insurance of twelve shillings per annum, even though this would be twenty per cent. more than his share of the loss. Would not the freedom from anxiety be worth the extra two shillings? Again, the forced economy, in our profession especially, is an immense gain, an absolute blessing. How few medical men, with an income varying from 800*l.* to 1200*l.*, would steadily lay by 150*l.* or 200*l.* yearly. So many temptations to increased expense come in their way—so many fond hopes would be cherished that a dash would take the town by storm, and that the shadow would produce the substance—that the attempted economy would rarely be continued; but a life-insurance once begun, cannot well be dropped; and the sum to be annually paid is soon looked upon in the same light as house-rent and taxes.

The bargain, therefore, being essentially an equal one, between the insurance company on the one hand and the mass of insurers on the other, it seems reasonable enough to conclude, that as the office pays its own medical adviser, the insurer should pay his.

It has been anticipated as a possible inconvenience, that, in a few disputed cases, the evidence of the patient's medical attendant might be weakened by the fact of his having been previously paid for it. The same kind of objection, however, that would then be made to the certificate, might be made even now; for if the opposing counsel is to suppose a practitioner so dishonest as to give a false certificate for a two-guinea fee, he might still more plausibly

allege that he had falsified the document from the hope of retaining an old patient—a sort of life annuity. We do not ourselves think any thing of this objection, nor imagine that paid certificates would lose their credibility. On the contrary, from the greater care with which they would unquestionably be drawn up, they would have the additional value which punctilious accuracy inevitably confers. Those who are afraid of a diminution of credibility in the evidence of the patient's medical adviser, if paid by him (like those who imagine that the bargain is a gainful one to the company alone), propose that he shall be paid by the company. To this the plain objection is, as before, that the contract being advantageous for both parties, the preliminary expenses should be divided between them. Mr. Farren, however, the resident director of one of the life-offices, has ferreted out another objection (a curious one enough). He says, "The business in which I have been actively engaged for fifteen years has brought me acquainted with many hundreds of medical professors who do not need my feeble testimony in favour of their acumen, liberality, and honourable feeling; but, sir, as there must be unworthy as well as worthy persons in all professions, it is sufficiently obvious that a very profitable source of business might be created for dishonourable practitioners, or rather for those who had no practice at all, from the concession sought by the Leeds combination. Men who never intended to complete insurances might make numerous proposals, with the certainty that the medical referees would be enriched, although the offices would as certainly be the losers of the fees they were silly enough to pay on faith of such references*."

To this, a writer, under the signature of W. A. W., in the *Times* of Novem-

ber 15th, replies, that this difficulty might be got over by paying the fee in those cases only where the insurance is effected. Let us observe in our turn, upon this, how readily medical men are thrust to the wall upon all occasions. If (to return to our old instances) a surveyor makes out a list of the dilapidations of a house, no one dreams that he is not to be paid because the vendor and purchaser cannot come to terms. Or suppose that the other day, when Sergeant Talfourd gave his written opinion as to the lawfulness of doctors talking publicly upon the unwholesomeness of arsenical candles, he had said "No!" instead of "Yes!" and thus put a gag on a hundred eager mouths, would the client have thought of withholding the fee, or rather of asking for it back again?

Mr. Farren makes another objection, which, we humbly submit, is even weaker than the last one. He supposes a fraudulent transaction, where rupture, fistula, hæmoptysis, or some other grave disease, has been concealed, and where the insured life of a man of thirty is not equal to that of a hale man of sixty. In such a case the directors would now bring evidence to show that the proposer's witness was a rogue, and had wilfully concealed an important fact; but Mr. Farren thinks that, under the new system, this could not be done, and that judge and jury would unite in saying to the directors, "this is your own witness, and you are bound to believe his statements." This we believe to be a grievous mistake; for it is quite obvious that the extraneous testimony which is now admitted to rebut that of the proposer's witness, would be equally good then, particularly if the company had consulted the witness at the request of the proposer.

We think it needless, however, to enter into a disquisition on this point, or to lose ourselves in the intricacies of the

* Letter in the *Times* of November 6, 1837.

law of evidence, as we have already given it as our opinion that the insurer ought to pay for his own witness; but then the witness ought to have his remuneration secured to him, and we would propose an arrangement by which almost every inconvenience might be avoided. It is, that the medical man, the insurer's witness, should in the first instance be paid by the office, and that this should be repaid by the fee being charged upon the policy. This would place the medical man in a very different position from that which he now occupies — he would know where to apply for his fee, and would have no difficulty in doing so; whereas at present he often does not, at the time, even see the party to whose general state he certifies; and if he did see him, would often, if not always, go without his fee rather than be compelled to ask for it.

As for the duty of medical men, it is clear enough; singly, or in combination, they should follow the example of the practitioners of Leeds, who passed a resolution some few months ago, stating that they would in future return the questions of insurance companies unanswered, unless accompanied by a suitable fee. Those, however, who think with us that the contract between the company and the insured must, in the long run, be advantageous for both parties, will not encumber their resolution with the words adopted at Leeds, "considering that the insurance company is the party benefited by the information thus obtained." Who gains most by the contract is a matter of indifference to the medical practitioner; he should act with the utmost impartiality, and send back without answer the unballasted questions that are thrust upon him, from whatever quarter they may arrive:

Tros Rutuluse fuat, nullo discrimine habeo!

ANIMAL MAGNETISM.

At page 291 will be found a communication from M. le Baron du Potet, for the insertion of which he has trusted to our "justice" and "impartiality." *Audi alteram partem* is an appeal which, couched in temperate and courteous language, as in the present instance, and confined within reasonable limits, it would be unhand-some to disregard. We have therefore complied with his request; nor, as to the rest, do we think that an historical sketch of Mesmerism will be without its interest to our readers.

ST. GEORGE'S HOSPITAL.

Punctured Wound, involving the Diaphragm, Spleen, Stomach, and Liver.

JAMES RANDALL, æt. 40, valet, was admitted into St. George's Hospital, November 3, 1837, under Sir Benjamin Brodie, with punctured wound in the side. He was admitted at 2 p.m., having received a stab with a table-knife but a short time before. There was found a wound about an inch and a half long, on the left side, just below the true ribs. The policeman stated that the blade of the knife was stained with blood for four inches and a half of its extent. It was noted by the house surgeon, Mr. Berkeley, that his extremities were cold, pulse feeble and slow, countenance pale and anxious, with but little bleeding from the wound. Bottles of hot water were applied to his feet, and lint, plaster, and wet compress, to the wound.

Three hours after his breathing became difficult; his pulse rose in frequency, but was very small and compressible.

Ten hours after he was bled to 3x., and the following was prescribed:—

Hydrargyri Chloridi, gr. x. statim in formâ pulveris, c. haustu sequente.

Misturæ Camphoræ, ʒiiss.; Liqueoris Opii Sedativi, ℥xxv. M.

12 hours.—The pulse, which had sunk, is now nearly the same as before the bleeding; he breathes more easily.

16½ hours.—Complains of pain over the surface of the abdomen.

Fotus abdomini.

20 hours.—Pulse continues very frequent; pain more severe.

V.S. ad ʒviij. Hydrargyri Chloridi, gr. iij. 6tis horis.

23 hours.—Pain in abdomen undimi-

nished; he shrinks under pressure. Hiccup; pulse 140, very small; bowels not open.

Hirudines xx. abdomini; postea fotus.

24 hours.—Soon after the leeches were put on he became so weak that they were immediately removed, and the bleeding stopped. The coffee and tea which he has been drinking has, within the last half hour, begun to run out through the external wound; pain over the abdomen considerable; extremities cold; skin perspiring; extreme thirst. An injection of warm water and oil was administered, and a quarter of a grain of opium was added to each pill.

26 hours.—Complains of a desire to make water; the catheter was passed, and one ounce drawn off. Pulse very rapid and weak; very anxious expression of countenance.

28 hours.—His pulse has been gradually getting weaker, and his extremities more cold. Very soon after this report he expired.

I am indebted for the above detail of his symptoms to Mr. Berkeley, the house-surgeon, who noted down his symptoms every two hours.

Post-mortem examination.—The tenth rib, over which the wound was situated, was fractured obliquely from behind forwards. The instrument had penetrated the space between the tenth and eleventh ribs, passed through the pleura and diaphragm, but produced no injury of the lung, further than slight ecchymosis of the contiguous portion; it then glided through the upper and fore part of the spleen, and passing posterior to the gastro-splenic omentum, without injuring the colon, penetrated the coats of the stomach immediately posterior to where the anterior and posterior layers of peritoneum pass off to inclose the spleen: then coming out at the anterior surface of the stomach, at its left side it had plunged into the substance of the left lobe of the liver, at the middle of its inferior concave surface, and having passed through, came out at its superior surface, midway between its anterior and posterior margins, about one inch from the suspensory ligament, and was then arrested by the posterior surface of the sternum, where there is a fissure left, as made by a knife or some sharp instrument, close to the articulation with the fourth rib. There was a considerable quantity of blood collected in the cavity of the peritoneum. About the middle of the superior convex surface of the liver was a circular spot of ecchymosis, about the size of a shilling. The spleen was very much enlarged, and turgid with dark blood. The

stomach was very flaccid, and contained much bile and glutinous fluid, which was collected at its pyloric extremity. The duodenum was filled with fluid matter, and its coats congested with venous blood. The coats of the jejunum were also similarly congested, but to a less extent; they were thickened more than naturally. The ileum was empty and natural, except at its inferior extremity, where there was much ecchymosis under the serous coat.

The right testis was smaller than natural. There were two flat circular cicatrices, or rather scars of former ulcers, on the glans penis, at the inferior part, one on either side of the frænum. Some sebaceous secretion was collected behind the corona glandis.

ACCIDENTS ADMITTED AT THE LONDON HOSPITAL.

Mr. ANDREWS, *Surgeon*.

Mr. ADAMS, *Assistant-Surgeon*.

Oct.	Sex.	Age.	Case.
31.	M.	31	Scald.
	M.	26	Compound fracture of the lower jaw.
	M.	30	Retention of urine.
Nov.			
1.	M.	65	Retention of urine.
	M.	58	Fractured ribs.
	M.	17	Contused chest and neck.
2.	M.	50	Dislocated humerus and fractured ribs.
	M.	41	Fractured leg.
	F.	25	Puncture and wound of the throat.
4.	M.	50	Contused eye.
6.	M.	40	Fractured leg.
	M.	26	Fractured tibia from gunshot.
	M.	39	Fracture of both femora.
			Death.
	F.	67	Fractured humerus.
	F.	17	Injured back.
	F.	82	Fractured thigh.
	F.	48	Fractured ribs.
	M.	46	Rupture of internal lateral ligament of the knee joint.
	M.	11	Burn.
	M.	15	Burn.
	M.	13	Burn.
	M.	61	Contused eye.
	M.	10	Fracture of the palatine processes of superior maxillary and palate bones, from gun shot.

In-patients 24

Out-patients 50

Total 74

MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, November 14.

Observations on the Result of Amputations in this and other Countries. By BENJAMIN PHILLIPS, F.R.S.

THE object of the author, in the paper now presented to the Society, is, in the first place, to prove that the opinion commonly entertained with respect to the mortality succeeding to amputation is incorrect; secondly, to exhibit the results of inquiries undertaken in France, Germany, America, and Great Britain, as to the mortality consequent upon this operation, and to compare these results with each other, for the purpose of ascertaining to what extent is justified the belief in the greater success of amputation in our own than in other countries; and thirdly, to adduce evidence in proof that the treatment commonly employed in certain cases of old-standing disease, is less favourable in its results, and less in consonance with pathological principles, than that proposed by himself, in the course of the present inquiry. The author lays much stress on the importance of possessing more exact records of the results of operations in large hospitals, as the only sure data upon which to found a judgment of their comparative safety; the memory of individuals being, as he has often had the opportunity of proving, quite insufficient to furnish even an approximation to the truth. The author admits that many hospitals may be so circumstanced, either permanently, or at particular periods, as that the results of amputations in them should seem to be peculiarly unfavourable; but he has endeavoured to obviate this difficulty by making such a selection as would produce something like a compensation in this respect. The amputations included in this inquiry, are those of the arm, and the forearm, the thigh, and the leg; all have been performed within the last four years in civil hospitals, or in the private practice of hospital surgeons. The gross number of such cases is 610. Of these, 490 are reported "cured," and 150 died, either in consequence of the operation, or from the progress of the disease for the relief of which it had been performed. The author proceeds to analyze the gross number, and exhibit the proportion furnished by the different countries comprised in the inquiry, in a table, from which it appears that the proportion of deaths after amputation, in Great Britain and France, is a fraction below, while in Germany and America it is a fraction above, the average. It would be impossible to detail, within the limits of a brief abstract, all the observations upon which the author founds the opinion that the amount of mortality is greatly increased by the almost entire abandonment of the system of consecutive union of stumps, resulting

from the amputation of long-standing chronic diseases, attended with profuse suppuration; an opinion which leads the author to recommend a modification, in such cases, of the present mode. Although the author is not an advocate for the substitution of consecutive for immediate union, in any class of cases, he is strongly impressed with the opinion that many of the evils resulting from immediate re-union, in an extensive class of cases, might be obviated by establishing, in the vicinity of the part where amputation is to be practised, an artificial suppurating surface, by means of a seton or issue, and maintaining such secretion for a time after the healing of the stump. The author professes to have no direct evidence of the efficacy of the proposed modification; but he thinks himself warranted, by the premises, in maintaining the probability that it would render unnecessary the plan of having recourse to secondary union, whose advantages in any cases are almost counterbalanced by great inconvenience.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, Nov. 16, 1837.

Job Harrison, Chester.—Jenner Plomley, Sussex.—E. H. S. Banks, Folkestone.—E. C. Seaton, Kent.—Rice H. Anwyl, Bala, Merionethshire.—G. J. Parker, Bristol.—G. E. Williams, Brighton.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, Nov. 7, 1837.

Abscess	1	Heart, diseased	2
Age and Debility	30	Hooping Cough	6
Apoplexy	8	Inflammation	18
Asthma	12	Bowels & Stomach	5
Cancer	1	Brain	3
Childbirth	5	Lungs and Pleura	3
Consumption	50	Insanity	5
Convulsions	27	Liver, diseased	1
Croup	2	Measles	8
Decidua or Teething	2	Mortification	1
Diabetes	1	Rheumatism	1
Diarrhoea	1	Small-pox	1
Dropsy	8	Thrush	1
Dropsy in the Brain	8	Unknown Causes	63
Fever	17		
Fever, Scarlet	5	Casualties	4
Fever, Typhus	11		

Increase of Burials, as compared with } 96
the preceding week

NOTICE.

The paper of Mr. Gorham, on the Pulse, is unavoidably postponed till next week.

ERRATA.

In Mr. Schweitzer's remarks on Chirayitine, Quina, &c. in our number for October 28, for "Causarra," read "Causcora;" for "sulphate," read "muriate of barytes gave a white precipitate, not soluble in muriatic acid, proving the presence of *muriatic* (instead of sulphuric) acid.

WILSON & SON, Printers, 57, Skinner-st., London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, NOVEMBER 25, 1837.

LECTURES

ON

FORENSIC MEDICINE;

Delivered at the Aldersgate School of Medicine.

By SOUTHWOOD SMITH, M.D.

Physician to the London Fever Hospital.

LECTURE II.

ON EVIDENCE.

Acquaintance with the nature of Evidence necessary to the Medical Witness—Insufficiency of Common Sense—Desirableness of Systematic Knowledge—Sources from which such Knowledge may be derived—Evidence—Testimony—Complex Evidence—Facts the subject-matter of Evidence—Principal and Evidentiary—Physical and Psychological—Positive and Negative Facts—Cause of Belief in Evidence—Analysis—Experience—Mental Law to which belief in Evidence is referable—Experience the ground of Belief in Evidence—Cause of Belief in Testimony—Personal and Real Evidence—Personal Evidence, voluntary or involuntary—Testimonial Evidence, original or unoriginal—Real Evidence.

Acquaintance with the nature and evidence necessary to the medical witness.—Before entering on the individual topics which form the subject matter of the present course, there is one thing to be considered, which is to forensic medicine what anatomy is to surgery. In a surgical operation not a single movement of the hand can be made with the prospect of advantage, nor even with safety to the patient, unless it be guided by an intimate acquaintance with structure. As medical witnesses, you can neither collect the facts which are required to guide the jury and the Judge, nor, when you have collected them, state them with the requisite clearness, exactness, and force, unless you understand the nature of evidence.

Yet this important subject has, in

general, been so much neglected by the members of our profession, that there are many amongst us who have no correct conception even of the very meaning of the term; and there are few who have ever taken a comprehensive and scientific view of it.

Insufficiency of common sense.—Some may even regard an account of evidence in general, as unnecessary in relation to forensic medicine, although forensic medicine is merely the application of a particular species of evidence to a special purpose. The facts, it may be said, which form the subject matter of the evidence which the medical witness is called upon to give in a court of justice, must be familiar to every well-educated medical man. It can require but little art to collect them; it must be easy to state them; and on this, as well as on so many other subjects, common sense is a reader and safer guide than science.

The answer to this sort of cloak for indolence and ignorance has been given in a masterly manner by a distinguished logician of our own day, in the following words: "There are many," says Archbishop Whately, in his *Treatise on Logic*, "who allow the use of systematic principles in other things, who are accustomed to cry up common sense as the sufficient and only safe guide in reasoning. Now, by common sense is meant, I apprehend, (when the term is used with any distinct meaning) an exercise of the judgment unaided by any art or system of rules; such as we must necessarily employ in numberless cases of daily occurrence; in which, having no established principles to guide us,—no line of procedure, as it were, distinctly chalked out,—we must needs act on the best extemporaneous conjectures we can form. He who is eminently skilful in doing this, is said to possess a superior degree of common sense. But that common sense is only our *second-best* guide—that the rules of art, if judiciously framed,

are always desirable when they can be had—is an assertion, for the truth of which I may appeal to the testimony of mankind in general, which is so much the more valuable, inasmuch as it may be accounted the testimony of adversaries. For the generality have a strong predilection in favour of common sense, except in those points in which they respectively possess the knowledge of a system of rules; but in these points they deride any one who trusts to unaided common sense. A sailor, for example, will perhaps despise the pretensions of medical men, and prefer treating a disease by common sense; but he would ridicule the proposal of navigating a ship by common sense, without regard to the maxims of nautical art. A physician, again, will perhaps condemn systems of political economy, of logic, or metaphysics, and insist on the superior wisdom of trusting to common sense in such matters; but he would never approve of trusting to common sense in the treatment of diseases. Neither, again, would the architect recommend a reliance on common sense alone in building; nor the musician in music; to the neglect of those systems of rules, which, in their respective arts, have been deduced from scientific reasoning, aided by experience. And the induction might be extended to every department of practice. Since, therefore, each gives the preference to unassisted common sense only in those cases where he himself has nothing else to trust to, and invariably resorts to the rules of art, wherever he possesses the knowledge of them; it is plain that mankind universally bear their testimony, though unconsciously and often unwillingly, to the preferableness of systematic knowledge to conjectural judgments."

Desirableness of systematic knowledge.—I hope I need say nothing more to engage your attention to the attempt I am about to make to put you in possession of systematic knowledge, on a subject which may be regarded as the basis of forensic medicine. If the exposition which I propose to give of the general nature of evidence should convey to you a clear and accurate conception of this subject, the rules which should guide you in the collection and deposition of evidence in relation to your own particular department—that is, the application of the general doctrine of evidence to the particular subject of forensic medicine, will be easy, striking, and most instructive.

Sources from which such knowledge may be derived.—It would be altogether out of place, on the present occasion, to enter much into detail. The most that your time will allow me to attempt, is to indicate the main points which deserve your attention. You will

find minute and ample information in the great work of the English jurist, the illustrious Bentham, entitled "the Rationale of Judicial Evidence," in which he has entered into a profound and detailed investigation of the subject, and has wrought out every point with a degree of completeness which leaves hardly any thing more to be done or desired. You will also find a brief but clear analysis of evidence in Mr. James Mill's masterly "Analysis of the Phenomena of the Human Mind." In relation to this subject, you should also read with particular care that portion of Professor Brown's "Lectures on Moral Philosophy," which treats of cause and effect; and Dr. Whateley's (the Archbishop of Dublin) "Treatise on Logic." The attainment of the mere knowledge, inestimable as it is, contained and unfolded in these works, is the least part of the advantage they are capable of affording: the great benefit to be derived from their diligent study, is the habit of correct reasoning, to which such study tends to train the mind, and which, once formed, will render the acquisition of any branch of knowledge to which the mind can apply itself, easy and sure.

Evidence.—A sailor, shipwrecked on an unknown coast, sees the print of a man's foot on the sand. The perception of this foot-print produces in his mind the belief that a man has been present in that place. Here, then, are two events; the first the antecedent, the second the sequent; the latter produced by the former. The first, the antecedent, the footmark on the sand, induces the belief of the second—the sequent, the existence of a man. Now, the first event, the foot-print on the sand, is called the evidence; the second event, the persuasion produced of the existence of a man, is denominated the belief induced by the evidence.

Testimony.—The sailor who has seen the foot-print reports it to his companions who have not quitted the wreck. Instantly they have the same belief. In this case the report of the sailor to his companions is called testimony; the effect produced upon their minds, namely, the persuasion of the existence of the event testified to, is denominated belief in testimony.

The first event to the sailor is an object of sense; this is to him evidence of the second event. The first event to his companions is the affirmation of their comrade; this is to them evidence of a second event, the existence of the print; and this is evidence of a third event, the existence of a man. Here, then, is what is termed evidence of evidence; the testimony, evidence of the print; the print, evidence of the man.

Complex evidence.—If the evidence con-

sist of more than one event, it is said to be complex. Thus, after discovering the print on the sand, the sailors found a stick, fashioned into a club; on searching further they discovered a spear, and at some distance they saw the embers of a recent fire. These circumstances are all so many additional events, tending to produce the same belief, namely, the presence of men in this place. Now, the whole of the events which go in this manner to form a case of belief, constitute the evidence of that case.

Facts the subject-matter of evidence.—We now, then, can form a clear conception of the nature of evidence. The term evidence expresses a fact, the tendency of which, when presented to the mind, is to produce a belief in the existence of some other fact. These two facts bear to each other, as I have stated, the relation of antecedent and sequent.

Principal and evidentiary fact.—The first fact, the antecedent, is always a fact already known; the second, the sequent, is never a fact already known, but a fact to be sought. This latter, the fact sought, is termed the principal fact; the former, the fact already known, is called the evidentiary fact, because it affords evidence of the existence of the fact sought.

Poison is suspected to have been administered to a deceased person by a particular individual; that is the unknown fact—the principal fact—the fact sought. The facts known, the evidentiary facts, are, that the suspected person is discovered to have had poison in his possession; that he was observed to give the deceased the last thing she drank; that by an eye unseen he was detected in slipping something secretly into the cup; that soon after taking the contents of this cup the symptoms of poisoning came on, and so on. Suppose the existence of these facts to be without doubt, it is clear that the tendency of each of them is to produce in the mind a conviction of this other fact, that the individual in question really did administer the poison. The facts bearing on this point, and having this tendency, are all evidentiary facts; and the whole of them taken together constitute the evidence of the case.

Facts having such and such a tendency are also termed circumstances. Any fact may be a circumstance with reference to any other fact; *CIRCUMSTANTIA*, standing about or around—objects by which a given object is encompassed, which may be considered as standing around it.

Physical and psychological facts.—Facts, whether principal or evidentiary, are of two classes—physical and psychological. A physical fact is one which belongs to some physical body; a psycho-

logical fact is one which belongs to some living being. Physical facts include the whole range of phenomena comprehended in the inorganic world, and form the subject-matter of natural philosophy; psychological facts include the whole range of phenomena comprehended in the world of mind, and form the subject-matter of mental philosophy.

States of things are also termed facts, and so are events. By an event is meant some motion which comes about in the course of nature. The fall of a tree is an event; the existence of the tree is a state of things: both are alike facts.

In like manner an action, a name given to an event which has its source in the human will, is termed a fact.

Positive and negative facts.—Facts are either positive or negative. A certain thing exists, and is designated by a certain name. This constitutes a positive or an affirmative fact. The declaration of the non-existence of that thing constitutes a negative fact; but it must be at once obvious to you, that the only really existing facts are positive facts; for a negative fact is nothing more than the non-existence of a positive one.

Cause of belief in evidence.—When an evidentiary fact is presented to the mind, we believe in the existence of the fact evidenced to, the principal fact. This belief is a part of our nature; it is a part of the constitution of the human mind. It does not depend upon our will. We believe, and we cannot do otherwise. When an evidentiary fact possesses a certain degree of strength, we can no more help believing in the existence of the fact of which it is the indication, than we can help the sensation of light when we direct our eyes towards a luminous body.

What is the cause of this belief in evidence? If as mental analysts you trace this remarkable psychological phenomenon back to its source, you will find that it is referable to the mental law which is termed the association of ideas.

Analysis.—During the whole course of our lives we are witnesses of incessant trains of events. The events of which these trains are composed succeed each other in a certain order. Some are antecedents, some are sequents. Between some antecedents and some sequents, we observe that the order is constant, that the connexion is invariable. We never see the one take place without the other. We never see that the one is sometimes the antecedent and sometimes the sequent; but we observe that the antecedent is invariably the antecedent, and the sequent invariably the sequent. Now in all the cases in which we observe this constant, this invariable conjunction between two events, an association is

formed in our mind between them, in the order in which they take place. The association of the two events in the mind is as inseparable as the combination in which they occur in nature. A stone thrown into the air falls to the ground. Between the antecedent, the stone thrown into the air, and the sequent, the falling of the stone to the ground, the connexion is fixed and invariable. The idea of the one calls up the idea of the other. Do what we may, we cannot think of the one without thinking of the other. The two ideas are forced upon us at the same time, and in the same order; and by no effort which we are capable of making can they be disjoined.

On the other hand, in regard to a great number of events, we observe that there is not between them this constant conjunction; that they do not bear to each other the relation of invariable antecedents and sequents; that sometimes one happens without the other; and that, when they do happen in conjunction, they occur sometimes in one order, and sometimes in another. The crow flies sometimes north, sometimes east, and sometimes west. For ought we have observed, it flies as often in one direction as in another. The consequence is, that no association is formed in our minds between the flight of the crow and its flight in any particular direction. But no instance has ever been witnessed by us, in which a stone thrown into the air did not fall to the ground; consequently between the idea of a stone thrown into the air, and the idea of its falling to the ground, there is formed in our minds an indissoluble association.

One remarkable circumstance connected with these two different classes of events, is, that the events which are found to have no necessary connexion with each other, pass readily out of the memory, and are soon forgotten; while the events between which a constant connexion is observed to be established, make a deep and lasting impression upon the mind, and cannot be forgotten. From the first moment of our consciousness, we have been incessantly employed in observing these constant conjunctions of events. We have had the greatest possible interest in looking out for, and ascertaining them. All that we know of external nature—all that we know of ourselves—all that we know of the influence of physical agents upon our own living organization—all that we know of good and evil to ourselves and others, is most intimately connected with these observed, these ascertained antecedents and sequents. Without a knowledge of many of these constant conjunctions of events, we could not maintain our existence for a single moment; and

in general, when we increase our stock of knowledge, we do nothing more than merely add to the number of events which we previously knew to have this relation of constant conjunction,—other events which we now for the first time ascertain to have that same relation.

Experience.—And it is this which constitutes our experience. That stock of accumulated knowledge to which we give the name of experience, consists of a number of events the existence of which we have ascertained, and between which we have discovered, or suppose we have discovered, either that there is a constant conjunction or that there is not. In proportion to the number of events the existence of which we have ascertained, is the extent or range of our experience; in the degree in which our notion of their constant conjunction or non-conjunction is just, is the soundness of our experience.

Mental law to which belief in evidence is referable.—There is no analysis of a compound substance, effected by the modern chemist, more complete and satisfactory than that accomplished by the mental analyst, when, by the resolution of compound ideas into the elements of which they are composed, he succeeds in progressively tracing back some of our most complex states of consciousness to the simple mental law of association; which is to mind, what gravitation is to matter. From what I have already said, you will be able clearly to see that what we call belief in evidence is referable to this mental law; and you will be able, each for himself, to trace it back to this law.

You have observed the invariable conjunction between two events—the invariable relation of antecedents and sequents. The existence of one of these events, the sequent, is made known to you: instantly you are convinced of the existence of the other, the antecedent. Why? Because, by reason of the indissoluble connexion between the two events already established in your minds, through the operation of the law of association, you believe (and you cannot but believe) in the existence of the antecedent, the moment you are satisfied of the existence of the sequent.

Such, then, is the cause of the belief in evidence. It is the necessary result of that constitution of the human mind by which such an association is formed between two events, observed to occur in invariable conjunction, that the one cannot be presented to the mind without its calling up the presence of the other.

“This is no more Daniel Clarke’s bone than it is my bone!” exclaimed the murderer Houseman, when examined at an inquest on a skeleton that had been accidentally found in a field.

This man, whose habits were known to be profligate, and whose disposition was deemed ferocious, had long been suspected of having been concerned in the murder of a person of the name of Clarke. Fourteen years had elapsed since the event. The skeleton now found was supposed to be that of Clarke.

Housman, on being examined touching the matter, betrayed some confusion—indicated something of that involuntary emotion which results from what is commonly termed being conscience-stricken.

As if to divert attention from this, he suddenly assumed an air of levity, and, taking up one of the bones of the skeleton that lay beside him, exclaimed, "This is no more Clarke's bone than it is my bone."—"Then," instantly inquired the examiner, "where are Clarke's bones?" This unexpected question threw the conscience-stricken criminal off his guard. He could not conceal that he knew; he turned King's evidence; denounced Aram as the murderer; admitted that he had been an accomplice, and described exactly where the true skeleton would be found.

"In St. Robert's Cave (he said) it will be found, just by the entrance, and with the head towards the right."

There was found, exactly as described, a skeleton. On examining the skull, there was discovered a fracture on its left side, apparently produced by the stroke of some blunt instrument. The piece was beaten inwards, and could be replaced only from within.

"It is not a recent fracture (said the medical witness examined on the occasion). It could not have been produced by the instrument with which it was dug up. It appears to have been of many years' standing."

This is the story of the celebrated Eugene Aram.

It is obvious that this murder was discovered by a remote evidentiary fact, suggesting the principal fact. "This is no more Clarke's bone than it is my bone!" exclaimed the suspected murderer.

What was the train of thought that instantly passed through the mind of the examiner, on hearing this statement?

This is not Clarke's bone; you know that it is not; but you cannot know it is not, without knowing where Clarke's bones are.

If you know where these bones are, you probably know how they came where they are.

It is not likely that you can know how they came where they are, without knowing something about the circumstances of the death that preceded their being placed there.

What you know about such death, you

probably know from having been concerned in producing it.

Such was the train of thought. The evidentiary fact, the exclamation, suggested the principal fact—the perpetration of the murder. The knowing what the fact was not, suggested that there must be a knowledge of what the fact was. The connexion between the two was close and inseparable, and all that chain of reasoning which it has taken so much time to put into propositions, passed through the mind in an instant; for thought is quick.

That the suggestion and the reasoning were correct—that there was a real connexion between the events in question, is placed beyond all doubt, by the subsequent confession of the murderer by the murderer.

Experience the cause of belief in evidence.—From what has been stated, you can understand clearly what is meant when it is said that the ground of belief in evidence is experience. The fact observed by you, that such and such conjunctions of events are invariable, you are said to know from experience, because of the certainty of such facts your senses have given you uniform assurance; for all that you know from your own experience is resolvable ultimately into what you have been taught by your own sensations.

Cause of belief in testimony.—The cause of belief in testimony is perfectly analogous. That there exists in the human mind a propensity to believe in testimony, is matter of universal experience; and the like universal experience establishes the justness of that belief. Continue your belief in testimony as you have been accustomed to believe in it; the business of your life will go on according to its ordinary tenor: withhold your belief from testimony with the same regularity as that with which you have been used to bestow it, and if you should find it possible to preserve your existence at all, you certainly will not find it possible to preserve it without intense and unceasing suffering. On the knowledge you have of the states of men and things, depends the prosperity with which the business of your life is carried on; but of that knowledge, the portion which you can obtain from your own experience is but as a drop of water to the ocean; the rest of it of which you have need, must come to you, if it come to you at all, from testimony.

You believe, then, in testimony, and you cannot do otherwise if you would. Why? After what has been said of the cause of belief in evidence, you can be at no loss to assign the cause of belief in testimony.

A person bears testimony in relation to a certain event; the words of the testifier excite in your mind certain ideas in regard to the thing testified. Words call up ideas

by association. You know, from your own experience, the manner in which words are applied to events. From the first moment you have been capable of using words, or of understanding their import, you have employed them yourself, and you have observed them employed by others, in exact conformity with events. You have occasionally observed cases in which words are not employed in exact conformity with events, but these cases are few, in comparison with those in which they are so employed. The greatest liar that ever lived, speaks truth a thousand times for once that he utters a falsehood. In your ordinary intercourse with your fellow men, propositions affirming the existence of this or that fact are incessantly made, in a vast variety of forms. For the most part, as occasions arise in which you have an opportunity of verifying by your own perceptions whether the assertions made concerning these facts are conformable to the facts, you find that they are conformable. What is the consequence? That the connexion between the use of words, between affirmations and the idea of conformable existences, comes to be established as one of the strongest associations of the human mind.

The general conformity of testimony to the real state of things, of the facts reported upon to the reports made concerning them, is thus a matter of universal experience. Thus the ground of belief in testimony is experience, just as experience is the ground of belief in evidence; and an inseparable association being formed between the idea of the general conformity of the words of the testifier with the real state of the things testified, it is plain that the cause of belief in testimony is association, just as the same great mental law is the cause of belief in evidence.

"It is unaccountable that Asa should choose to be out of the way at such a time as this," said his mother, the wife of Ishmael Bush.

Esther and Ishmael Bush were the heads of a family of settlers in America, on those broad plains which stretch for immense distances to the basis of the Rocky Mountains. The wild position and habits of this party, and their perilous and spirit-stirring adventures, have been graphically depicted, by a masterly hand, in a narrative familiar to you all. Whether the narrative be fictitious or historical, it will serve the present purpose of illustration equally well.

Among the varied incidents described in this story, there is one in which an irresistible conviction that a certain individual had perpetrated a frightful crime, is produced by the testimony of a single witness, — a few words spoken by that witness pro-

ducing this conviction, even against evidence of no slight degree of force tending to produce a directly contrary persuasion.

In reply to the observation of his mother, that it was unaccountable that Asa should be absent at this particular time, Abiram, the brother of Esther, the uncle of the young man who was missing, the murderer of the youth, said, "it will be well if the boy has escaped the Tetons. I should be sorry to have Asa, who is one of the stoutest of our party both in heart and hand, fall into the power of the red devils."

The lad does not return; the family is thrown into a state of great alarm at his continued absence; and having set out in search of him, they at length discover the dead body in a thicket.

The two brothers who discovered the body, agree in their account as to the position in which they found it; seated nearly upright, the back supported by a mass of matted brush, and one hand still grasping a broken twig of the alders. Owing to the former circumstance the body had escaped the rapacity of the carrion birds long observed to have been hovering about the thicket; and the latter proved that life had not yet entirely abandoned the victim when he entered the brake. The opinion became general that the youth had received his death-wound in the open prairie, and had dragged his enfeebled body into the corner of the thicket for the purpose of concealment. A trail through the bushes confirmed this opinion. On a close examination of the spot it further appeared that a struggle had taken place on the margin of the thicket, as was apparent by the trodden branches, the deep impressions on the moist ground, and the lavish flow of blood.

"He has been shot in the open ground, and come here for a cover," said Abiram; "these marks clearly prove it. The boy has been set upon by the savages in a body, and has fought like a hero as he was, until they have mastered his strength and then drew him to the bushes."

On a careful examination of the corpse, with a view to obtain a more accurate knowledge of the injury sustained, it is found that a rifle bullet has passed through the body, entering beneath one of the shoulders, and making its exit by the breast. The very bullet is discovered. It is supposed to have come from the pouch of an old man, a wild hunter, a trapper.

"There is no mistake," said Ishmael. "Like many of the hunters he has a mark in his mould in order to know the work his rifle performs; and here you see it plainly—six little holes laid crossways."

"I'll swear to it!" cried Abiram, triumphantly. "He shewed me his private mark himself, and boasted of the number

of deer he had laid upon the prairies with these very bullets. Now, Ishmael, will you believe me when I tell you that old knave is a spy of the red skins?"

The traces of the different struggles which were to be seen between the spot where the first blood was spilt, and the thicket to which the murdered youth appeared to have retired as a place of refuge, were now interpreted into so many proofs of the weakness of the murderer, who would have sooner dispatched his victim had not even the dying strength of the youth rendered him formidable to the infirmities of one so old; while the danger of drawing some of the other hunters to the spot by repeated firing, was deemed a sufficient reason for not again resorting to the rifle after it had disabled the victim.

The trapper falls into the hands of the borderers, and is charged with the perpetration of the crime. The evidence adduced against him is the bullet found about the person of the murdered youth. "With this morsel of lead did he lay low as fine a boy as ever gave joy to a parent's eyes."

The defence of the accused was made in the following words:—"As we were all outlying about your camp, seeing that I was more skilled in scouting than the others, while they lay beneath in the cover I was sent upon the plain. As I lay in a low bed of grass I saw two hunters come nigh each other. Their meeting was not cordial, yet I thought they would have parted in peace, until I saw one put his rifle to the other's back and do a treacherous murder. The manly youth stood the shock for more than a minute before he fell. Then he was brought to his knees, and a desperate fight he made to the brake, like a wounded bear seeking a cover."

"Who was the perpetrator of this deed?"

"Yonder stands the man, and a disgrace it is to our race that he is of the blood and family of the dead."

"He lies! he lies!" shrieked Abiram. "I did no murder, I gave but blow for blow."

Ah! That exclamation, the admission implied in it, was a fatal confirmation of the testimony of the eye-witness. The conviction of the truth sunk into the heart of the most reluctant to believe it.

The voice of Ishmael was deep and even awful, as he answered—

"It is enough. Unbind the old man, and let him go. Boys, put the brother of your mother in his place."

Evidence personal or real.—Evidence is either personal, that is, afforded by a human being, a being belonging to the class of persons; or real, that is, afforded by a being not belonging to the class of persons, but to the class of things.

Personal evidence is either voluntary or involuntary.

Voluntary personal evidence is all such evidence as is afforded by means of language or discourse. This is the description of evidence commonly termed testimony.

Involuntary personal evidence is all such evidence as results from emotion exhibited not in consequence of any act of the will directed to that end, but frequently in spite of the will; in direct opposition to every exertion which the will can make. To this kind belong all the involuntary modifications of which the deportment, and all the involuntary changes of which the countenance is susceptible.

The subject of all personal and voluntary, and consequently of all testimonial evidence, is always either the very fact, the existence or non-existence of which is the principal matter of fact in question; or some fact which, though distinct from it, is considered as being evidentiary of it. Hence result two species of evidence, direct and circumstantial, to the distinction between which I shall direct your attention hereafter.

Testimonial evidence is either original or unoriginal: original, where the deposing witness states, that the matter of fact in question, at the time and place in question, fell under his own observation; unoriginal, when the witness speaks of some other person, and not himself, as the person to whose senses the matter of fact presented itself.

Things, in contradistinction to persons, may be the source of evidence; and all objects belonging to this class come under the denomination of real evidence. So extensive a source of evidence is this, that there is scarcely an object in nature which is not capable of becoming the subject of inquiry in a court of judicature. A person is killed or hurt! What are the objects capable of affording real evidence in relation to the event; the blood upon the ground; the instrument by which the injury was inflicted; the pistols, the sword, the club, the knife. Poison has been administered. A portion of some one of the different classes of poison is discovered in the house, in the chamber of the murdered person; in the possession of the suspected person. On examination of the body of the deceased, a portion of this very same poison is detected in the stomach or intestines. A house has been broken open, and a person murdered. There are found the pick-lock key, the crow, the chisel, the ladder; and even the very floor on which the person killed was standing, the chair on which he was sitting, the bed on which he was lying, bear witness to the kind of violence perpetrated.

ON THE
PRACTICAL INFERENCES
WHICH RESULT FROM
SOME LATE PHYSIOLOGICAL
INVESTIGATIONS
RESPECTING THE
LAWS OF THE VITAL FUNCTIONS
IN THE MORE PERFECT ANIMALS.

By A. P. W. PHILIP, M.D. F.R.S.L. & E.

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PART III.

To the Editor of the Medical Gazette.

SIR,

HAVING in my former papers treated of the physiological principles on which the nature of the diseases we are considering depends, the immediate causes of those diseases, namely, the state of body which immediately produces their characteristic symptoms, and the manner in which this cause operates in producing their various phenomena; I am now to point out their remote causes, the manner in which they operate, and the general principles of the treatment which counteracts their effects, and which, as might be expected, has been found by many degrees more successful, than that which is founded on a mistaken view of the nature of those diseases; thus affording the most important and gratifying proof of the soundness of the view which has been taken of them.

It appears, from what was said in the last of the foregoing papers, that a state of continued general debility or nervous irritation, which indicates the existence of a cause which has to a certain degree debilitated the organs of the leading power of the vital system, may be of various origin. It may arise, we have seen, from original disease of its organs; it may be a sympathetic affection, arising from the state of the central organs of the sensitive system; or it may be the effect of disease in some other vital organ, directly influencing the state of the central organs of the vital system.

The first of these causes is of rare occurrence, the second less so; but it is to the last cause that, in at least nineteen cases of twenty, the disease must be ascribed.

Thus the most common cases are those,

the general character of which is given in the latter part of my last communication, consisting of the affection of some vital organ influencing the central organs of the vital system, and a debilitated state of these organs causing a third affection, that of some other vital organ, which always proves both the most prominent and formidable disease, because, as appears from what has been said, the affection of the organ in which the disease originates must have two conditions: it must belong to an organ little disposed to change of structure, and the affections of which in general are not immediately productive of alarming consequences. Without the former condition it could not be of so chronic a nature as to induce the debility of the central organs; and without the latter it could not be disregarded or lightly treated by the medical attendant; or, as not unusually happens, so disregarded by the patient, that medical advice is either not obtained or not regularly attended to; for although the symptoms appear little formidable, in order to produce the effect we are considering, they must always be obstinate or of frequent recurrence; for this effect seldom takes place except from repeated and long-continued causes of irritation.

I am now to inquire in what part of the vital system, that is, in what organs, such an affection is most apt to arise.

We should, *a priori*, expect to find it most frequently occurring in those organs which are most extensive, and consequently most capable of a powerful impression on the central organs of the brain and spinal marrow, and most exposed to causes of irritation; and yet not the most immediately essential to life, and therefore their functions such that they may be disturbed to a considerable degree, and for a considerable length of time, without immediately endangering it.

The organs to which these observations most eminently apply are the alimentary canal; but it is very unusual for irritations of this canal alone to produce the serious effects which form the subject of these papers, because, when its affections are unaccompanied by other disease, they are seldom of sufficient constancy or recurrence to induce debility of the central organs of the vital system. But this, however rarely, may happen. I shall state, in a cursory way, the circumstances of such a case,

because it is very uncommon, and particularly illustrates much of what has been said.

One condition of such cases in particular is strikingly illustrated by it. The organ last affected must generally be one which is apt, from other causes, to be disposed to disease, because, as the debility of the central organs influences all vital organs, the chief effect falls on the weakest part; either that by the nature of our constitutions most exposed to disease, or one that has been rendered so.

The case I refer to was that of a lady who had for many years been exposed to the frequent recurrence of severe irritation of the bowels; yet this state of the bowels depended little on bilious derangement, as it is usually found to do. She did not labour under the symptoms always attending bilious complaints of long standing, and occasional mercurial doses never gave even the temporary relief they usually do in such cases, but only added to the irritable state of the bowels. Her attacks gradually became more frequent, till at length her strength was permanently impaired. In a short time after this she began to cough, which had never been the case at any former period of her life, although she was now between fifty and sixty years of age.

As she was past the time of life when a disposition to pulmonary consumption usually appears, and her family on neither side had been subject to this disease, the cough excited little alarm; but this, like her other symptoms, proved obstinate, and in a very short time after it had attracted serious attention, symptoms indicating diseased structure of the lungs appeared. Her debility was such that she became subject to sudden fits of syncope, and general anasarca supervened. She was now confined to bed, and died of one of the most rapid cases of pulmonary consumption I have witnessed.

The central organs of the vital system had been previously debilitated by the long-continued irritation of the alimentary canal, without any other morbid affection, although the more evident sympathies of this canal are, from its great sensibility, rather with the central organs of the sensitive than those of the vital system. The relations were surprised that a person at her time of life, who had never shown the least disposi-

tion to such a disease, should have been carried off by a more rapid attack of it than they had ever known in any other case.

Here the central organs of the vital system were injured both by the direct effect of the irritation, and sympathetically, by the effect produced on the central organs of the sensitive system by an affection of so extensive and highly sensible an organ, and of such continuance. By the previous disease, the system was in every way prepared for the result when the affection of the lungs supervened: the central organs of both systems had been strained, hence its rapid progress. Can any one doubt that if in this case the irritable state of bowels, which was supposed to be attended with no risk to life, had been relieved, the patient's life would have been saved? Here was a cause gradually and imperceptibly undermining the powers of life, which was thought both by the patient and those she consulted to be of too little consequence to be seriously attended to.

But although long-continued irritation of the bowels, from their great extent and sensibility, must be more or less powerful in slowly undermining the general vital powers, they are much less so than would otherwise be the case, because, although like other vital organs, their function is dependent on the vital organs of the brain and spinal marrow, their sympathy is stronger with the central organs of the sensitive than those of the vital system.

Of all our organs, the liver is the one which partakes most of the affections of the alimentary canal, and in a large proportion of cases supplies the source of many of its most powerful causes of irritation. I have, both in my Treatise on Indigestion and Gulstonian Lectures, entered at length into the proofs of the extensive influence of the liver, although an organ of little sensibility, on the sympathies of our frame; by which, more than any other cause, the consequences of its morbid states are regulated; a circumstance in a great degree depending on the immediate connexion which exists between this organ and the vital organs of the brain,—a connexion far more powerful than exists between any other part and those organs; nor is its influence less remarkable on the central organs of the sensitive system, although it is so ill

supplied with nerves of sensation, arising from the powerful sympathy which exists between the central organs of the two systems, and the direct influence of its affections on the alimentary canal.

Thus it becomes the most fruitful source of the class of diseases we are considering. Owing to its little sensibility, its slighter and more habitual affections are often overlooked, while from its function regulating that of the alimentary canal, their influence is spread over the most extensive and sensitive surface. When to these circumstances its powerful influence on the central organs of the vital system is added, we cannot be surprised at its effects, both in causing and regulating the course of disease.

It is a striking fact, and powerfully illustrates what I am now saying, that in sultry climates, where our sympathies are most, and for the security of health too active, almost all diseases, however they commence, terminate in disease of the liver; and even in temperate climates its general influence on all important diseases may be observed. We never see any serious disease in which its functions are not more or less deranged; but here its affections are less observed, and, if I may so speak, less monopolise disease, because its affections are not, as in sultry climates, disposed to run to deranged structure, and disease of structure is more powerful than that of function, in relieving all concurring diseases.

If these facts be such as here stated, and by the well informed and experienced physician they are too notorious to be questioned,—I say, if such be the facts, and we had understood the nature of the diseases which form the subject of these papers, we might have foretold that the liver must perform an important part in their production; and how amply would such a prediction have been confirmed by actual practice, did my limits admit of it, I could prove by reference to many diseases of the class we are considering. I shall select one of the most important, that of symptomatic pulmonary consumption; for the proneness of the lungs to change of structure often causes their disorganization to be the last in the chain of fatal causes. Many of the observations which I am about to make on this disease are applicable to others.

The circumstances which in pulmo-

nary consumption render the distinction between the original and symptomatic disease doubly important, are, that the former is often a fatal disease from the commencement—so little is the power of our remedies over the affections of the lungs; while in all cases, the latter can be arrested in its early stages, and generally in all stages previous to the structure of the lungs being actually diseased, which in most cases occurs at a later period after the commencement of the functional disease than where it is an idiopathic affection, and that for every case of the original disease in these kingdoms, we meet with many of the symptomatic.

I have said elsewhere, that at least three-fourths of the cases of this disease are of the latter description, but I can truly say that in my own practice, for one case of the idiopathic I have met with at least a dozen of the symptomatic phthisis. It also deserves attention that the latter is that in which the hereditary predisposition is by far the most powerful: I have never in a single instance known many members of the same family fall a sacrifice to the original disease; while I have seen almost whole families swept off by the symptomatic form: that form, although as certainly fatal as the other if the structure of the lungs has been allowed to become seriously diseased, but which, in its early stages, is always curable. A most gratifying proof of which I can adduce, because I have seen in many families, in which one after another had fallen a sacrifice to symptomatic phthisis, its ravages wholly and finally prevented, and that by means of easy application.

It is a striking fact, and shews how essentially different is the nature of these cases, that in the practice of more than thirty years I have never seen the two forms of the disease in the same family. If one of a family labour under either, in any other of the same family seized with it, it will always be found of the same form.

It will appear from what has been said, that it is the functional, not organic, affection of the liver, which lays the foundation of the symptomatic phthisis of which I speak. The organic affection of the liver is too serious a disease to be disregarded; and when, as often happens, it terminates in disordered

structure of the lungs, this disease is to the most careless observer evidently symptomatic.

This circumstance has tended to confirm the error which has prevailed, because, according to a law of our frame which I have had frequent occasion to refer to, the original disease, when only functional, is generally relieved by the secondary disease, even when it also is functional; but when the latter has become structural, it never fails to relieve, and very often wholly removes, the functional disease from which it sprung. From this cause many have been confirmed in their erroneous view of the case, by finding, on examination after death, that the liver bore no marks of disease.

We have a memorable instance of this error in the post-mortem examination of the late Dr. Currie, a man both of talents and correct observation, and who had long laboured under bilious complaints, and regarded all his complaints as originating in them. But those who made the post-mortem examination having reported the liver to be quite sound, but the structure of the lungs destroyed, it has been generally supposed that he had mistaken his own case; he could not have mistaken that he had been long distressed by bilious irritation; and the mistake imputed to him was probably the consequence of the fact just mentioned.

Unless the symptoms of disordered structure of the lungs are unequivocal in the cases we are considering, however in other respects alarming the symptoms, experience has taught me never to despair, unless all morbid distension of the liver having disappeared, it has retired into its proper position under the ribs; for I have in a few instances seen recovery by the means I am about to refer to, where, at first view, the case appeared to be hopeless. But the affection of the liver being completely removed, without having afforded relief to the pulmonary symptoms, I have, as stated in my Treatise on Indigestion, uniformly found a fatal symptom.

It is to be observed, that in such cases the feeling given to the hand by the distended liver is very different from that given by an enlarged liver, the former being much softer and more yielding, and never presenting the defined edge of the latter.

The degree of distention is best ascertained by examining the hepatic region in the erect position, and pressing with the same force on the corresponding part of the left side, and thus determining to what degree the pressure checks the breathing on each side; for in the healthy subject, as I have ascertained both alone and with other physicians, the feeling of the patient is precisely the same on both sides; not even the edge of the liver, in a healthy state, coming below the ribs, and the hand pressing only on the soft bowels on both sides.

The function of many vital organs, particularly those of dull feeling, may be slightly disordered without causing much inconvenience. The vessels of the liver, for example, may be so weakened as to allow of some accumulation of bile taking place in it, so as to cause its edge to descend below the ribs, while the individual still retains the feelings of health; and if the trial be made, such an affection will be found in many, who consider themselves well; this deviation, and even tenderness on pressure in the regions of the pylorus and liver, in a country where bilious complaints so much prevail as in these kingdoms, are by no means uncommon, who cannot be said to be out of health.

But in judging of the tendency in bilious complaints to produce serious disease, the following observation should be constantly kept in view: the more distressing and formidable effects never arise directly from the debilitated state of, and consequent accumulation of bile in, the liver. It is not at all uncommon to find, in two individuals whose livers are equally inactive, and consequently distended, the one declaring that he is in all respects in good health, while the other tells you he is in a constant state of misery from his disease; nay, the liver of the former shall even be more debilitated and distended than that of the latter.

The difference in the two cases consists in the nerves of the one having resisted the cause of irritation, while those of the other are fretted by it; the direct cause of all the more distressing symptoms of bilious complaints. I have had occasion to observe, that in some, the vital organs of the brain and spinal marrow resist causes of nervous irritation more powerfully than in others; and that in some,

after the fret of nerves and general debility becomes permanent, indicating some affection of the central organs, the other vital organs resist the diminished supply of the nervous influence more powerfully than in others: so it happens in the present instance, that in some the nerves resist the impression, made on them by the debilitated state of the organ originally affected, more than in others.

Now until this state of nerves at least temporarily takes place, however much the liver may be loaded, the general health is but little impaired, and the individual will tell you that he is in good health. The slight and temporary effects depending directly on the complaint, he disregards; he is troubled occasionally with some degree of flatulence and acidity of stomach, causing occasional heartburn and some languor of bowels. By easy means these symptoms are relieved, and it is only as the fret of nerves, or a sense of debility caused by the recurrence of the bilious symptoms comes on, and tends to become permanent, that he considers himself an invalid. When, we have seen, this fret of nerves or sense of debility becomes permanent, the central organs of the vital system begin to suffer, and then any of those vital organs, the functions of which depend on the influence supplied by them, and which is from other causes disposed to disease, will feel most the cause which acts equally on all organs of the same description, namely, some tendency to a diminished supply of the influence necessary to their functions; and it is only as this happens that the sufferer comes into danger. Unless these positions be kept in view, neither the nature nor proper treatment of the cases we are considering can be understood.

What are called bilious complaints, while they retain the form expressed by this term, being unattended with danger, and our not being aware that many of our most formidable diseases derive their origin from their continuance,—for did the limits of this paper admit, it could easily be shewn that other diseases of the chest, and many of the most fatal diseases of the head, often derive their origin from the same source,—I say that in consequence of the apparent unimportance of bilious complaints, according to the usual sense of the term, the attention of physicians has

never been directed to them with the care that their effects, when protracted, demand.

Believing that the functions of the nervous system only contribute to those of the sensorial power, they could not be aware that its affections are often the parent of the most formidable diseases, and that, as in the case of the species of pulmonary consumption we have been considering, their removal, which, by the proper treatment, may always be effected, would often be the means of preventing a fatal termination.

It is true that we often see such affections continue for a long life, without producing any of their fatal consequences, owing to the circumstances above stated as depending on peculiarity of constitution; but as it is impossible to foresee what the course of the symptoms will be in any particular individual, the only safe plan is to employ every means in our power in radically removing what are called nervous complaints as soon as our patient applies to us; which, for the most part, they seldom do until the fret of nerve or general debility has become permanent; and then some degree of debility has always been established in the vital organs of the brain and spinal marrow.

Under which circumstances, when the disease makes its next step—that of influencing the function of some other vital organ, by which, according to a law of our frame, to which I have frequently had occasion to refer, the whole force of the disease being diverted from all other organs, is concentrated in one—we are sure to have a disease of difficult treatment, if not of a fatal termination; for we never find even a functional disease, if it arise from previous debility of the central organs of life, yield in the way that original disease of the same organ is found to do; and if this debility has been allowed to remain until it has produced any degree of structural disease in the organ last affected, in which the continuance of functional disease will generally terminate, we shall not, in one case in twenty, prevent a fatal termination. Absolute safety can in no way be obtained but by a removal of the disease in those stages in which we can with almost certainty effect our object.

I shall here, without entering on the particulars of the treatment most successful in confirmed bilious complaints,

for which I beg to refer the reader to the seventh edition of my Treatise on Indigestion, only endeavour to point out the causes which have hitherto rendered the treatment in them so inefficacious; for the usual plan, after a certain degree of relief is afforded, is to tell our patient to attend to the rules of diet we lay down; regulate the bowels by the means we have pointed out, think as little as possible of his complaints, and have recourse to a dose of calomel when he feels more indisposed than usual; and so he is dismissed, an invalid for life. I have said, however, that under such circumstances a cure may almost always be effected. I shall here point out what, as far as I can judge, have been the causes which have prevented the permanent cure, in all cases of long standing, and the principles on which repeated experience has assured me such a cure can almost always be effected.

It appeared that the cause of our failure was, trusting alone to doses of mercury, the only medicine on which we can rely in affections of the liver of however slight a nature, of such amount as could only be given at considerable intervals, that of two or three days, without rendering the effects of the medicine as pernicious as the disease, or more so, in consequence of which the bile always, more or less accumulating, in the interval, in the debilitated liver, and the effect of our doses being thus only temporary, the permanent cure was impossible. Another circumstance which, of itself, would have rendered a permanent cure, in many cases of long standing, impossible, even under the most efficient administration of mercury, is, that we overlooked a consequence of the long continuance of the bilious complaint and the impaired function of the stomach, which generally more or less attends it, the continuance of the irritations of indigestion producing an inflammatory state of the parts most exposed to them, which I found from post-mortem examinations, first takes place in the pylorus, which in those who had long suffered from bilious complaints, was often found as red as if it had been actively inflamed, and spreads more or less to neighbouring parts, and particularly to the left edge of the liver.

This causes a tenderness on pressure, to be felt in the region of those parts, and prevents the proper effects of our

medicines. In the least inflammatory habits this tendency does not always occur; but I can say, from extensive experience, that where it does, which is in a large majority of cases, it obviates the effect of our remedies, and renders the removal of the disease impossible.

With respect to the first of these difficulties, which have rendered our practice, in what is called bilious complaints, if of any standing, so inefficient, it occurred to me, above twenty years ago, that as we cannot give the usual mercurial doses, except at long intervals, during which, in such cases, their effects are in a great degree obviated, some means must be found to maintain these effects during them before we can make effectual progress in removing the disease.

None that I tried had an effect at all to be depended on, till I had recourse to minute doses of mercury, given at intervals of eight hours. The dose I found to answer the purpose was either the twentieth part of a grain of calomel, or half a grain of blue pill, which is equal or nearly equal in power to it. One I have found to suit best one constitution, and the other, another. The minute dose of calomel is most aperient, but on the whole, I have thought that of the blue pill the best alterative.

There are many circumstances to be observed in their employment, which have gradually shown themselves, and many of which are stated in my Treatise on minute doses of mercury. Their good effects, however, are not to be wholly ascribed to their keeping up the effect of the larger dose, which must always be continued under their use, for they have little effect in enabling the liver to discharge the accumulated bile; but in proportion as this becomes less, the larger dose may be employed at longer intervals.

It is a law of our frame, that, although in the sensitive system, every stimulant, however slight, is followed by corresponding exhaustion in the vital system this is only the case with stimulants which exceed a certain power*. Thus it is, that in this system whatever produces the stimulant effect within that range, acts as a tonic. I have found that such is the effect of the minute mercurial dose on the liver.

* Philosophical Transactions for 1596.

While the excitement of the larger dose is followed by a more languid action of the liver, the minute dose supplying a constant and gentle stimulus to the liver within the limit which produces corresponding depression, acts as a tonic, and the organ gradually and uniformly recovers its power under the use of the minute dose, and the whole system, the debility of which depends on that of the liver, experiences a corresponding effect. And I can say, that I am unable to recall one case where there was no disease but the bilious complaint, however long it had lasted, and the patient did not become tired of the means,—for in long-protracted cases the recovery is necessarily slow—in which he was not restored to permanent health; and I can at this moment point out cases which had lasted for twenty years, in which the patient for many years past has enjoyed uninterrupted health.

With respect to the tenderness on pressure of the parts chiefly concerned in the disease, the only means of its removal is blood-letting from the parts in their immediate neighbourhood. The patient is never sensible of this tenderness, as it occasions no uneasiness, except on pressure in its seat; it is therefore always overlooked if the physician does not detect it by an examination of the parts affected; and frequently-repeated local blood-lettings are often required for its removal.

This tenderness is of an inflammatory nature; and although it never assumes the form of active inflammation, it is sufficient to bind up the surfaces, while the object of the treatment is to relax them, and thus to promote a free flow of the secretions; and as our means must always be of the gentlest nature, (for large doses of mercury are capable of causing, and cannot, therefore remove the disease), their effects are easily counteracted. Along with the foregoing means some stomaehic medicine is proper, for grateful as the mercurial is to the liver, to the stomach and bowels it is more or less offensive; but tonics can seldom be employed without injury, and never till all inflammatory tendency is subdued.

Such, with the means of relieving occasional symptoms, which are very various, depending on accidental causes and peculiarity of constitution, is the general outline of the treatment which I have found successful in effectually

removing old bilious complaints, which, although never themselves dangerous in this country, where the liver rarely runs to disorganization, except in drunkards, which has caused too little attention to be paid to them by our profession, are, in the manner above explained, the parent of many of our most important diseases; so that I believe, from very extensive experience, that I speak within bounds when I say, that however slow their progress, they are eventually, in this country, where they may be regarded as the endemic disease, the cause of more deaths than any other, or perhaps of any half dozen of all the diseases we are subject to; for although many constitutions are strong enough to resist their serious effects even for a long life, in a large proportion of cases they run the course which has been described.

It is hardly necessary to add, after what has been said, that their ultimate effects are very various, as they may terminate in a fatal disease of the head, chest, or abdomen, according to the tendencies of the particular constitution, the period of life, and the circumstances in which the individual happens to be placed.

I shall only again remind the reader that, in all serious diseases, we should carefully determine from an examination of the state of the digestive organs, and the history of the case, how far the irritations of these organs have had a share in producing the attack his patient labours under; because if this has been the case, and the disease of those organs, even although in a mitigated form, remains, the removal of the more prominent disease is impossible, unless that in which it has had its origin be detected and removed.

Thus I have found in many acute, and still more in chronic cases, that on examination of the region of the three great digestive organs, the stomach, liver, and duodenum—for when the regular flow of healthy bile languishes, the action of the duodenum becomes imperfect, and the food accumulating in it causes more or less morbid fulness in its region, as well as in that of the liver,—under such circumstances, I say, I have found this examination of as much consequence as that of the pulse, and often more so. For many years I have have never failed to have recourse to it; for even where such affections have no concern in the cause of the disease, they

often, from the extensive sympathies of the liver, supervene in it, and always influence its course, and ought if present to influence the treatment.

If there be any truth in the preceding observations, and they are equally founded in the general laws of our constitution, and extensive experience of actual practice, how erroneous must that treatment be by which all cases of the same disease of the same organ, are often treated in the same way, whether arising from a cause immediately influencing the organ itself, or one which has made its first impression on another part of the system, without correcting the effects of which it is impossible to remove the more prominent disease; for a disease will, *à fortiori*, be supported by the continuance of the cause which produced it. The correctness of this observation is strikingly illustrated by the disease which, on account of its eminence and frequency, I have chosen as an example; even to the degree, I may say, of almost certain safety, instead of quite certain death.

The practical importance of the subject of the preceding part of the present paper has caused it to be prolonged so much beyond what I had intended, that I have left little room for the remaining part, namely, the inferences which necessarily arise from its having been ascertained that the agent employed by the vital organs of the brain and spinal marrow, in all their functions, is voltaic electricity*, for we have seen that it is capable of all those functions; in which, therefore, I shall only attempt the most general observations, referring to the sources of more particular information.

It is reasonable to suppose, that its having been ascertained that a power which we are at all times capable of commanding, and that to any extent that can be required, having been ascertained to be capable of all the functions of the leading power of the vital system, must, if judiciously employed, often be of eminent service in the practical department of our profession; and such has been the result†.

Several circumstances, however, have

hitherto tended to prevent its beneficial effects having been so extensive as it may be hoped they will soon be found to be. New modes of practice, in a profession open to so many proposals which prove of little or no value, the care and anxiety, and in many instances the trouble attending the first trial of means with which we are not familiar, especially if (as in the present case) they require an apparatus which few members of our profession are in the habit of employing, and, above all, if they be such as may do harm instead of good, if not employed judiciously, are, and for the first and last of these reasons, ought to be, cautiously adopted.

I shall close this series of papers by pointing out the chief precautions requisite in the employment of voltaic electricity, which have already been laid before the public in the works just referred to, but have certainly not obtained the attention which is indispensable, even of those by whom this remedy has been most extensively employed,—a circumstance which has greatly tended to confine its employment.

As we might, *à priori*, have inferred, although we can easily direct voltaic electricity to any part in which we know, from the phenomena of disease compared with the results of the experiments referred to in these papers, there is a deficiency of nervous influence, and we always find, from the effects, that deficiency supplied by it; it is not to be supposed that we can supply it precisely in the same way as nature supplies it; or supply it in the exact proportion required, and confine it exactly to those points to which nature confines it.

We find, in consequence, that in addition to the effects we wish to produce, if its application is not cautiously arranged, we obtain at the same time effects that we by no means wish.

All such effects may be ascribed to its tendency to produce an inflammatory state of the parts to which it is applied. I have never in one instance seen this inflammatory state, produced by voltaic electricity, approach to active inflammation; because its beneficial effects may be obtained by the employment of very moderate powers, and I have laid down rules of easy application for confining it within those powers in the cure of diseases, so that there is no risk

* Philosophical Transactions for 1836, and the publications there referred to.

† See my paper in the Philosophical Transactions for 1817, and Gulstonian Lectures for 1835; also Mr. Earle's account of trials made by him in St. Bartholomew's Hospital, in a letter published in the last part of my Treatise on Indigestion.

to life in its employment ; but if it at all excites any degree of the inflammatory tendency, particularly in cases where the disease itself has more or less of this tendency, as is the case in many of the diseases in which its assistance is most called for, we soon find that, with all the precaution we can employ, the use of it cannot, without injury, be continued.

To recur to the case that I have throughout this paper taken as an example. When the disease is in its first stage, that of the bilious complaint, it must not be employed in those cases where any tenderness is perceived on pressure in the region of the pylorus, or liver, or on any part of the alimentary canal, until by local blood-letting this symptom has been removed ; and where it has not existed, care must be taken to examine the regions in question, that we may be assured that it is not produced by the remedy we are employing.

I have found that this precaution is chiefly necessary on first employing it, because when it has succeeded in restoring the due action of the parts, and thus in a great degree removed the irritations attending the disease, it is much less inclined to produce this effect ; which, when it does occur, is always more or less the joint operation of the disease and the remedy.

When the tenderness on pressure occurs under the use of the latter, it must be discontinued, and the tenderness removed by the same means as when it arises from the disease alone, before it is again employed.

With respect to the power of the voltaic electricity employed, I have generally directed that it at first should be no more than causes the patient just to feel it at either of the parts to which the wires are applied, and that it should not be applied for more than ten minutes once in the twenty-four hours ; or, if there is much doubt respecting its effects, in forty-eight hours, its power if necessary being gradually increased, provided no objectionable effects shew themselves. For the cases in which it is most useful, and the particulars of its effects, both favourable and unfavourable, I must refer to the publications above mentioned.

There is one of its effects which was very unexpected, and which, as it has

not been fully stated in any of those publications, I shall here state more fully. It is evident that it may assist the organ to which it is applied in three ways, either by simply supplying the nervous influence where it is defective, in which case its effects are necessarily temporary ; or by exciting the brain or spinal marrow to better action, as far as relates to the diseased part ; or lastly, by restoring to the ganglionic nerves of the part more vigour in conveying their influence to the part in question. Now I have never seen its effects merely temporary, although they are much more so in some cases than in others ; therefore its action must always be more or less by one of the last two means ; but in some cases so small a power has sometimes produced so great an effect, that it can hardly be explained except by supposing that its operation was merely that of giving vigour to the nerves which are its channel of conveyance.

I shall close this paper with a short statement of the most remarkable case of this kind which has ever occurred to me. One of the diseases which it is most powerful in relieving, and for which, as far as I know, we have no other effectual means, is what I have termed in the first publication above referred to, habitual asthma. A lady of about 30 years of age applied to me for the removal of a case of this kind, who had never, even when she was a child, been able to run about with her companions, on account of shortness of breath, as she termed it, which, as she advanced in life, increased so that she could not, when I saw her, even walk up hill without much suffering. I advised no medicine for her, but directed the moderate use of voltaic electricity, which was applied in my presence. In a few minutes she said she breathed with more ease than she remembered ever to have done ; but the most unlooked-for part of the result was, that she never required a second application of the remedy, her breath remaining as free as in the most perfect health, so that she could not only walk, but run up hill, with as much ease as other people.

I have seen some similar instances, in which a few applications of voltaic electricity produced permanent relief, but none so striking. In such cases

I think we must suppose that some obstructing cause in the ganglionic system, as far as it is connected with the organ which is the seat of the disease, had proved sufficient to obstruct its function, though not to affect its structure; for cases of this kind are unattended with danger if unattended with cough. Even a ligature thrown round the eighth pair of nerves materially influences the function of organs supplied by them. If such be the case, the obstruction is of such a nature as may sometimes be relieved by a few slight applications of galvanic electricity; and in almost all cases of this species of asthma*, more or less relief appears to be uniformly its effect, the relief being more or less perfect, and more or less permanent, and not unfrequently final. In the paper just referred to, in the *Philosophical Transactions* for 1817, the result of its employment in about a hundred such cases is given. Most of these were treated in the course of several years, in a public establishment, the County of Worcester Hospital, where the disease among the sedentary manufacturers is of very frequent occurrence, and in the presence of many observers.

I am, sir,

Your obedient servant,

A. P. W. PHILIP.

Cavendish-Square,
Nov. 19, 1837.

OBSERVATIONS ON THE PULSES OF INFANTS.

To the Editor of the Medical Gazette.

SIR,

ON referring to page 190 of the *MEDICAL GAZETTE* for October 28, 1837, I find the following:—

“At the conclusion of Dr. Guy’s paper, Mr. Gorham stated the results of some experiments made by himself on the pulses of infants. These remarks, though *irrelevant* to the paper, were valuable,” &c. &c. By inserting the following communication in the pages of your valuable periodical, you will oblige me, as it will be then rendered evident whether my observations were really *irrelevant* or not, and I shall be enabled to say a few words on a

* Voltaic electricity has little if any effect in removing other forms of asthma which are spasmodic affections, unless they are complicated with habitual asthma, as I have defined it in the publications referred to, which sometimes happens in protracted cases.

subject which has occupied my attention for some years past, although I had not intended to have made any thing public for many to come.—I am, sir,

Your most obedient servant,

J. GORHAM, M.R.C.S.L.

225, High-Street, Borough.
Nov. 1837.

From pretty active obstetric practice during the last five years, it must be imagined that many infants and young children have come before my notice; and for the whole of that time I have been in the habit of collecting divers facts and phenomena which presented themselves. This was done not with a single eye; and the results, some of which are exceedingly interesting, pertain to physiology, and also to the bettering of a prognosis which is proverbially deficient, viz. that in diseases of children.

On ascertaining that a paper was to be read by Dr. Guy, at the Physical Society of Guy’s Hospital, of which I am a member, it occurred to me, that as it purported to treat expressly on the “effect of posture on the pulse,” I might, from the collection I had formed, put together something which should go to illustrate the opinions promulgated in the paper, tending, perhaps, to improve some, or probably to disprove others. After looking through about one half of my books, I was enabled to obtain the results of 150 experiments, made at different times, under varied circumstances and conditions of the infantile state. They were collected with the greatest care, as it was done for my own personal good, in order to assist me in forming a better diagnosis in disease; neither were they obtained in a hurry (five years having elapsed since they were begun), in order to get them finished at any particular period; nor would they have appeared now in such an unfinished state, had it not seemed necessary.

The naked facts of themselves, then, may be relied on: and as I have not time at present to handle them in a manner which I am sure they deserve, in consequence of being engaged in producing a more complete essay on the subject, I trust that a stimulus may be given to others, by their imperfections, to take up a subject which has been very much neglected in this country.

I inclose the tables which were made use of the other evening at Guy’s Hospital.

Y

EXPERIMENTS ON THE PULSES OF 150 INFANTS AND CHILDREN.

TABLE I.—*From Birth to the Age of Twenty-four Hours.*

No.	Age in Hours.	No. of Pulse.	Sex.	Accidental Remarks.
1	12	112	Male.	Length, 19 inches.
2	8	145		
3	7	120		
4	16	128	Male.	
5	24	120	Male.	
6	12	104		
7	12	136		
8	9	116	Female.	Twin—horizontal—waking from sleep.
9	9	136		Twin—horizontal.
10	16	132		
11	24	120		
12	24	100		
13	24	120		
14	24	160		
15	25	104		
16	5 minutes.	120		Counted from funis—horizontal.

Experiments on these sixteen infants, from birth to the age of twenty-four hours, led me to the following conclusions:—

1. The mean number of the pulse of the new-born infant is 123 and a fraction.
2. The maximum number in the above experiments was 160, and the minimum 100.
3. The mean number of three, who were stated in the table as being in the horizontal posture (for I believe all were horizontal, although I cannot aver this for a fact), was 124.
4. The average number of the pulse of three male infants, taken from the above experiments, was 120.
5. The pulse of one twin female was 116.

My first conclusion is derived from facts, without any allusion to the posture, the state of motion or rest, or muscular exertion. Neither do I know of any author who has noticed these particulars as regards the infantile state, and as connected with the pulse. My third conclusion is derived from three experiments: a scanty number, truly; yet more, indeed, than has been made by any one else, according to my reading. It will be seen at once that it relates to

posture, and also to the pulse: it bears, in fact, immediately on the subject of the *effect of posture on the pulse*, and is irrelevant to a paper read actually on the very subject! (?).

From the table it will also be learned, that the pulse of the infant, at this early age, is slow; and it does, indeed, acquire frequency up to a certain period, which will be mentioned and better understood when the following tables have been considered.

Also, that although all the observations that have been hitherto made on the pulse of the infants are not without their value, yet that they are rendered imperfect and susceptible of improvement, by taking into consideration the circumstances of posture, and laws emanating therefrom, mentioned in the extremely valuable paper of Dr. Guy.

As regards the postures, it is impossible, indeed, to obtain some on which we may wish to experiment in these young creatures. Is it likely, for instance, to find little Sampsons at birth, who should be able to retain themselves in the erect position by their own muscular effort, unaided by any external support for one moment? Now as “the effect of posture forms merely a particular case of the general law—viz.

that muscular contraction, whether employed to change the position or to maintain it in the same position, accelerates the pulse," it will be evident that, although experiments can be made, and easily too, in this particular posture in the adult, yet that in no instance have they ever been made, nor, indeed, can be, on the infant a day old: and the same observation will apply to the sitting posture.

It should be expected then, according to a law which emanates from that of Dr. Guy, that the pulse of the infant should be slower *now* than afterwards,

in consequence of the relaxed and usual horizontal position of their bodies; and this is actually the case from my experiments. Dr. Guy states it as matter of fact, that "the effect produced by change of posture on the pulse is in both sexes and *in all ages* considerable." The Doctor certainly could not have made this statement, "including all ages," from actual experiment, inasmuch as it is a physical impossibility to place an infant in the erect position, under the proviso enacted by Dr. Guy—that muscular action shall sustain and keep it there.

TABLE II.—*From One Day to One Week Old.*

No.	Age.	No. of Pulse.	Sex.	Accidental Remarks.
1	40 hours	120	female.	
2	78 "	120		
3	30 "	136		
4	4 days	120	male.	
5	6 "	96—112		Asleep.
6	4 "	148	female.	Taken from heart.
7	2 "	112		
8	36 hours	136		
9	7 days	128	male.	Stout.
10	6 "	132	male.	
11	not a week	152	male.	Taken from heart.
12	do.	112		
13	do.	130	female.	
14	do.	132	male.	
15	do.	108	male.	
16	do.	140	male.	Taken from heart.
17	do.	160	male.	
18	39 hours.	128		
19	2 days.	112		Asleep.
20	6 days.	152	female.	
21	47 hours.	120	male.	
22	5 days.	120		
23	6 "	136		
24	3 "	128		
25	7 "	112		
26	6 "	152		
27	46 hours.	110		
28	5 days.	126	male.	
29	3 "	124	female.	
30	under a week.	120	female.	
31	do.	120	male.	
32	do.	136	male.	
33	do.	130	male.	
34	do.	108	male.	
35	do.	128	female.	
36	do.	120	male.	
37	do.	136	male.	
38	do.	140	male.	
39	do.	128	female.	
40	do.	146	female.	
41	do.	132	female.	
42	2 days.	120	female.	Horizontal ; 22 inches long.

We do not, indeed, deny that the erect position does not cause an acceleration of the pulse in the new-born infant; but we assert, that if such be the case, then another cause must be assigned for this frequency besides muscular contraction.

These experiments on 42 infants, from one day to one week old, led me to the following conclusions:

1. The mean pulse of the infant between the age of 1 day and 1 week is 128.

2. The maximum in the above experiments is 160, the minimum 96.

3. The pulse of one female in the horizontal posture (although most of them were probably in the horizontal posture, yet it is not stated in the table, and must not, therefore, be given as a fact) was 120.

4. The mean number taken from eleven females is 131.71.

5. The mean number taken from nineteen males is 122. From four and

TABLE III.—*From One Week to One Month Old.*

No.	Age in Days.	No. of Pulse.	Sex.	Accidental Remarks.
1	11	108	Female.	Twin—position horizontal.
2	11	108		Twin—position horizontal.
3	18	176		Sucking.
4	9	146		
5	15	108	Male.	
6	12	148	Female.	Counted from heart.
7	9	140		
8	28	120		
9	11	152	Male.	
10	15	128		Dozing—horizontal.
11	27	152	Female.	Counted from heart.
12	10	156	Male.	
13	9	108	Male.	
14	10	116	Female.	
15	14	132-120	Female.	
16	21	144		
17	8	128		
18	10	168		
19	14	144	Female.	
20	11	128		
21	9	120		
22	9	156		
23	21	140		
24	15	104		Asleep—horizontal.
25	10	120		Asleep—horizontal.
26	28	140		Asleep—horizontal.
27	20	140		Sucking.
28	12	148		
29	18	152		Dozing—horizontal.
30	8	128	Male.	
31	19	140	Female.	Sleepy—horizontal; length 22 inches.

five it results that the pulse of females is much more frequent than that of males.

6. The average of three experiments on children asleep, and in the horizontal posture, is 108.

7. The pulse is quicker during the first week than during the first day.

8. The pulse is slower during the first week than it is found to be afterwards.

Infants at this age also cannot stand or sit upright, unsupported.

From One Day to One Month Old.

From the second and third tables, which together include experiments on 73 infants, it results—that the pulse in the infant from one day to one month old, is 131.

Remarks on Table III.

These experiments led to the following conclusions:

1. The mean number of the pulse in the infant from one week to one month old, is 135.45.

2. The maximum in the above table is 176, and the minimum 104.

3. The mean, from experiments on eight in the *horizontal* position, is 125: the mean of two of them which were asleep being 122, and of two which were twins, and not asleep, 108.

4. The mean, from experiments on six females, is 141.67.

5. The mean, from experiments on five females, is 130. From five and six it results that the pulse is quicker in females than in males.

TABLE IV.—*From One to Five Months Old.*

No.	Age, in Days.	No. of Pulse.	Sex.	Accidental Remarks.
1	70	160		Asleep.
2	30	104		Asleep.
3	42	136		
4	148	112		No teeth—lower gums rounding.
5	30	152		
6	126	128		Sitting.
7	39	130		
8	42	140		
9	49	140		
10	98	156		
11	35	160		Asleep.
12	112	156		Sitting—stout— $22\frac{5}{8}$ inches long.
13	49	136		Asleep—horizontal—premature.
		160		Awake—horizontal.
		160		Sucking—semirecumbent.
		176		Sucking—semirecumbent.
14	126	116		Twenty-four inches in length.
15	84	136		Sucking—semirecumbent— $22\frac{1}{2}$ inches long.

These experiments allowed me to draw the following conclusions:—

1. The mean pulse of the infant from one to five months old is 148.85.

2. The maximum number in the above experiments is 176, and the minimum 104.

3. The mean of five, which were in the *horizontal* posture, is 144; and of these,

four were asleep, the mean number of the pulse being 140.

4. The mean of five, in the semi-recumbent position, is 151.20; and of these, three were sucking.

5. The pulse is much more frequent after the first month than before it; and this frequency increases up to a certain period.

TABLE V.—*From Five Months to Two Years Old.*

No.	Age, in Months.	No. of Pulse.	Sex.	Accidental Remarks.
1	24	100-118		
2	5½	144		Asleep—horizontal—9 A.M.
3	12	120		Eight teeth.
4	14	136		Five teeth.
5	14	120		Asleep—horizontal.
6	13	120		
7	19	130		
8	21	136		Sitting.
9	14	124		Standing.
10	15	136		Sitting.
11	16	108		Horizontal—asleep.
12	13	133		Standing—30 inches long.
13	16	112		Asleep—two teeth only—horizontal.
14	8½	148		Asleep—horizontal.
15	6	128		Standing.
16	7½	148		Sitting—seven teeth—24 inches long.
17	12	132		Sitting—two teeth—25½ inches long.
18	11	158		Sitting—two teeth—25½ inches long.
19	6	128		Semirecumbent.

TABLE VI.—*From Two to Four Years Old.*

No.	Age in Months.	No. of Pulse.	Sex.	Accidental Remarks.
1	34	96		Asleep—horizontal.
2	39	124		Standing.
3	35	92		Asleep—horizontal.
4	36	108		
5	47	104		Asleep—horizontal.
6	40	116		Asleep—horizontal.
7	30	124		Standing—32 inches long.
8	36	108		
9	36	120		
10	48	120		
11	29	120		
12	36	108		Standing.
13	48	116		Standing.
14	36	120		Sitting.

Remarks on Table V.

These experiments on nineteen children led to the following results:—

1. The mean pulse of the child, from the fifth month to the second year, is 130.

2. The maximum from these experiments is 158, and the minimum 100.

3. The mean of five experiments, in which the children were in the horizontal position and fast asleep, is 126.40.

4. The mean of five experiments, in which the children were sitting, is 142.

5. The mean of three experiments, in which the children were standing, is 128.33.

6. The pulse at this age, which embraces the teething period, is quicker than at birth.

Remarks on Table VI.

From these experiments on fourteen children I draw the following conclusions:—

1. The mean pulse in the child from two to four years old, is 112.56.

2. The maximum number in the experiments is 124, and the minimum 92.

3. The mean of four, who were in the horizontal position and asleep, is 102.

4. The mean of four who were standing, is 118.

5. The number of one who was sitting, is 120.

6. The pulse at this age, which has passed the teething period, is much slower; and from this time it gradually diminishes in frequency to the end of life.

TABLE VII.—*From Four to Ten Years.*

No.	Age in Years.	No. of Pulse.	Sex.	Accidental Remarks.
1	7	88		Horizontal.
2	5	100		Horizontal.
3	4½	100		Standing.
4	5	100		Sitting.
5	5½	100		Horizontal—asleep.
6	7	128		Horizontal—now awaking.
7	10	104		Horizontal.
8	5	120		Standing.
9	4½	133		Standing.
10	5	124		Sitting.
11	7	{ 92		Standing.
		{ 92		Sitting.
12	9	100		Standing.
13	5	{ 116		Standing.
		{ 104		Sitting.

These experiments on thirteen children lead me to infer, that—

1. The mean pulse in the child between the fourth and tenth year, is 107.63.

2. The maximum in the above table is 133, and the minimum 88.

3. The mean of five experiments on those in the horizontal position, is 104.

4. The mean of six experiments on those who were standing, is 110.17.

5. The mean of four experiments on those who were sitting, is 107.50.

6. The pulse is slow in the horizontal posture. It acquires frequency if this be changed to a sitting, and becomes still more frequent in the erect position.

TABLE VIII.—*Effect of Posture on the Pulse.*

No. of Exper.	Posture.	Age.	Mean Pulse.
3	Horizontal.	Under a day.	124
1	Horizontal.	From one day to one week.	120
8	Horizontal.	One week to one month.	122
5	Horizontal.	One to five months.	144
5	Semi-recumbent.	Do.	151·20
5	Horizontal.	Five months to two years.	126·40
5	Sitting.	Do.	142
3	Standing.	Do.	123·33
4	Horizontal.	Two to four years.	102
4	Standing.	Do.	118
1	Sitting.	Do.	120
5	Horizontal.	Four to ten years.	104
6	Standing.	Do.	110·17
5	Sitting.	Do.	107·50
60 Experiments relating to posture as connected with the pulse in infancy and childhood.			

If the foregoing tables, and particularly the last, which is only, indeed, a recapitulation from parts of them, relate to the effect of posture on the pulse, then it were well that those gentlemen to whom it is entrusted to make public the discussions which emanate from the Physical Society of Guy's Hospital,

should look before they leap. If, on the other hand, they do not relate (?) to the effect of posture on the pulse, still they are valuable, and serve in a small degree to fill up a deficiency which always exists in works on physiology—viz. original facts and observations on the infantile condition.

REMARKS
ON
THE NATURE AND TREATMENT
OF THE
DIFFERENT FORMS OF PORRIGO.
By WALTER DICK, M.D.

THERE are some writers, and amongst them the Baron Alibert, who wish to have the names of cutaneous diseases descriptive of one or more of the prominent characters or symptoms of these diseases; in other words, to have the sign significant of the thing signified.

But the diseases of the skin are so numerous, and many of them differ so little from one another, that we cannot anticipate much success for such a proposal. Here, as in other departments of science, we are inclined to think that attempts at descriptive terminology will generally prove unsuccessful, and tend perhaps to retard, rather than to advance the progress of knowledge. In dermatology what is especially necessary is uniformity in the use of terms; no matter whether these terms be arbitrary, or not descriptive of any prominent characters of the diseases to which they are given.

The indeterminate manner in which the names of cutaneous diseases have been used, has unquestionably tended greatly to impede the progress of cutaneous pathology, and hence we ought at present to be anxious to fix the acceptance of terms already in use—to establish, by careful observation and faithful description, the diseases to which each of these terms ought to be restricted, rather than to be coining new terms. By and by, as our knowledge of skin diseases increases, it will perhaps be requisite to reform and simplify the cutaneous nomenclature, and we hope the time is not very distant when we will be enabled to construct a nomenclature of these diseases, taking from their seat, or the organs in which they originate, generic terms, and particularising the subordinate varieties by appropriate epithets. But till once we are enabled thus effectually to reform the nomenclature and arrangement of skin diseases, let us adhere to the terms hitherto in use, and labour to be unanimous as to the number, external characters, and seat of the morbid appearances of the cutaneous tissue.

Porrigo and tinea capitis are terms used synonymously by writers, but the acceptance of either is not yet very definite, nor is their derivation calculated to lead us to bestow either term on any particular group of eruptions. The term tinea came into use some time before the revival of letters, and was bestowed by writers on several eruptions of the scalp bearing more or less resemblance to one another. Lorry thinks it is a corruption of the Arabic word *alvathim*, by which Avicenna designated the humour which he supposed produced porriginous affections. “A quo nomine (alvathim) barbari ut videtur thim et thineum et tineam fecerunt*.” Another, and I think a more plausible opinion, respecting the origin of the term tinea, is that of Mercurialis, who supposed that it was bestowed on certain eruptions of the scalp, from some fancied resemblance which they bore to the ravages of moths, or tineæ, on certain substances. But it seems as probable that erroneous pathological views gave rise to the term in question, and that it originated from a belief in certain minute insects, or tineæ, being the proximate cause of the eruptions to which it was given, rather than from

their resemblance to the ravages of moths. Indeed, we find a writer not only broaching this view of porriginous eruptions, but actually describing the appearance of the insects which he supposed were the cause of them. “Sennertus,” says Turner, “hath often seen them, and been consulted by way of prescription to destroy them*.” What Sennertus took for insects were probably neither more nor less than *nits*, as they are vulgarly called, or the remains of the ova of pediculi,—a class of parasites neither very scarce nor barren on the heads of many individuals.

Since the promulgation of Willan's classical nomenclature of eruptive diseases, the term tinea has been but seldom used in this country. Willan rejected it as a word not sanctioned by ancient usage, and adopted its synonym, porrigo, which, as Bateman remarks, he uses nearly in the same sense in which it was used by Celsus, who bestowed it on the moist and ulcerating, as well as the furfuraceous, eruptions of the scalp.

Considerable difference is still to be met with in books respecting the number of species that ought to be ranked under the generic term, porrigo. That a culpable laxity of diagnosis leads many to range eruptions under that term, which, strictly speaking, are not at all entitled to it, every one who has seen any thing of cutaneous practice will readily admit. By several practitioners and different writers, almost every eruption which attacks the scalp is indiscriminately classed as a porriginous disease. Willan himself is, to a certain extent, chargeable with this error. The eruptions which he has arranged under the generic term porrigo, are very dissimilar in their nature and external appearance. While some of them, such as those eruptions so common in young children, and known by the name of porrigo larvalis, crusta lactea, or milk scall, are generally of a salutary tendency, and may be considered as efforts of the system to obviate more serious diseases, others are more strictly local in their origin, and in place of being salutary, harass the patients during a long period, bidding defiance too often to the best-directed efforts for their cure, and when cured not unfrequently leaving behind them permanent deformity in the shape of in-

* Lorry, de Morbis Cutaneis, p. 463.

* Turner, de Morb. Cut. p. 136.

curable alopecia, or baldness, to a greater or less extent.

In place, then, of keeping diseases so dissimilar in their nature in the same genus, it would be better I think to draw a line of distinction between them, and refer them to different genera, as we may be warranted in doing. That the porrigo larvalis and porrigo favosa of Willan are diseases very different from the other forms of porrigo described by the same author, and infinitely less rebellious in general to remedial treatment, few, who have paid particular attention to eruptive diseases, will deny: and that the two forms of eruption just specified would be more correctly placed in the genus impetigo, than in that of porrigo, is an opinion which is now beginning to be pretty generally admitted.

Bateman, who adopted Willan's arrangement and nomenclature of porriginous diseases, in deference to his illustrious preceptor, and in order to avoid the confusion of altered arrangement, nevertheless observes, "That experience has led me to question altogether the propriety with which Dr. Willan classed the crusta lactea under the genus porrigo, and to believe that impetigo larvalis would have been the more correct appellation." The porrigo favosa of Willan seems likewise to be a variety merely of impetigo affecting the scalp. The pustules which it exhibits are, psudracia, and the discharge which they pour out when ruptured, is very similar to that furnished by the other forms of impetigo*. Referring, then, these two eruptions to the genus impetigo, we have remaining Porrigo furfurans—P. lupinosa—P. scutulata—P. decalvans: four forms of cutaneous lesion alone entitled to be ranked as true porriginous affections.

In Alibert's new classification, his group of porriginous diseases ("dermatoses teigneuses") consists of three genera.

Genus.	Species
1. <i>Achor</i>	{ <i>Lactuminosus</i> . <i>Mucidiuus</i> .
2. <i>Porrigo</i>	{ <i>Furfuracea</i> . <i>Amiantacea</i> . <i>Granulata</i> . <i>Tonsoria</i> .

3. *Favus* { *Vulgaris*.
 { *Scutiformis*.

However ingeniously Alibert may argue in favour of ranking the above-mentioned eruptions together—however acute he may be in seeing the affinities which they possess, and which entitle them, in his estimation, to form together "one natural group"—we can see no reason for his referring the genus, *achor* and *porrigo granulata*, to the "dermatoses teigneuses," rather than to the "dermatoses eczematueuses," except that they, like the true porriginous eruptions, appear ordinarily on the scalp. The region of the body, then, on which they appear, seems to us to be the bond of union between Alibert's dermatoses teigneuses. He, however, observes, "les diverses teignes ont des phénomènes communs, qui considérés d'une manière générale doivent les rattacher au même groupe dans la grande famille des dermatoses; tels sont l'irritation plus ou moins vive de cuir chevelu, son état hyperhémique, le prurit, une sorte de douleur tensive, et plus ou moins prononcée, selon quelles pénètrent plus avant dans l'épaisseur du derme*."

We nevertheless are of opinion that Alibert has approximated eruptions which have really no great claim to be ranked together; and in place of simplifying, he has rendered more intricate the study of porriginous eruptions, by his confused arrangement, and the introduction of new terms.

M. Bielt is of opinion that the species belonging to the genus porrigo may be reduced to two—the porrigo lupinosa and P. scutulata. In this opinion he comes pretty near Lorry, who affirms that the only disease entitled to the term tinea is the disease known at present by the name of porrigo lupinosa. Bielt, Rayer, and others, maintain that the eruption denominated porrigo furfurans is not referrible to porrigo, being always, as they assert, either eczema attacking the scalp, or pityriasis. That eczema is often mistaken for a porriginous affection, we very readily admit; but we are at the same time firmly persuaded, that the above-named pathologists have committed a great, and to us incomprehensible mistake, in denying the existence of Willan's furfuraceous porrigo. It is the form of porrigo most commonly met with in this country, and it is

* Dr. Burns, whose opinion is entitled to great deference, observes "that porrigo favosa is a mere variety of porrigo larvalis," or, as it ought rather to be called, impetigo larvalis, "differing in nothing except in the pustules being a little larger."—*Burns' Midwifery*, p. 595.

* *Monographie des Dermatoses*, tom. i. p. 426.

in general exceedingly difficult of cure. We would hence have supposed that it would have forced itself upon the attention of the most careless observer. From its spreading through families into which it has been introduced, and from being occasionally produced by the other forms of porrigo, we are surely warranted in ranking it as a porriginous affection. It is to this disease that the popular term Ringworm is most frequently given*. We admit, however, contrary to the opinion of Willan and Bateman, that the elementary form of the eruption is not pustular, and hence objections may be most justly started against placing it in the genus porrigo. Yet notwithstanding the apparent inconsistency of classing a non-pustular eruption with others of a pustular character, it is proper, I think, considering its affinity to the other forms of porrigo, to suffer it to remain where Willan and Bateman have placed it.

Plenck, from observing that the different species of porrigo appeared with different elementary lesions, was led to rank them in no less than three different orders, viz. Squamæ—Crustæ—Morbi Pileorum. Thus, in his little work, entitled "*Doctrina de Morbis Cutaneis*," we find several eruptions of the scalp under the terms tinea and scabies capitis, amongst the crustæ. We find nearly the same diseases, under the term porrigo, amongst the squamæ. Lastly, porrigo decalvans, or, as Plenck calls it, alopecia areata, is met with amongst the morbi pileorum. Admitting (as we must) that there is a difference in the elementary appearance of the several eruptions included under the term porrigo, it is, nevertheless, we think, an unnatural and unnecessary proceeding to separate, so widely as Plenck has done, diseases in other respects having many points of resemblance. Such a proceeding would prove highly disadvantageous.

* We are quite aware that it is the *P. scutulata* of Willan that is deemed, by the majority of medical practitioners, "ringworm." We shall afterwards prove, however, that it is the *P. furfurans* which is, by non-professional people, reckoned ringworm, a disease which is beheld with much alarm and regret by well-informed parents, when it appears within the family circle. They are aware of its inveteracy. It is a disease, moreover, which is better known by many out of the profession than by some practitioners. We shall afterwards state our reasons for thinking that *P. scutulata* is a term which might, with every propriety, be expunged from the cutaneous nomenclature.

Taking contagion, difficulty of cure, and alteration of the appearance of the hair, as the most striking features of the genus porrigo, we may place in it, without reference to their elementary forms, the *P. lupinosa*, the *P. scutulata*, the *P. scutulata*, the *P. furfurans*, and the *P. decalvans*.

By observation and reflection we have been led to believe that the organs attacked by porrigo are the sebaceous glands and structure secreting the hair. The connexion between these two structures is so intimate that they must act and re-act upon each other. From this consideration some light is thrown upon the symptoms of the disease in question. In porrigo lupinosa, for example, the sebaceous follicles are the organs primarily affected, and from the intimate connexion between these organs and the structure producing the hair, the latter is very apt to suffer. It is from this communication of disease from the one structure to the other, that the hair so generally and so speedily becomes altered in appearance in this form of porrigo; and the incurable alopecia, which it not unfrequently causes, results from the disease destroying both the follicles and structure secreting the hair. In porrigo furfurans (true ringworm) the disease originates in the structure producing the hair, and from the reverse of what occurs in *P. lupinosa*, the sebaceous follicles may become secondarily affected, and produce an irregular pustular crop scattered over the furfureous patches.

It is not long since Alibert remarked, "Que trouve-t-on dans les auteurs touchant la nature et le caractère spécifique des teignes? Des renseignements incertains, des dissertations vaines, des détails vagues*." Since then some important light has been thrown on some of these very interesting forms of cutaneous disease. Much, however, yet remains for further investigation.

In the preceding remarks we have endeavoured to establish the forms of disease strictly referrible to the genus porrigo—a task by no means so unimportant as some may be inclined to think.

Respecting the number and specific names of the eruptions referrible to porrigo, much difference of opinion is ob-

servable in the writings of French and British dermatologists; and a considerable step in the progress of improvement will have been made, when uniformity on their topics is manifested by the profession profession at large, in this and other countries.

In admitting the existence of four forms of porriginous disease, we beg to remark, that we believe these four species originate in but two different structures or sets of organs, so that we are inclined to think that porriginous eruptions might be reduced to two, viz porrigo originating in the sebaceous glands, and porrigo originating in the structure producing the hair. These opinions may be premature or incorrect; let observation confirm or confute them.

[To be continued.]

PUNCTURATION IN ASCITES.

To the Editor of the Medical Gazette.

SIR,

THE patient treated for ascites by puncturation, of whom there appeared some account in the number of your journal of the 7th ultimo, has continued to improve in health, and is now nearly well. That account extends to the 25th of September last, at which period I had made thirty-eight punctures, and obtained a reduction of four inches in the circumference of the abdomen, with alleviation of the urgent symptoms, and marked amendment of the general health of the patient. Thirty-three punctures have been made since that time: in September, two on the 26th, two on the 29th, and five on the 30th; in October, two on the 2d, two on the 4th, two on the 5th, one on the 10th, one on the 11th, one on the 13th, one on the 16th, one on the 18th, two on the 20th, two on the 22d, one on the 24th, two on the 26th, and three on the 30th; in November, one on the 3d, one on the 6th, one on the 11th, and one this day. Their mode of action appears to be this:—the fluid, with the exception of a small quantity which escapes through the opening in the skin, oozes for three or four hours from the cavity of the peritoneum into the subcutaneous cellular tissue, which it loads and oedematizes. From hence

it is gradually absorbed, and chiefly during the next 5 or 6 hours; after which the patient voids a good deal of urine, and often perspires pretty freely. This process appears to be continued or repeated more or less completely, until the peritoneal orifices of the punctures close. Thus has the patient been relieved of the greater part of the fluid which the peritoneum contained. He is now almost restored to health. The circumference of his abdomen is now only three feet four inches, being one foot four inches less than when the operation was first performed: his pulse is at seventy-two, his breathing free; the muscles have regained firmness and strength, and he is beginning to resume his occupations as a surveyor. His tongue is still rather red at its margin, and smooth on the surface, but the appetite is good, and the stools are natural. The urine appears to be of good quality; it is frequently abundant, and then rather pale and aqueous.

The punctures were performed nearly on a level with the umbilicus, with the exception of eight or ten made three or four inches above it: about one-third of them were made in the linea alba, and the others on either side, and within a distance of this line varying from four to eight inches. With regard to their number and the days on which they were practised, I was guided somewhat by the effect they produced on the disease and on the patient. As they sometimes left a little soreness, and I found latterly a small number answer as well as more, I made the fewest requisite. The soreness, and occasionally the indisposition of the patient, sometimes induced me to make them less frequently than every other day, which, however, upon the whole, seemed to me to leave the fittest intervening time from one operation to another.

During this treatment the diuretics and blue pill, previously administered by Mr. L'Estrange and myself, have been discontinued. We have given only potas. nitrat. gr. xii. thrice a day, with whey and parsley tea.

I remain, sir,
Your obedient servant,

THOMAS KING.

6, Maddox-Street,
Nov. 13, 1837.

LUNATIC ASYLUMS.

To the Editor of the Medical Gazette.

SIR,

YOUR observations on the "Regulation of Lunatic Asylums" are judicious and humane; for although much has been effected to improve the management of the insane since the existence of the flagrant instances of abuse which you quote, it is to be feared that much still remains to be done. To assist in such improvement has been the great object of my professional life, and I trust that my efforts have not been altogether unavailing. Unwilling, however, to thrust myself on public notice, I should not now have addressed you but for an expression in your remarks which has been misinterpreted to my disadvantage, and which, as it is evidently unintentional on your part, I feel confident you will permit me to explain.

In page 135 you say, "In a Wiltshire madhouse, Dr. Fowler found a man confined in an oblong trough, chained down;" and you then copy a part of Dr. Fowler's evidence. You do not, however, as Dr. Fowler did, mention the name of the house; and as this is the oldest asylum in Wiltshire, and as Dr. Fowler has been for more than twenty years the physician appointed by law to visit here, the observation has been erroneously supposed to point at this house. You are aware that the slightest breath is sufficient to taint the purest reputation, and you will not wonder at my earnest desire to correct so groundless a mistake; and the rather, that the Minutes of Evidence before the Select Committee of the House of Commons, from whence the extract is made, contain many flattering testimonies on the conduct of this asylum, and that Dr. Fowler himself stated that Laverstock House was the best private asylum he had ever seen. This good opinion, I have reason to believe, has never been forfeited.

Whilst I am upon the subject, I would remind you that the legislature formerly sanctioned the ignorant and barbarous treatment of lunatics to which you allude; for an act of parliament, which has not been repealed many years, actually authorised the confinement of lunatics in asylums, "there to be chained and beaten if necessary!"

And to shew you that such practices exist even now *out of* asylums, it is not long since, that, on the arrival of a patient here, the person who brought him counselled my partner, Mr. Lacy, to shew the patient a whip if he was refractory, for that he was always better after a good horsewhipping!" Every day's experience, indeed, shews me how little the kind and true management of the insane is understood, where they are under the care of relations or mere keepers; and as I am willing to hope that great improvements have taken place in most lunatic asylums, I entertain the firmest conviction that no management can be so perfect and effectual as may be found in the best establishments of that kind.

Your "strongest instance" to prove the necessity of better regulations in licensed houses—namely, the "Willis Moseley" case—is scarcely available as an argument; because in that case there was no licenser, and consequently no visitations.

With respect to the proportion of cures at the Middlesex Lunatic Asylum being less than the acknowledged excellence of its arrangements may lead you to expect, it should be borne in mind, that as the house was quickly filled with pauper lunatics, brought from all parts of the country, many of them were old cases, long established as incurables. To illustrate this remark, I would mention the result of practice at Laverstock, transmitted some time ago to the Lord Chancellor, in conformity with his instructions.

* Of every 100 patients admitted here, within three months from the first attack of insanity, there were cured 88.

Of every 100 admitted whose disease had existed more than three months, and not exceeding five years, there were cured 45.

Of every 100 admitted whose insanity had been existing more than five years, there were cured only 12.

The number of patients here, under restraint, does not exceed $2\frac{1}{2}$ per cent.; and the number of deaths, including periods of cholera and influenza, is not more than 5 per cent.

I have been led on farther than I in-

* A similar return from the York Retreat states the number cured in this class to be 87; and Capt. Basil Hall states a similar return from the best American asylum to be 89.

tended, and fear that I have intruded too much on your space; but the subject is one which deeply interests me, and if I can see my way clearly to a point where legislative enactment may, without cramping and fettering the energies of honourable practitioners, effectually guard the unhappy objects of its care from ignorance and brutality, I shall indeed rejoice; but there are a thousand considerations which arise, too important to be dispatched hastily, and which require the most cautious examination. The cure is as much an object of anxious attention as the care of lunatics; and after all that can be accomplished by mere law, much must necessarily depend on the skill, feeling, and integrity of those who have the conduct of asylums.—I am, sir,

Your very obedient servant,

W. FINCH, M.D. F.L.S.

Laverstock House, Wilts,
Nov. 13, 1837.

SIR DAVID BREWSTER ON PRESBYOPIA AND CATARACT.

To the Editor of the Medical Gazette.

SIR,

SOME years ago, my attention was attracted by the following remarks from the pen of Sir David Brewster*.

"There is a change which takes place in the human crystalline lens, between the age of 30 and 50, when the person begins to experience slight difficulties in reading small type or ill-printed books, particularly with candle light. This imperfection of vision, at first very slight, and scarcely appreciable, arises from a mechanical change in the condition of the lens, by which its density and refractive power, as well as its form, are altered. This change is analogous to that which takes place in every other part of the human frame on the approach of age. It commences in general, not uniformly throughout the whole lens, but in particular parts of it; most frequently at the margin of the lens, and is often accompanied by a partial separation of the fibres and laminae of which it is composed.

"If the human eye is not managed with peculiar care at this period, the

change in the condition of the lens often runs into a cataract, or terminates in a derangement of fibres, which, though not indicated by white opacity, occasions imperfections of vision that are often mistaken for amaurosis and other diseases. A skilful oculist, who thoroughly understands the structure of the eye and all its optical functions, would have no difficulty, by means of nice experiments, in detecting the very portion of the lens where this change has taken place; in determining the nature and magnitude of the change which is going on; in applying the proper remedies for stopping its progress; and in ascertaining whether it has advanced to such a state that aid can be obtained from convex or concave lenses. In such cases lenses are often resorted to before the crystalline lens has suffered an uniform change of figure or of density; and the use of them cannot fail to aggravate the very evils which they are intended to remedy. In diseases of the lens where the separation of fibres is confined to a small spot, but is yet of such a magnitude as to give separate coloured images of a luminous object, or irregular haloes, it is often necessary to limit the aperture of the spectacles, so as to allow vision to be performed by the good part of the crystalline lens."

Similar remarks, with part of the above as an extract, are to be found in Sir David Brewster's Treatise on Optics, p. 302. Speaking of long-sightedness, he observes, that the change in the state of the crystalline upon which it depends "frequently begins at the margin of the lens, and takes several months to go round it."

As it is not readily to be supposed that a philosopher of Brewster's patient turn of mind and profound knowledge would throw out any hasty or ill-founded observations on so important a subject, it is to be regretted that the learned author has not been somewhat more explicit in the statements he has made in the above passages. Many of your readers, I think, would consider it desirable to be favoured with answers to the following queries.

1. How has it been ascertained that the change on which presbyopia depends commences in general, not uniformly throughout the whole lens, but in particular parts of it, and most frequently at its margin?

* Edin. Encyclop. vol. xv. p. 623.

2. How has it been ascertained that this change is often accompanied by a partial separation of the fibres and laminae of which the lens is composed.

3. What is the evidence that mismanagement of the eye at the period when presbyopia commences, often causes that change to run into a cataract, or to terminate in a derangement of fibres, occasioning imperfect vision that are often mistaken for amaurosis and other diseases?

4. What are the nice experiments by which it is so easy to detect the very portion of the lens where this change has taken place, and to determine the nature and magnitude of the change which is going on?

5. What are the proper remedies for stopping the progress of this change, which the skilful oculist would have no difficulty in applying?

6. Is it common for those in whom the presbyopic change is advancing, to complain of seeing coloured images of luminous objects, or irregular haloes of light?

7. On the supposition that they are affected in the way just mentioned, will limiting the aperture of the spectacles really prevent vision by the disordered portion of the crystalline lens?

8. On the supposition that limiting the aperture of the spectacles allows vision to be performed only by the good part of the crystalline, will the continued use of such spectacles be followed by any permanent advantage?

These queries may not fall under the notice of Sir David Brewster: if they do, I should hope that he would honour them with a few sentences of explanation. Perhaps some of your numerous readers may have directed their attention to the same subject, and may take an opportunity, through the medium of your widely circulated pages, to give their views upon it to the public.

I am, sir,

Your obedient servant,
A STUDENT OF THE EYE.

Glasgow, Oct. 18, 1837.

[We shall take care that this is brought under the notice of Sir David, and shall be happy to insert any answer to our intelligent correspondent with which he may favour us.—*Ed. Gaz.*]

SINGULAR NERVOUS ATTACK.

To the Editor of the Medical Gazette.

SIR,

If you deem the following case worthy of insertion, you will oblige me by giving it a place in the *Medical Gazette*.

I am, sir,

Your obedient servant,
HANNAM S. THOMPSON, Surgeon.
Dover, Nov. 1837.

Jonas Archer, a mariner, *ætat.* 27, has had gonorrhœa and syphilis; the latter severely, for which he was copiously salivated: has had also ulcers on both his legs, which were open nearly a year and a half, and have only healed up three months.

On August 25th he rowed thirty miles under a very hot sun, and drank a moderate quantity of brandy during the exertion; was well and sober when he reached home in the evening. The following morning, while out walking, he suddenly became giddy, and fell to the ground. In a few minutes he felt partly recovered, and began to walk homewards, but he found that he could not direct his steps. Whatever effort he made he could not prevent himself from reeling from side to side of the road. All this time he was perfectly conscious, and fearing lest passers-by should think him tipsy, he reached home by the most unfrequented paths. He was immediately visited, when there was found a slight hemiplegic affection of the right side. The head was rather hot; both pupils moderately dilated, but perfectly contractile; a little pain over the left temple; pulse a little slower than natural, not labouring nor strong; faculties perfect; articulation quite distinct. Local depletion, brisk cathartics, and an antiphlogistic regimen, were prescribed, and in a few days nearly all the traces of the attack had disappeared; when, on Sept. 15, he was suddenly seized with a violent uncontrollable fit of laughter: this occurred several times in the morning, and in the afternoon he was much worse.

There was now hemiplegia, still slight, of the left side of the body; the powers of the right side being completely restored. His articulation was indistinct, and deglutition difficult;

the pupils were dilated but sensible, and the pulse slow and rather labouring; his sensorium was also confused. He was bled from the arm, freely purged, and a blister was applied to the nape. His hair cut off quite close, and a cold lotion applied. The following day he was much better, *with the exception of his mouth being obstinately closed*. The teeth were so firmly clenched together that they could with the greatest difficulty be separated so as to admit the point of a spoon. The masseters and temporals felt hard and rigid. When a little fluid was poured into the mouth it was swallowed by a sort of convulsive effort, and then only after the lapse of a few moments, and when he was in the recumbent posture. He not only could not articulate in the least, but the larynx had lost the power of even forming a sound. He was perfectly conscious; had no pain; the head cool, pulse slow, small, and feeble; heat of surface natural. Three grains of hydrarg. chlorid. were conveyed into the mouth every three hours. In three days ptialism shewed itself, and from that date he began to recover the power of opening the mouth. He could also make a sort of grunting noise in his throat. The paralytic affection of the left side of the body was likewise diminished.

Nov. 4th.—He can speak, swallow, and protrude the tongue, nearly as well as ever. The faculties of his mind are perfect, and there is no paralytic affection of either side of the body remaining.

Some of the features of this case appear to me to be exceedingly interesting. 1st, the fact, that in the first attack the *right side* of the body was paralyzed, while in the second, (occurring only a few days subsequently) the *left* was similarly disabled. 2d, the complication of trismus with paralysis in the second attack. 3d, the very rapid subsidence of the symptoms immediately ptialism was produced. 4th, the violent and uncontrollable laughter which ushered in the second seizure, and which was far more characteristic of hysteria than a well-marked case of hemiplegia.

It would be a waste of time and space to speculate on these phenomena; still, I would ask, were these hemiplegic attacks caused by sanguineous effusion? If so, was there, or was there not, a distinct effusion on the occasion of each

attack? Is the disappearance of the paralysis of the right side before the supervention of the affection of the left, to be attributed to the removal of the coagulum by absorption, or to an acquired insensibility of the brain to its presence? What is the explanation of the second attack being ushered in by involuntary laughter? Why were the muscles supplied by the eighth and ninth pairs of nerves totally paralyzed, while those influenced by the third branch of the fifth, were in a state of spastic rigidity and contraction.

These questions naturally suggested themselves to me during the progress of the case. I have attempted to frame answers to them, but, however satisfactory to myself, I feel they are not sufficiently well-grounded to justify me in inflicting them on your readers. I shall take care not to lose sight of the case, and in the event of an unfavourable termination I shall take the liberty of forwarding you a description of the morbid changes in the brain.

SOME ACCOUNT OF MESMERISM.

By BARON DU POTET DE SENNEVOY.

[Continued from p. 295.]

AFTER the death of Mesmer, magnetism was forgotten, with all the individuals who possessed a knowledge of it. A conflict of a very different magnitude soon arose in France. Other interests were then to be defended. Every one, amidst the tumult of public affairs, thought of himself, and paid little attention to a truth which sank into insignificance under the pressure of events that riveted the attention of all, and wrought changes in every state.

The disciples of Mesmer, all possessed of wealth and rank, were obliged to expatriate themselves, to save their lives: they bore back the science of magnetism to its cradle. It was studied in Germany and Holland; some of the disciples of Mesmer carried it even as far as America. Thus, during a period of several years, we do not perceive any trace of magnetism in France. Some isolated individuals alone operated in silence, and spread benefits around them, without publishing the results of their practice.

The followers of Puysegur, however,

soon appeared upon the scene, animated by a philanthropy that personal misfortunes had not diminished. They published new works, in which they developed the doctrine, supported by numerous facts. Magnetic somnambulism added to the attractions of magnetism, and physiology and psychology, were enriched by a discovery destined to awaken the admiration of the learned. If the enthusiasm with which it was received was less vivid on this second appearance of magnetism, it was, however, more lasting; less parade was made of this newly discovered truth, and more persons occupied themselves in the privacy of the closet with studying its results. There were, however, some found willing to enter the lists against the incredulous; but having no fixed rule by which to conduct themselves, and not knowing any certain test by which to recognize the real effects from such as might be feigned, they rendered but an imperfect service to the cause. Of this number was the Abbé Faria. He opened a public institution for magnetism, and drew around him many scientific men. Without entering as yet into judgment on the doctrines of magnetism, we should at least render him full justice. He contributed much towards spreading it in France.

Endued with the power to a degree that would be almost incredible, if it were not a known fact that its intensity increases with exercise, he forced several scientific men to acknowledge an agent till then unknown to them. The spirit of the age was positive, and as then, more than ever, it was generally acknowledged that there could be no effect without a cause, the new converts to magnetism esteemed it incumbent on them not to disdain the study of this discovery. They attached themselves, therefore, to this man of wonders, and followed him with increasing ardour; they learnt themselves to magnetise, and soon found that the magnetic effects were produced by a natural cause, depending on our volition; that this faculty was common to all; and that the Abbé Faria did not exercise it by any exclusive charter. More considerable works upon it appeared; the names of their authors, and the reputation they enjoyed, at length permanently fixed upon it the attention of many educated persons.

The major part of the learned, who

remembered that they had been judges in this question, and that they had pronounced its non-existence, shrank from the risk of being obliged to retract; they feared to be convinced, and that they should be accused of having decided too hastily.

Before we proceed further in the history of magnetism, it is requisite to give some account of the means which Mesmer employed to spread his discovery, and to support his doctrine. The effects produced being also those which were submitted to the examination of the first Commissioners, and on which they founded their report, we must, in order to enable the reader to form a just estimate of their importance, carry him back for a moment to that early epoch; and this is the more necessary, inasmuch as the magnetic effects which we are producing at the present day are no longer the same as those exhibited by Mesmer; the means necessary to call them into action are entirely different, and the agents which he considered essential are now neglected.

The following is a description of the processes, and of the apparatus by which Mesmerism was established in France:—

In the centre of the chamber was a kind of tub, several feet in depth, having a cover, with two lids pierced with holes, through which curved and moveable iron rods were passed. The interior was filled with bottles full of water which had been previously magnetised. These bottles were placed in rows, one above the other, so disposed as that the necks of those in one tier were turned towards the centre of the vessel, and their other end towards the circumference; whilst those in the next tier were in the reverse position. The vessel also contained a certain quantity of water, filling up the spaces caused by the disposition of the bottles; but it was not indispensable that every thing should be precisely as above detailed. Sometimes iron filings, ground glass, sulphur, manganese, and many other substances, were put into the water.

The patients were placed round the apparatus, and directed the iron rods to the parts affected. Sometimes the body was encircled by a hoop suspended from above. They occasionally formed what was called the *chain*, by holding each other by the thumb and fore finger. The magnetiser, armed with an iron

rod, which he passed over his patients, appeared to direct at will the course of the magnetic fluid. All this apparatus of water, bottles, and metallic rods, was supposed to be essential to the disengagement of the magnetic agent. Sometimes the operation was accompanied by the notes of a piano or armonica; for it was one of the propositions of Mesmer, that the propagation of magnetism was especially assisted by sound.

These processes formed the basis of the treatment applied to several individuals together. Magnetism could also be exercised in various different ways. The universal fluid being every where, the magnetiser contained a portion of it in himself, which he had the power of communicating and directing by a rod, or by the movement of his extended fingers. In addition to these gestures, performed without coming in contact with the patients, it was customary to touch gently the hypochondria, the epigastric region, or the limbs. To add to the effect of these operations, trees, water, articles of food, or other objects, were also magnetised; for all bodies in nature, according to Mesmer, were susceptible of magnetism.

The patients submitted to this magnetisation experienced various unusual sensations, wandering pains all over the body, but especially in the head and stomach; the augmentation or the suppression of the cutaneous perspiration; palpitations of the heart, and momentary suffocation; sometimes a certain exaltation of the mental faculties, and a vivid sensation of comfort and enjoyment. The nervous system appeared to be more especially affected; the organs of the senses experienced unusual modifications, such as tingling in the ears, dizziness, and sometimes a kind of somnolency of a peculiar character. These effects varying infinitely according to the nature of the disease, and the idiosyncrasy of the invalids, continued increasing in proportion to the duration of the magnetisation, and this series of phenomena terminated also by the most remarkable and most constant of all, viz. convulsions. The convulsive state once excited in a patient, which sometimes did not take place till after several hours, never failed to manifest itself in all the others. This was designated by the term of the *magnetic crisis*. As

this crisis was usually the *dénouement* of the effects produced, it was considered as the object of the magnetic action, and as the means employed by nature to produce the cure.

The convulsions were alarming by their violence and duration. The patients, when seized by them, were immediately carried into a neighbouring apartment, called, on account of its destination, the *Crisis Chamber*, where they gradually came to themselves. One very remarkable circumstance was, that on their recovery they felt only a slight sensation of fatigue, and several even professed to have received a decided relief.

To these physiological results were joined some very extraordinary moral phenomena. Some laughed violently, whilst others, submitted to exactly the same mode of treatment, shed tears. They were frequently drawn towards each other by irresistible impulses of sympathy, and reciprocally testified the warmest affection. But what was most surprising was the prodigious influence possessed by the magnetiser over his patients. An intimation of his will excited or calmed the convulsions; commanded love or hatred; his rod seemed like a magic instrument, to which body and spirit yielded obedience. These astonishing results were also produced, although in a less degree, when he operated on individuals separately.

Such were the effects which the first Commissioners verified, and which they described minutely in their reports.

The Commissioners, however, declared that magnetism did not exist. They were of opinion that all these phenomena might be explained by known causes: they believed that imagination, animal heat, excitement (*Périsisme*) of the skin, and imitation, were the only agents employed by Mesmer; and that the pretended discovery of animal magnetism was a *German rerie*, to be classed with the numerous other reveries which appear and vanish in every age. The report, containing this celebrated judgment, was profusely circulated: but this only stunned public opinion; it did not prevent the progress of magnetism.

The judgment pronounced by the Commissioners was attacked from all quarters. A society existed at Strasbourg which published annals, in which

were inserted facts of magnetisation, contradictory to the explanations of the Academy. Nantes, Bordeaux and Lyons, followed the example of Strasbourg.

The following were the arguments opposed by the magnetisers to the conclusions in the report:—

We admit, said they, that imagination, animal heat, imitation, &c. do produce effects in certain cases, and that these effects are even incontestible.

But we say further, that magnetism can act without the intervention of any one of these agents, and can produce effects which those agents are insufficient to explain.

These are the grounds of our belief. The magnetic action may be exercised in various ways. Some magnetisers touch their patients, hold them by the thumbs, and employ friction; others, on the contrary, content themselves with simply directing their hands in front of the individual whom they are magnetising; finally, others only look at them, and fix their thoughts upon them. All, however, notwithstanding such different modes of operating, succeed in producing the same phenomena.

Now let us examine whether it be the causes specified by the Commissioners which act, or if it be not rather the magnetic agent.

We will begin with *animal heat*. In order, however, that this cause may act, it is necessary to touch the patient; but as we have just remarked, this was not in any degree requisite, as the magnetiser could operate at the distance of several paces.

The case is the same with excitement of the skin. To act on this part, it would still be necessary to rub, or at least touch it; and the proceedings which that mode of treatment required have been laid aside, without the magnetic effects ceasing to appear.

Imitation.—This cause may act in an assembly when several persons are magnetised together; but if in silence and privacy, when the magnetiser is alone with his patient, more striking effects are obtained than in public, then imitation must also be rejected.

We now come to *imagination*, a favourite argument with many. To act by or upon the imagination, the individual to be acted upon should be informed of the effects intended to be produced upon him; he should be made

aware that the operator possesses great power; he should know that he is able to influence him by occult causes, or at least the magnetiser should have command over him either by his attitude or his looks, &c. There is no doubt that by any one of these circumstances it might be possible to disturb his imagination; but if we prove that the effect we produce is so much stronger in proportion as the individual is less aware that we are acting upon him—if we establish by certain and positive facts, that an individual magnetised without his knowledge, and even through a thick partition, is sensible to magnetism—if we see that infants, and those animals of which the organization is the most closely related to our own, are decidedly affected in some way or other when submitted to magnetisation, it must then be admitted that the imagination cannot be considered as the determining cause of these phenomena.

The magnetisers were no otherwise embarrassed than in the selection of the materials by which to prove that the effect produced was independent of the imagination; but their principal argument was this:—When we magnetise adults or children while in a state of natural sleep, we see effects come on almost immediately; and yet they are ignorant of the action exerted upon them. Although we may be placed at the distance of several feet from the magnetised persons, the effect is yet more rapid and more marked than in their waking state, even when they are touched.

No satisfactory answer could be made to these assertions; nevertheless they did reply, but only by negatives. They formally denied the fact; and from that period the magnetisers gained their cause, for they needed only to shew the contested phenomena to those who doubted, in order to insure belief.

It was thus that magnetism took root in France. At the present day very few enlightened men doubt the existence of the magnetic agent; they only dispute its results. But the majority of the scientific know well that the discovery of Mesmer is a truth worthy of the greatest interest, for it is destined to work the greatest changes in the systems of philosophy and medicine.

In the next article, when speaking of somnambulism, we shall shew that it is that singular state which has prevented

magnetism from making a more rapid progress. We shall then explain why the experiments for proving the existence of the lucid state rarely succeed. For the present, confining ourselves to the part of the historian, we only make known the explanations which have been hazarded by some modern magnetisers, to render the magnetic phenomena comprehensible. But the reader ought to be informed that several systems have already existed; that three very distinct schools were formed; that next to that of Mesmer came that of M. Puysegur, and then that of the spiritualists.

The school of Mesmer was founded on a system analogous to that of Epicurus, as explained in the poem of Lucretius. That of the spiritualists, which has many partizans in France, reminds us of the Platonic philosophy. The school of M. Puysegur is founded on observation.

[To be continued.]

SEQUELA OF RHEUMATISM—PHLEBITIS.

To the Editor of the Medical Gazette.

SIR,

THE following case, in which the veins of the right leg became secondarily inflamed from rheumatism, was admitted into St. George's Hospital a few years ago; and as several lectures are now in the course of publication on "Rheumatism and its effects," in the MEDICAL GAZETTE, I thought it might be sufficiently interesting to occupy a small space in the same journal.

Your obedient servant,

C. J. B. ALDIS, M.D. Cantab.

13, Old Burlington Street,
Nov. 10th, 1837.

George Emerly, æt. 23, painter, admitted November 9th, 1831, under Dr. Chambers. Pulse 108; skin cold; tongue red, with a brown fur, and a red streak along the mesial line; bowels open from medicine; urine free at present.

Complains of pain in the left hip and thigh; pains also in the shoulder and in the right leg above the ankle, shifting from one spot to another.

Attacked twelve weeks ago with cough and expectoration, and about eight weeks

since with the pains in his legs. Has been confined to his bed six weeks. Has been twice bled, blistered on the chest, and had leeches applied.

R. Pil. Hydrarg. gr. iv. omni nocte.
Rhei Pulv. ʒj.; Aq. Piment. ʒiiss.
cras mane.
Aq. Dist. ʒiiss.; Acid. Nit. ℥iv.;
Sacch. Albi, ʒss. ter die.
Diæta lactea.

11th.—Pulse and skin natural; tongue much cleaner; bowels open.

Rep. Haust. omni mane.
Diæta è pisce.

13th. *Vespere*.—Was attacked with symptoms of phlebitis in the right leg, and pain in the lower part of the abdomen.

R. Mist. Ætheris C. ʒj.; Liq. Opii Sedat. ℥xx. statim.—Fotus abdomini.—
Hirud. xij. femori, postea cataplasma Lini.—Fetus Papaveris cruri.—Hydrarg. Submur. gr. ij.; Opii gr. ss. 4tis horis.—Rep. Haust. Rhei mane.

14th.—Was attacked yesterday evening with pain in the lower part of the abdomen, which spread to the thigh and leg. At present the leg and thigh are swelled and tense, the former slightly œdematous, and the synovial membrane of the knee-joint is distended with fluid; abdomen still tense and tender. Relieved by the leeches: pulse 120, hard and jerking; tongue white, brownish along the centre.

V. S. ad ʒx.—Hirud. xij. femori.—
Catap. Lini et Fotus ut antea.—Rep.
Calom. et. Opium.
15th.—Olei Ricini, ʒiij. statim.—Rep.
V. S. ad ʒx.—Enema Oleosum.—
Haust. Salin. Efferv. 4tis.

16th.—Pulse much softer, still frequent; tongue furred; bowels open, with ochrey motions, very offensive; abdomen much less swelled and tense; still some tenderness in the right ilium; the leg still much swelled, with a tense œdema occupying the whole thigh and leg, with much tenderness. Blood drawn yesterday much inflamed: frequent hiccup.

Hirud. xx. femori, postea Catap. Lini amplum; applicetur abdomini 4tis horis. Catap. Sinap. per horæ ½.—
Haust. Ætheris C. urgente singultu.—Omit. Haust. Efferv.—Soda water.
18th.—Rep. Hirud. xij. Enema Oleosum.

From this time the symptoms gradually subsided, and the patient was cured in December.

MEDICAL GAZETTE.

Saturday, November 25, 1837.

"Licet omnibus, licet etiam mihi, dignitatem
Artis Medicæ tueri: potestas modo veniendi in
 publicum sit, dicendi periculum non recuso."

CICERO.

ON THE REMUNERATION OF
MEDICAL MEN.

OUR motto bespeaks us the defenders of the *dignity* of our profession, but we may, we hope, lay claim to some merit for having also maintained its more tangible interests. We have lately called the attention of our readers to some instances in which it has seemed to us that the services of medical men are most inadequately remunerated; and certainly there cannot be a more striking anomaly than the degrees of esteem (as evidenced by pecuniary reward), in which medical services are held by the same individuals, when acting for themselves, and when acting for the public or for smaller communities.

The governor of a charitable institution, for example, willingly tenders his fee for the cure or relief of a malady from which he himself suffers; but, acting in concert with others, he deems that a formal return of thanks is sufficient to repay the labours of the same physician or surgeon in the relief of the objects of his compassion. The director of an insurance-office, again, will not hesitate to pay for attendance on his own, it may be, trivial disorder; but he thinks that the time and opinions of the same person may be fairly given for the benefit of the company to which he belongs. We never heard of a coroner who expected to receive prescriptious gratis, but we have of many who refused to repay their medical attendant for all the trouble and responsibility of obtaining and giving evidence; and we should regard it as a most remarkable

occurrence, if a commissioner of the poor-laws, or an overseer, expected to be cured by contract, or preferred to have his medicines on the lowest possible tender, rather than draw his purse-strings for a sufficient repayment of the services rendered to him as a private individual.

The claims of medical men seem to be estimated in a very similar manner by the whole public. The amount of fortune which some have acquired in private practice are sufficient to testify how generously all are ready to reward benefits bestowed on themselves. Look, for instance, at the incomes of some of the more eminent: Dr. Meade earning and spending more than any contemporary, of whatever trade or profession; and Dr. Baillie receiving 11,000*l.* in eight months. Or to come to modern times, see the station which the leading men of the profession maintain in society—their household, and their equipages. Is it not strange that these very men should be expected to devote the hours in which they might increase their incomes almost indefinitely, to gratuitous attendance in the public service? Yet the very men whose incomes at the present time testify the value which private individuals set on their services, are gratuitously giving up some hours, nearly every day, in attendance on objects of charity, or in other works tending to the public good, and this without receiving more than constrained thanks from those in whose immediate service they are engaged, or having the credit among the public of being in the slightest degree more liberal than the members of other professions, who only work for and according to their pay.

We look in vain for analogous instances in other callings of gratuitous services, in works either of charity or of public good. In the law, for example,

those cases are the exceptions to an extensive general rule, in which its members are not paid for the occupation of their time and talents in the causes of charitable institutions, at the same rate as for other business; and so, too, whenever they are concerned in works of humanity, or of science, or generally in such as contribute to the good of society. Were it to be gravely proposed, that the defence of all benevolent institutions against actions or injuries—that the conveyances of titles to all their estates, and all their legal business, whether requiring the assistance of a barrister or attorney, should be gratuitously performed—it is easy to conceive the ridicule with which the proposition would be received; and yet the law, like medicine, is called a *liberal* profession, and its members do not fail to claim their title to this honourable appellation with as much earnestness and vigour as they defend their pecuniary claims. Some, indeed, find it difficult to establish both these claims, but consider themselves well recompensed for their losses in one, by being able to laugh as they win in the other.

If we look through the items of expenditure of a charitable institution, the only name that is ever missed is that of a medical attendant. Attorneys, surveyors, builders, government officers, parish authorities,—all have put in their claims, and been satisfied; and the one who, in many cases, as in infirmaries, hospitals, &c. has rendered the most important services, alone remains unpaid. Even in the largest institutions the only medical man (as a general rule) who receives a salary, is the resident apothecary, who is prevented from practising in private, and whose time is entirely devoted to the affairs of the institution. We know an hospital, where a gentleman employed as clerk to

the governors, and to conduct their legal transactions, receives 300*l.* a year, has a handsome house, rent free, and is, besides, paid for all law business as between attorney and client; while the medical officers to the same establishment receive a merely nominal salary. We do not mean to say that, in this instance, the gentleman to whom we allude is too well paid; his services doubtless fully merit their remuneration; but we place the case here for contrast's sake, and may add, that while he is not professionally known beyond the walls of the institution, the medical officers are all men of eminence, and receiving large incomes in their private practice.

In short, look where we may, we find no profession or trade wherein public services are given gratuitously, except our own; and it is not a little annoying to find that they are received as if those to whom they are rendered were legally entitled to them, and that the only anxiety shewn is to obtain more at the same cheap rate. But this we sincerely hope the profession will in a body resist. Let all that is required in adding to the efficiency of charitable institutions be given, and payment never once asked for; and let it be given conscientiously and liberally: a medical man can enjoy no greater satisfaction than he will receive from the assurance that he has employed his time and talents as diligently for the relief of those who were dependent on his charity, as of those from whom he received the largest donations, and we should be sorry in this respect to see the present system (except in a very few cases) at all departed from. At the same time, it must be said that the members of our profession are entitled to more thanks and credit from the public than they ever receive; and seeing that, in these cases, those whom they serve are so little grateful, let them in all others refuse to render their as-

sistance without adequate pecuniary remuneration being in some way insured to them. It was long since well observed that every thing is valued according to the sum it cost to be obtained, and we have no doubt that medical men will find the services for which they now receive neither fees nor thanks, repaid with thanks and money too, when the latter only is demanded.

But some one will say, these gratuitous services are not forced upon medical men, and therefore why do they undertake them? Nay more, why do they anxiously contend to obtain the situations in which they may render them? Now we are not disposed to deny that in many cases where there is no direct payment made for services to charitable institutions, an indirect remuneration is obtained; but we contend that this is very uncertain, and the hope of it often altogether fallacious. In the larger hospitals, for instance, to which schools are attached, the fees of pupils afford a considerable income to some of the medical officers, but these are subject to great fluctuations, according to the management of the institution, the reputation of the lecturers, &c. We need not go out of the metropolis to find an instance where, from a failure in one of these essential circumstances, the income of the officers derived from this source is not much more than nominal, and this in no degree the consequence of their own fault, or their own want of public reputation. The prospect of obtaining a large connexion among the governors of the various institutions, seems to attract many to all the troubles, and even degrading annoyances, of contests for these situations; but, as a general rule, there is no more illusive hope: we know many who have for years been medical officers of hospitals and dispensaries of various extent, and who allow that they cannot remember to

have received a single guinea which they could trace to their connexion with the governors; who, indeed, seemed to think them amply repaid by the votes, which enabled them to give their gratuitous services.

But these are only a part of the calls which are made on the time and services of medical men. In coroners' inquests, for instance, the remuneration is generally quite inadequate, obtained with trouble and difficulty, and at the expense of much anxiety and responsibility, and always given grudgingly; while the annoyance is heightened by seeing the members of the sister profession regularly, and without difficulty or remark, receiving their accustomed liberal, but not more fairly-earned, fees.

In all matters of public hygiene, &c. medical men, in common with other men of science, are generally expected to give their opinions gratuitously; while others, and even those who are to be profited by the results of the inquiry, are receiving adequate payment. In the instance to which we alluded last week, insurance-offices which refuse to pay a medical man for his opinion on the probable life of one who is about to be a source of profit to them, do not hesitate to pay a guinea to each director every week that he will come, and have a few hours' conversation with some friends, on his own and their business; and this, too, while the directors are generally men of leisure, retired from business, with little else to engage their attention; whereas the medical man has to abstract his time from other engagements in which pecuniary remuneration would be certain.

Perhaps the most flagrant instance in which medical attendance for public good is insufficiently repaid, is in the case of the poor; but on this we have written already too often to need much further remark. It is a striking ex-

ample of how little respect is paid to those who demand little. Practitioners not only admitted the system of contracts, and allowed their time and (where it existed) talent, to be sold like so much bread, or cloth, at the lowest possible price, but they entered into it with as much alacrity as if trade were their element; and even surpassed, in many instances, all the tradesmen, in their anxiety to be permitted to degrade themselves and their profession. They have been taken at their own price, and having once shown their willingness to move downwards in the scale of respectability, the efforts of all they are concerned with will henceforward be directed to push them still lower. But it is now fully time for them to stop. In every charitable cause, we hope the profession will be unanimous in giving their services, and that every individual will conscientiously exert himself to relieve the suffering poor, with as much attention and assiduity as he would in the case of his most munificent patient: nothing can be lost by this. But in scarcely any other instance of public duty, should they permit themselves to be unpaid. We recommend them, in short, to act in all these cases as the members of the legal profession do. We believe, that if they will follow this advice, they will increase in public esteem, in exactly the same proportion as their incomes from these hitherto unprofitable sources improve.

PATHOLOGICAL CHAIR.

WHEN we last alluded to this subject, we stated that Dr. Thomson, who had come to London for his health, had been obliged to return to Edinburgh, in consequence of the Town Council having refused to allow his deputy, Dr. Simpson, to lecture. Since that time the opinion of the Dean of Faculty has been received, and it proves to be in favour of the Professor's right to ap-

point another to officiate for him, in the event of his being himself incapacitated. It is obvious, that under the circumstances this point of law ought to have been ascertained at an earlier period, as Dr. Thomson has been put to great and unnecessary inconvenience. It is, however, to the general question of the expediency or inexpediency of having such a chair at all, that we wish to direct attention, because we know that a strong effort is being made in the new University to establish a separate course of Pathology as a portion of the curriculum; and a strong belief prevails that this results from a desire to serve one particular party, just as happened when the chair was established in Edinburgh. Now, acknowledging as we do that the gentleman in question is a most accomplished morbid anatomist, we must still protest against this as an unnecessary tax upon medical students, and an unwarrantable infringement on the department of other teachers.

In an Edinburgh paper before us, we find it stated that the question of instituting separate lectures on pathology was brought under the special notice of the Royal Commissioners, appointed some years ago to visit the Scottish Universities; and that they, after mature investigation, decided against it. "The subject," the learned Dean says, "was most anxiously considered by the Royal Commissioners; and *after examining all the medical men of science, and receiving opinions from all quarters*, they were unanimously of opinion that a separate class of pathology ought not to be instituted." We trust the Council of the University of London will give due weight to the opinion above expressed, as well as to the strong feeling which exists upon this subject, and avoid beginning their career with what, if carried into effect, will be generally regarded as a job.

CLINICAL LECTURE

ON

FRACTURE OF THE SKULL,

WITH DEPRESSION AND INJURY OF THE
DURA MATER AND THE BRAIN;

AND ON

CANCER OF THE LIPS.

Delivered at St. Bartholomew's Hospital,

BY WM. LAWRENCE, Esq. F.R.S. &c.

THE case to which I now direct your attention is a severe injury of the head—a comminuted fracture of the cranium, with bone beaten in on the brain, and portions of the latter forced out through the wound. An important practical question arises in the treatment of such injuries at two periods of their progress, namely, whether the injured bone should be exposed and elevated, or removed, at the time of the accident; and whether, if this has not been done, the measure ought to be resorted to at a subsequent period, if serious symptoms, such as convulsions and paralysis, should supervene.

Thomas Bennett, 13 years of age, a nobleman's groom, was in attendance on his master, in the neighbourhood of the House of Lords, on the 6th of April. He was mounted on a high-bred horse, which took fright and ran away with him, the lad not being strong enough to hold him in. The animal went at full gallop along Parliament-street and Whitehall, until it was stopped by a timber-waggon, which was turning into Scotland-yard: against this it came with full force, receiving so severe an injury, that it died within half an hour. The boy was thrown violently forwards, and his head came in contact with a piece of the timber. He was picked up and conveyed into a neighbouring house, where I saw him almost immediately. He was insensible from the shock of the injury, but could move his limbs: he was breathing naturally, and his pulse was not materially affected: he had thrown up the contents of his stomach. The upper and right part of the head was covered by a large mass of clotted blood, which I removed with my hand, and found, on turning it over, that it contained portions of brain, which might have been about sufficient to fill a dessert spoon. This had escaped through a wound of the scalp, about one-third of an inch in length, just over the coronal suture. There was a fracture of the right parietal bone, which could be traced under the integuments from the back of the bone to the situation of the wound, at

which there was a considerable and irregular depression. He was conveyed in a coach to the hospital; and a further escape of cerebral substance, to a small amount, was found to have taken place when he arrived. At the end of two hours he manifested sensibility on being pinched; he moved his limbs readily; the circulation, respiration, temperature, and colour of the skin, were natural.

As the bone was here evidently and considerably depressed, and as it was also probably driven in upon the brain, it would, I believe, have accorded with principles of treatment generally admitted, to have performed an operation for the purpose of elevating and removing depressed and detached portions of bone. The considerations which determined me not to do this were, the favourable state of the patient generally, and in particular the absence of all symptoms indicating compression of the brain; the specimens, in pathological collections, of very extensive injuries of the skull repaired by a natural process; the smallness of the external wound, which brought this case nearly into the state of simple fracture; the extensive incision of the integuments, and exposure of the bone, dura mater, and brain, which an operation would have involved; and the almost invariably fatal termination of such proceedings, within my own experience in hospital practice. In the latter respect, the chances are less unfavourable in a young than in an older subject; on the other hand, the powers of natural restoration are more vigorous in the former than in the latter. On the whole, I consider the dangers attendant on such an injury as that which befel this poor lad, to be much less than those belonging to the operation, which would have been required in this case.

A strictly antiphlogistic treatment was adopted, comprising perfect quiet, opening medicines and clysters, and tea diet. The head was shaved, and kept cool by the repeated application of cold cloths; and the wound was kept open, that blood or effused liquids might escape readily. In thirty six hours after the accident, the external senses and the mental powers were completely recovered, and speech was restored. When questioned, he complained of pain in the head, which continued more or less for about ten days, and was the principal symptom referrible to the accident. On the 9th, the neighbourhood of the wound was red, hot, and painful, and there was restlessness. The application of six leeches removed these symptoms.

The entirely favourable progress of the case seemed to justify the course which had been adopted; but new and alarming

symptoms appeared on the 11th. There was some increase of temperature in the head, and general feverishness. Convulsions came on in the face and right limbs, while the left arm and leg were completely paralysed. The question of operation was again considered. I did not refer these symptoms to pressure of the displaced bone, believing that if any effect had been produced by that cause, it would have followed the accident immediately, and I thought the present symptoms must be ascribed to the inflammation of the brain, which might be expected after such an injury. The external wound was opened and probed, and gave issue to some bloody matter. Four leeches were applied in the neighbourhood, and were followed by bread-and-water poultices. Four grains of the hydrargyrum cum cretâ were given every four hours. A blister was applied to the nape, and the vesicated surface, after removal of the cuticle, was dressed with strong mercurial ointment. The mischief was speedily and effectually arrested by these means. The convulsions which had come on in paroxysms, did not return after the bleeding. The left arm and leg remained totally powerless for forty-eight hours, and then recovered slowly, but completely. The mercury quickly affected the mouth, and lowered the pulse, which became slow and feeble, with considerable sense of weakness. It was therefore necessary to give it up entirely at the end of a week, and to allow some improvement of diet. The strength and health soon returned, and the patient became quite well, except that discharge of matter continued from the original wound, and from another small opening, which had been made a little lower down. The integuments had remained detached from the bone to a considerable extent from the time of the injury, and matter had accumulated under the loosened portion of scalp; a counter-opening had been made to allow a dependent discharge. Matter can still (June 3) be pressed out from both apertures, and a rough portion of bone can be felt with the probe introduced at the original wound. In all other respects this patient, who has for some time left his bed, is in perfect health: there is not a trace of imperfection in the senses, mental faculties, or the power of moving the limbs which had been paralysed.

T. Bennett remained some time longer in the hospital, in the expectation that a portion of bone would come away, and because a quiet mode of living, with strict attention to diet, was thought necessary until the part had become sound. No exfoliation, however, took place, and he left the hospital with the opening in the inte-

guments still occasionally discharging. He came occasionally, that we might watch the progress of the case. In about a month the wounds had firmly cicatrised, and no evidence of the injury remained, except a considerable indentation of the skull where the bone had been broken.

Cancerous Affections of the Lips.

I have the opportunity of showing you two specimens of cancerous disease of the lips, which I have removed in the last two days. One was taken from the lower lip of a patient about fifty, in whom the disease had existed five or six years, without pain or material inconvenience; having, indeed, attained only a trifling magnitude. It was seated at the boundary between the mucous membrane and the red edge of the lip, occupying an equal portion of both structures, and consisted of a small indolent induration, causing a slight swelling of circular outline, less in circumference than a shilling, with an ulceration in the centre, covered by a brown incrustation. It had commenced as an indurated tubercle, and had not been long ulcerated. This gentleman had never smoked. There was no affection of the absorbent glands. Ineffectual attempts had been made to destroy the disease by escharotics. I sliced off the morbid production by passing a double-edged knife under its basis. The diseased part presents a small mass of firm scirrhus induration; while the ulcerated portion is a broken surface, like that of some warts.

The other specimen exemplifies what is less frequent—namely, cancer of the upper lip. The patient from whom I removed it is a healthy person, near seventy, living in the country, and following the occupation of a farmer. Two years ago the disease began, as a wart on the integument of the upper lip, at least half an inch above the red portion. The hardened part ulcerated, and the complaint slowly spread downwards. There has been little or no pain, and the absorbent glands are unaffected. You see in this specimen a cancerous ulceration of the integument, about three-quarters of an inch in length, and half an inch wide. The edge is knotted, irregular, and hard, and the surface of the sore is uneven: it is bounded below by the mucous membrane of the mouth, which is sound. It produced a sparing discharge, which formed a hard adherent incrustation. I removed this disease by a semi-circular incision, in the manner recommended by the late Baron Dupuytren, which I have found to answer the purpose perfectly well in several instances of similar operations. A section of this disease shows that it is a specimen of genuine cutaneous cancer; the cutis having been

converted into a stratum of compact scirrhous structure, about a line in thickness, and the surface of the part thus altered having become ulcerated. The surrounding adipose, muscular, and mucous structures were quite healthy.

These examples show us that the disease in the lip may begin either in the common integument or in the red edge of the mucous membrane; that its characters are the same in both instances—namely, tubercular induration, proceeding to ulceration; indolent nature, slow progress, and absence of affection of the absorbent system. Disease exactly similar in nature and in progress is seen not unfrequently in the integuments of the cheek, nose, and lower eye-lid. The important difference between these several cases and cancerous affection of the mammary gland is, that the absorbents are not affected in the former, and thus that we may remove them with the knife, without any fear of those unpleasant subsequent occurrences which so frequently frustrate, in the end, our attempts at curing scirrhous disease in the breast by operation. I have removed portions of cancerous integument from various parts of the face, with complete success, in numerous instances, when the disease has been so situated as to allow the complete extirpation of the morbid structure, and the absorbent glands have been unaffected.

The latter ultimately become diseased in some cases. There are also malignant diseases of the lip, commencing in the interior, consisting of the successive occurrence of induration and ulceration, and affecting the absorbents at an earlier period. The present remarks are applied to those instances only in which the affection begins with tubercular induration of the external skin, the red edge, or the mucous membrane.

The superficial nature of the disease in these cases might induce us to attempt their destruction by powerful escharotics, such particularly as the chloride of zinc. I think the use of the knife preferable, as being more definite and certain: we remove all that ought to be taken away, and we do no more. It is, at the same time, far less painful. Some time ago I saw a lady with an affection of this character on the left temple; it was about the size of a sixpence; the mass of induration being inconsiderable, and the ulceration not deep. It was, however, very painful. Her sister had died of cancer in the breast, the disease having returned after an operation, and this had given her a great aversion to the knife. I applied the chloride of zinc, which certainly removed, both in surface and depth, fully as much as would have been taken away by the knife.

The application was followed by the most severe pain, which lasted for several days and nights. The separation of the slough was followed by healthy granulation, and an apparently sound cicatrization. Within a month, however, an indurated tubercle formed in the cicatrix, and was very painful. The patient now readily consented to the use of the knife. The part was freely removed, and there has been no recurrence to the present time, a period of more than twelve months.

Irritating and escharotic applications are not unfrequently tried to those diseases, almost invariably with the effect of inducing pain and increased activity in a complaint previously indolent and nearly stationary. I recommend to you, therefore, in the management of these cases, a choice between two courses; that of leaving things alone, or complete extirpation by the knife.

A few days ago I operated on an infant in the hospital for hare-lip. The fissure was confined to the lip; the child had thus been able to suck, and was still at the breast. The eligible time for this operation is before the first dentition, about the fourth or sixth month. The sides of the fissure were brought together with a silver pin, and the twisted suture at the red edge of the lip, and a simple suture at the upper part. I allowed the child to continue at the breast, having found, by experience, that the act of sucking does not interfere with the process of union, while it is much better for the health of the patient that it should continue to receive the wholesome nourishment provided for it by nature.

A FASTING WOMAN.

It was cried about the streets of Paris, that a woman of clear complexion and plump habit, suckling a large child six months old, had eaten nothing for a year and eight months. As she did not eat, she necessarily must have had no evacuations, and this was also asserted; so that the miracle was complete.

It was added that she had the power of living on water alone, without making the slightest effort, or even moving at all.

She was admitted into the Hôtel-Dieu on the 30th September, and is now in the St. Monica ward, No. 60, under the care of M. Magendie. Her name is Maria Rosa Felicity Hardy, and her age is thirty-one; she is a char-woman, and born at Catigny, in the department of the Aisne. She is neither fat nor lean, but, if any thing, rather inclining to the latter, and of a sanguine temperament.

For the first few days it could not be perceived that she took any nourishment.

At any rate she touched nothing that was placed near her bed, either solid or fluid; nor was any evacuation discovered.

She was then in the St. Benjamin ward, under the care of M. Caillard, who did not believe in these wonders, and thought that he should sooner or later find out that she was a trickster.

M. Caillard not being able to put this woman under lock and key in his wards, sent her into those of M. Magendie, who had a small room at his disposal quite fit for experiments of this kind. The woman was shut up with food, of which the quantity was exactly measured. She seems to have fasted for eight days, unless indeed she received nourishment secretly, and without the knowledge of the physician; but on the ninth day she ate. Faeces were found very well concealed in a mattress, which she had sewed up again. After this there was no interest in continuing the experiment; she was removed from the room, and had a bed in the ward.—*Gazette des Hôpitaux.*

PHYSICAL SOCIETY, GUY'S HOSPITAL.

Fourth Meeting, Nov. 18, 1837.

EDWARD COCK, ESQ. IN THE CHAIR.

On Normal and Abnormal Dentition in Adults.

MR. EDEN read a paper on the above subject. The first part of the paper was devoted to the description of normal dentition: the author then proceeded to the consideration of some of the most usual deformities, their causes, effects, and treatment. In alluding to the second dentition, and growth of the jaws, the author strongly insisted on the consentaneousness of these processes; he considered, however, that the roots of the temporary teeth acted as wedges to separate the permanent pulps from each other. Abnormal dentition was believed by the author to be less frequent in the country than in large towns: he was led to such opinion from a conviction that the disadvantages, arising from the artificial life, &c. adopted in populous districts, would tend to injure the general health, and thus act as a cause in producing irregularity of the teeth. Family peculiarities were as powerfully impressed on the teeth as on the other organs of the body. The author divided the irregularities of the teeth into the irregularities of growth and irregularities of disposition. With regard to the former, he referred to the papers by Dr. Ashburner, published in the *MEDICAL GAZETTE*. The most common irregularities of disposition were believed by

the author to be those in which the canines and superior incisors were principally implicated; the canine pulps being abnormally situated, or the jaw not enlarging in proportion to their growth. The treatment of abnormal dentition was entered into at some length by the author: he particularly condemned interference with the processes adopted by nature, except in cases of most absolute necessity. He concluded by saying, that it was quite impossible to lay down any general rule of guidance in the treatment of irregularities of the teeth.

A question having arisen as to the number of germs of teeth in the earliest period of foetal life, Dr. Ashburner being called upon, said, that, according to Serres, fifty-two germs of teeth existed in the foetus. Rousscan, in his *Système Dentaire*, stated that Geoffroy St. Hilaire declared the germs of all the teeth could be discovered in all animals at the earliest period of their uterine existence. These were considered by him as so many foetal organs, for which certain vessels were predestined; hence a regulation of the flow of blood through the head was procured. Dr. A. alluded to the difficulty of forming a correct standard of health. He agreed with Mr. E. as to the consentaneous growth of the teeth and jaws, but differed with him in his explanation of the progress of this development; for he did not believe that any local source of irritation, such as wedge-like pressure, was necessary to produce a perfectly healthy result; the truly healthy operations of the human body depended more upon the vitality, nervous energy, power of growth, &c. of the individual, than upon any local action: when local irritation existed, then abnormal dentition was the result.

MR. COOPER objected to Dr. Ashburner's principle of examining the whole system to account for local growths. He thought it more correct to make local examinations to explain local operations. Mr. C. referred to a preparation made by Mr. John Hunter, in the College Museum, of a foetus three or four months old, where the germs of the deciduous teeth alone existed, and occupied the whole of the jaw; there was no trace of the germs of any of the permanent teeth. These facts were further detailed in Mr. Hunter's work on the teeth; but Mr. C. said he himself had not pursued this branch of anatomy minutely.

DR. ASHBURNER called the attention of the Society to a fact mentioned by Serres, which it would be very interesting to confirm. Serres stated that there were two sets of vessels for the two dentitions: these were traceable in the lower animals. Dr. A. had seen this arrangement in four

specimens of the dry jaw, but would not undertake to say that this was the general rule.

MR. GORHAM took a more extended view of the subject than had been hitherto done. He had 300 observations to shew that the growth of the parietal bones kept pace with the progress of dentition; thus, when the anterior fontanelle was large and open, the teeth were cut late, and when the parietal bones were nearly united, the early dentition could be confidently prognosticated. Mr. Gorham endeavoured to explain this by arterial connexion.

MR. GREENWOOD thought Mr. Gorham had erred in his explanation. In weak children the fontanelles were always large, and the teeth cut late: both circumstances attributable to a general and not to a local cause.

MR. ROBERTS was of opinion that the health of the individual very much affected dentition. He mentioned the case of a child becoming hydrocephalic at the period of weaning, whose milk teeth appeared without enamel. The permanent set had only a thin coating of that structure.

DR. ASHBURNER entered at some length into a description of the process which nature adopts in absorbing the roots of the milk teeth: he compared it to the action observed in necrosis, where a sequestrum of bone becomes absorbed. He said he believed Mr. Bell thought that the growth of the jaw was partly produced by the wedging of the teeth; but he could not agree with that gentleman.

MR. COOPER, in alluding to the pathology of dentition, asked Dr. Ashburner whether he might not possibly have mistaken cause for effect. Why was it more likely that the bad state of health observed by him should be caused by the teeth, than that the irregularity of the teeth should be caused by a general affection of the system?

DR. ASHBURNER said he could not agree with Mr. Cooper, and quoted cases in support of his position. He decidedly had remarked (in many cases) the existence of general constitutional disturbance during the second dentition.

DR. ADDISON, in reply to Dr. Ashburner, declared that he had never observed general mischiefs to exist during the second dentition. He thought one source of fallacy might be detected in the Dr.'s views, viz. that during the period of second dentition the patient was at the age when scarlatina, variola, rubeola, &c. are known to occur. Dr. A. quite agreed with Mr. Greenwood, regarding Mr. Gorham's observations; it had been long known that those children who had large bregmata,

were weakly, and therefore cut their teeth late.

MR. COOPER proposed, and Dr. Addison seconded, a vote of thanks to Mr. Eden, for his valuable communication, which being unanimously carried, the meeting adjourned.

At the next meeting, December 2d, Dr. Guy in the chair, Mr. Iliff will read a paper on the difficulty of distinguishing the commencement of malignant diseases in the abdomen, illustrated by a case.

ACCIDENTS ADMITTED AT THE LONDON HOSPITAL.

Mr. SCOTT, *Surgeon*.

Mr. HAMILTON, *Assistant-Surgeon*.

Nov.	Sex.	Age.	Case.
7.	M.	14	Fractured tibia and fibula.
	M.	14	Burn from gunpowder.
	M.	50	Wounded head.
	F.	27	Sprained ankle.
8.	M.	42	Wounded head.
	M.	14	Wounded head.
	M.	36	Fractured tibia and fibula.
9.	M.	40	Fractured ribs.
	M.	20	Injured abdomen.
	M.	21	Contusions.
	F.	30	Laceration of the leg.
	M.	4	Retention of urine (<i>a.</i>)
10.	M.	46	Cut throat (attempted suicide.)
11.	M.	11	Burn from gunpowder.
	M.	38	Fractured tibia and fibula.
	M.	56	Cut head.
12.	M.	69	Hernia (reduced.)
	M.	29	Cut throat (attempted suicide.)
	M.	3	Scald (dead.)
	M.	2½	Scald.
13.	M.	44	Injured shoulder.
	M.	69	Injured side.
	M.	42	Fractured fibula.
	M.	47	Sprained ankle.
	F.	8	Burn (dead.)
	F.	67	Burn in the head (dead.)
	F.	28	Wounded leg.
	F.	72	Fractured femur.

In-patients 28

Out-patients 35

Total 63

(*a.*) This boy was admitted with incomplete retention of urine; his mother stating that only a small quantity had occasionally dribbled from him for the last four days; she attributes this to a kick the boy received from a cow. On introducing a small catheter it was distinctly felt to grate over a small calculus, near the mem-

braneous portion of the urethra. The water was drawn off three times a day, and on the 16th the urethra was cut down upon, and two very small calculi removed. The case is doing well.

CHAIR OF ANATOMY—UNIVERSITY OF DUBLIN.

(From a Correspondent.)

ON Monday the 6th instant, Dr. Harrison, Professor of Anatomy and Surgery in the University of Dublin, commenced the course of lectures on Comparative Anatomy, to which the public have free access; the professors being remunerated for these, and the public lectures on chemistry, at the expense of the University. The theatre was crowded to suffocation.

Previously to commencing his course, the Professor, after a well-deserved compliment to his predecessor, Dr. Macartney, made some forcible remarks on the importance of both general and professional education, as preliminary to medical graduation. After describing the advantages connected with the Irish School of Physic—the anatomical opportunities—the University anatomical and chemical establishments—the botanical garden—and the splendid hospital connected with the school, and founded by Sir Patrick Dun—the Professor observed that the University of Dublin was the only one in the United Kingdom where medical education was *duly* combined with study in arts; it being in the power of those candidates who chose to unite them, to obtain a full and recognised degree in arts, and also that of Bachelor in Medicine, at the commencement in immediate succession, or in little more than four years from entering the University.

The public courses of anatomy and chemistry have continued to attract a crowded auditory.

DR. A. T. THOMSON'S CONSPECTUS OF THE PHARMACOPŒIAS.

To the Editor of the Medical Gazette.

SIR,

HAVING been induced, by an advertisement on the cover of your journal, to purchase Dr. A. T. Thomson's tenth edition of his *Conspectus of the Pharmacopœias*, professing to contain all the *alterations and additions* of the last London Pharmacopœia, I was disappointed in finding, in such a useful little manual for the table of the practitioner, so many omissions of important articles in daily use in modern practice, and which have a place in the

London Pharmacopœia. In my cursory survey of the work, I find *Ergota* and *Strychnia* altogether omitted, and *Creosote* and *Veratria* only mentioned among the poisons; the former as an *acid poison*, and the latter as an *acro-narcotic poison*, without the least reference or direction about them in a medicinal view. *Morphia* and its preparations are placed among the *useful remedies* employed in the *United States and France*, having, by inference, no place in the London Pharmacopœia. *Carbo Animalis*, *Porcira*, *Potassa*, *Bromidum*, and *Potassa Ferro Cyanidum*, all articles in the London Pharmacopœia, are wholly omitted. The book is also dedicated to the Junior Members of the Profession, under the date of March, 1837. To me, an elder member of the profession, such omissions matter little; but when put into the hands of those for whom it is designed, and especially into those of the student on his way to the Hall or the College, these *corrigenda* are culpable, and the advertisement is fallacious.—I am, yours, &c.

MEDICUS.

Nov. 8th, 1837.

[The above is from the pen of an eminent provincial physician, who sends his name. We have farther satisfied ourselves, by a reference to the "*Conspectus*," that the omissions to which he alludes actually exist.—ED. GAZ.]

UNIVERSITY COLLEGE HOSPITAL.

FATAL BAYONET WOUND.

To the Editor of the Medical Gazette.

SIR,

A JOURNAL, notorious for its mendacity, having made some very false statements respecting the case of a man who lately died in University College Hospital, from the effects of a bayonet wound, I judged it necessary to advert to this subject in the lecture on Punctured Wounds, delivered by me, at University College, last night.

As several of my friends think that the observations which I there made ought to be rendered still more public, inasmuch as they afford a complete exposure of the wilful misrepresentations resorted to by the Editor of that publication, as well as of his disgraceful ignorance of practical surgery, I have decided to comply with their advice. The remarks were completely extemporaneous, and I have no copy of them at present; but I will prepare one, and transmit it to you in the course of two or three days.—I am, sir,

Your obedient servant,

S. COOPER.

Woburn Place, Russell Square,
November 22, 1837.

OF

DRUGS ON SALE IN THE ENGLISH MARKET,

With their Prices and several Duties.

(From the Official Returns up to Tuesday, November 21, 1837.)

		PRICE.			DUTY.			DUTY PAID	
		£	s.	d.	£	s.	d.	In 1837: 1 last week	Same time last year.
Aloes, Barbadoes, D.P.	c	12	0	0	to 30	0	0	104,952	101,637
Hepatic (dry) BD.	c	5	0	0	14	0	0		
Cape, BD.	c	2	0	0	2	12	0		
Aniseed, Oil of, German, D.P.	lb	0	9	6	0	9	6		
E. I.	lb	0	7	0	0	7	6		
Assafoetida, B.D.	c	0	2	10	0	5	0		
Balsam, Canada, D.P.	lb	0	1	3	0	1	4		
Copaiba, BD.	lb	0	3	9	—	—	—		
Peru, BD.	lb	0	4	3	—	—	—		
Benzoin (best) BD.	c	25	0	0	50	0	0		
Camphor, unrefined, BD.	c	9	0	0	—	—	—		
Cantharides, D.P.	lb	0	5	2	—	—	—		
Carraway, Oil of, D.P.	lb	0	9	0	—	—	—		
Cascarilla or Eleutheria Bark, D.P.	lb	1	15	0	—	—	—		
Cassia, Oil of, BD.	lb	0	7	0	—	—	—		
Castor Oil, East India, BD.	lb	0	0	6	0	0	10		
West I. (bottle) D.P. 1½ lb	lb	0	2	3	—	—	—		
Castoreum, American	lb	1	15	0	—	—	—		
D.P. Hudson's Bay	lb	1	0	0	1	4	0		
Russian	lb	none							
Catechu, BD.	c	1	2	0	—	—	—		
Cinchona Bark, Pale (Crown)	lb	0	2	0	0	3	6		
BD. Red	lb	0	3	0	0	6	0		
Yellow	lb	0	2	0	—	—	—		
Colocynth, Turkey	lb	0	2	6	0	4	0		
D.P. Mogadore	lb	0	3	0	—	—	—		
Calumba Root, BD.	c	1	4	0	2	5	0		
Cubebs, BD.	c	3	0	0	—	—	—		
Gamboge, BD.	c	5	0	0	15	0	0		
Gentian, D.P.	c	1	4	0	—	—	—		
Guaiacum, D.P.	lb	0	1	0	0	1	8		
Gum Arabic, Turkey, fine, D.P.	c	8	0	0	9	0	0		
Do. seconds, D.P.	c	5	0	0	7	0	0		
Barbary, brown, BD.	c	3	0	0	3	3	0		
Do. white, D.P.	c	4	15	0	—	—	—		
E. I. fine yellow, BD.	c	3	0	0	3	10	0		
Do. dark brown, B.D.	c	1	15	0	2	5	0		
Senegal garblings, D.P.	c	4	15	0	5	0	0		
Tragacanth, D.P.	c	8	0	0	12	0	0		
Iceland Moss (Lichen), D.P.	lb	0	0	2½	0	0	3		
Ipecacuanha Root, B.D.	lb	0	1	9	0	2	0		
Jalap, BD.	lb	0	1	10	—	—	—		
Manna, flaky, BD.	lb	0	4	0	0	5	6		
Sicilian, BD.	lb	0	1	7	—	—	—		
Musk, China, BD.	oz	1	0	0	1	8	0		
Myrrh, East India, BD.	c	5	0	0	14	0	0		
Turkey, BD.	c	2	0	0	11	10	0		
Nux Vomica, BD.	lb	0	8	0	0	9	0		
Opium, Turkey, BD.	lb	0	12	6	0	13	0		
Peppermint, Oil of, F. BD.	lb	1	1	0	—	—	—		
Quicksilver, BD.	lb	0	3	6	—	—	—		
Rhubarb, East India, BD.	lb	0	2	6	0	3	3		
Dutch, trimmed, D.P.	lb	0	3	6	0	4	0		
Russian, BD.	lb	0	8	3	—	—	—		
Saffron, French, BD.	lb	0	18	0	0	19	0		
Spanish	lb	0	19	6	—	—	—		
Sarsaparilla, Honduras, BD.	lb	0	1	0	0	1	9		
Lisbon, BD.	lb	0	2	0	—	—	—		
Scammony, Smyrna, D.P.	lb	—	—	—	—	—	—		
Aleppo	lb	0	12	0	0	15	0		
Senna, East India, BD.	lb	0	0	3	0	0	4		
Alexandria, D.P.	lb	0	1	6	—	—	—		
Smyrna, D.P.	lb	0	1	0	0	1	3		
Tripoli, D.P.	lb	0	1	0	0	1	3		

‡‡‡ BD. In Bond. — c. Cwt. — B. P. British Possessions. — F. Foreign. — D. P. Duty paid.

DR. JAMES FORBES.

DIED on the 7th instant, in Maddox-street, in his 59th year, Dr. James Forbes, Inspector-General of Army Hospitals. On his return last year from Ceylon, where he had served with great credit as Deputy Inspector-General, he was promoted to the important appointment of Inspector General of the Hospitals in the King's Army on the peninsula of India. Dr. Forbes was a man of high talent, a distinguished medical officer of the army, and enjoyed in a singular degree the regard and respect of all the officers, medical and military, with whom he had served in almost every quarter of the world.

NEW MEDICAL BOOKS.

J. A. Welsh's Treatise on Ringworm, 8vo. 5s. 6d.

G. T. Morgan's First Principles of Surgery, Part II. 8vo. 5s.

Changes produced in the Nervous System by Civilization, by R. Verity, M.D. 12mo. 4s.

Surgical Observations on Tumors, by S. C. Warren, 21s.

M. W. Hilles' Treatise on Hernia, 12mo. 2s.

Obstetric Plates, by W. Smellie, 8vo. 5s. The Use of Auscultation and Percussion, by D. J. Wolff, M.D. 8vo. 6s.

The Anatomical Remembrancer and Pocket Anatomist, 32mo. 3s. 6d.

Practical Surgery, by R. Liston, with 120 engravings, 8vo. 22s.

Classification of the Insane, by M. Allen, M.D. 8vo. 6s.

The Domestic Medical Pocket-Book, by W. H. Kittoe, 12mo. 10s. 6d.

RECEIVED FOR REVIEW.

The Student's Pocket Manual of Osteology. By F. Winslow.

Changes produced in the Nervous System by Civilization. By Robert Verity, M.D.

Obstetric Plates, with Explanations selected from the Anatomical Tables of William Smellie, M.D.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, Nov. 23, 1837.

Henry Hawkins Bowling.—Edward Knowles, Soham.—Thomas Whately, Bedford.—John Cass Smart, Hutton Bushel.—Richard Jones, Newtown, Montgomeryshire.—Alexander Alfred Augustus Harvey, Bath.—John Fortune, Edinburgh.—Richard Milnes Pigot, Nottingham.—Thomas Fitzpatrick.—Frederick Theed, Hutton.

WEEKLY ACCOUNT OF BURIALS.

From BILLS OF MORTALITY, Nov. 21, 1837.

Abscess	3	Hooping Cough . . .	7
Age and Debility . .	43	Inflammation . . .	21
Apoplexy	4	Bowels & Stomach . .	7
Asthma	11	Brain	6
Childbirth	3	Lungs and Pleura . .	12
Consumption	39	Influenza	1
Convulsions	23	Insanity	1
Croup	1	Liver, diseased . . .	1
Dentition or Teething	8	Measles	15
Diarrhœa	2	Mortification	2
Dropsy	11	Paralysis	2
Dropsy in the Brain .	8	Scrofula	2
Erysipelas	1	Small-pox	3
Fever	18	Tumor	1
Fever, Scarlet	5	Unknown Causes . .	37
Fever, Typhus	6		
Hæmorrhage	1	Casualties	6
Heart, diseased . . .	3		

Increase of Burials, as compared with the preceding week . . . } 3

METEOROLOGICAL JOURNAL.

Oct.	THERMOMETER.	BAROMETER.
Thursday . . 9	from *21 to 45	30.07 to 29.95
Friday . . . 10	*41 55	29.91 to 29.85
Saturday . . 11	43 54	29.84 to 29.79
Sunday . . . 12	33 45	29.89 to 30.04
Monday . . . 13	29 47	30.04 to 29.82
Tuesday . . . 14	40 47	29.53 to 29.45
Wednesday 15	30 45	29.53 to 29.37

Prevailing wind, S.W.

Except the 11th, 12th, and 15th, generally cloudy, with frequent rain.

* The great change in the temperature on the 9th and 10th, is worthy of particular remark.

Rain fallen, .325 of an inch.

Aurora Borealis.—Twice during the past week the Aurora has been remarkably brilliant; first on the evening of the 12th, from 5 till after 10 o'clock, when the coruscations, though few, were vivid, and of a deeply red colour. Again, on the evening of the 15th, about 7, when it was even more splendid; the coruscations were white, and also more numerous, vivid, and extensive, and apparently based upon a deep crimson ground. About 8 in the evening of the 12th, a brilliant meteor passed through Ursa Major.

Thursday . . 16	from 26 to 39	29.93 to 29.87
Friday . . . 17	22 40	29.88 to 29.96
Saturday . . 18	21 39	30.01 to 29.96
Sunday . . . 19	30 53	29.94 to 29.79
Monday . . . 20	35 47	29.80 to 29.75
Tuesday . . 21	29 47	29.73 to 29.86
Wednesday 22	33 55	29.86 to 29.82

Prevailing wind, N.E.

Except the 18th, 19th, and 22d, generally clear, with rain.

Rain fallen, .1625 of an inch.

CHARLES HENRY ADAMS.

ERRATA.

In Dr. Philip's paper in our last number, line 7 from the bottom of the last note in page 285, for "they," read "ganglionic nerves;" also, page 287, 2d column, line 30, for "organ," read "organs;" and in page 290, 1st column, line 14 from bottom, for "their insensibility," read "insensibility of the vital organs of the brain and spinal marrow."

NOTICE.—Medicus next week.

WILSON & SON, Printers, 57, Skinner-st., London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, DECEMBER 2, 1837.

LECTURES
ON THE
PHYSIOLOGY AND DISEASES OF
THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL
AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of
1836 and 1837,

At the Anatomical School, Kinnerton-Street,
near St George's Hospital,

By CHARLES J. B. WILLIAMS, M.D. F.R.S.

LECTURE VIII.

Physical Examination of the Chest (concluded) — Methods of Auscultation — Directions for Percussion — Pleximeter — Immediate Auscultation — Advantages of Mediate Auscultation — The Stethoscope, its Principles and Construction — Different kinds of Stethoscope — Directions for using the Stethoscope.

WE are to be occupied to-day, gentlemen, in considering the *methods* of physical examination, or the means which we use to obtain a cognizance of those physical phenomena which we have found to be signs of the condition of the organs within the chest. We have already described the methods of examining the chest by sight, touch, and measurement, and we shall have now only to demonstrate them practically on the living subject. But we have to say something on the methods of hearing, or *auscultation*, and the best means of obtaining the signs which it affords.

In getting the sounds of percussion you will always hold in mind the principles on which they depend; and when you have to compare those of one part with those of another, you will take care that the parts are struck alike, and that the pleximeter,

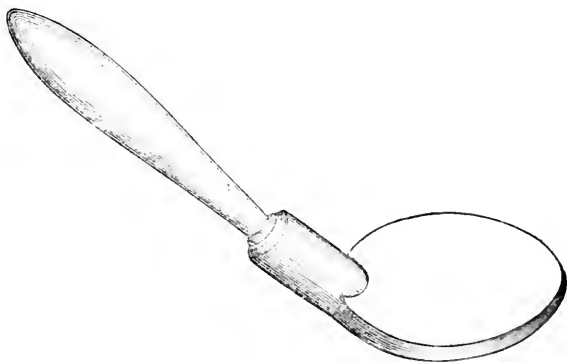
or fingers, on which you strike, are applied with an equal degree of pressure, and to corresponding parts. Thus, in comparing the two sides in doubtful cases, when you strike on one side on an intercostal space, take care that you strike on the other side on the corresponding intercostal space, and not on a rib. And when you strike on the clavicle remark well on what part of it you strike; for the sternal portion of this bone always sounds much clearer than the humeral end. So also in percussing this or any other part, let the direction of your stroke be perpendicular towards the lungs, and not sideways, or you will get the sound modified, not by the lungs, but by the adjacent muscles or other parts towards which the impulse is directed. It is from neglecting this precaution that beginners sometimes get nothing but dull sounds all over the chest. It is not generally necessary to use much force in percussion; in fact, many of the most valuable results are obtained by gentle mediate percussion in the manner which I before described; but you will remember, from the principles then laid down, that varying the force will give you different results, which in some cases may be usefully consulted. I do not give you any absolute rule whether to percuss with the ends of the fingers, or their palmar surfaces, because both methods are useful in different cases. So also where you want to test the density of a small spot, percussion with a single finger is best; whilst for trying a surface of greater extent, flat percussion with several answers better. In doubtful cases it may be useful to try both. In percussing the regions of the back and shoulders, seek the bony prominences of the scapulæ and ribs; for these transmit the impulse to the interior far better than the thick layers of muscles do. But as the scapulæ are moveable bones, it is quite necessary to see that they are in corresponding places on both sides; and to en-

sure this, and to increase the tension of the muscles, it is well, in examining these regions, to desire the patient to cross his arms in front, and bow his head forwards. At that part of the chest near the humeral end of the clavicle, a most important region for examination, there is often a falling away of the chest; and the more tense the pectoral muscles are made, the further they are removed from its walls: here, then, instead of making the muscles tense, they must be relaxed, by letting the elbow hang close to the side whilst mediate percussion is practised in this region. There are several other little details with regard to the practice of percussion, that you will soon learn by experience, now that you know the principles.

The best posture of the patient for percussion is erect or sitting; and in comparing the two sides both before and behind, you should place yourself right opposite to him, and right behind him. When the patient is lying down, the sound is modified by the matter on which he is lying; if it be a soft feather-bed the sound is more dull, if a mattress or any thing harder, the sound will often be increased, because the elasticity of the contents of the chest is increased by the unyielding matter behind it. Never, if you can avoid it, examine a patient in a very empty room, in the corner of a room, or very near a wall, for the sounds are singularly modified by the reverberation of flat surfaces, and the side nearest the wall will

consequently give too loud a sound. You may in some measure remedy this by drawing a curtain against the wall, which diminishes the reverberation: and this is another thing to be attended to, that drapery hanging near the patient will diminish the length of the sound. As most of the effects of percussion are judged by comparison, the chief object of the cautions which I have been giving you is to take care that any of the external causes of modification may not act unequally on the different parts of the chest. If you can, remove the object of your examination beyond their sphere, by percussing him standing in the middle of the room; but as you cannot always do this, try to equalize as much as possible the modifying circumstances, so as not to affect one part more than another, and you may still get good comparative results.

I do not say much about pleximeters, because I really do not think them generally necessary. If, however, you have a mind to be very well furnished, I recommend you one of this form and size; it is made of boxwood, and the upper surface of its little plate is covered with soft leather to prevent the clacking sound of the fingers. The handle, rising from its rim at an angle of about 45 degrees, is particularly convenient for holding it firm to the chest, without interfering with the percussing fingers. The chief precaution necessary in using it is, to take care that it be applied flat, and not tilted.



Let us now study the best methods of listening to the audible signs of the motions, the *dynam-acoustics*, of the chest.

All these signs can be heard by the direct application of the ear to the chest; and this immediate method of auscultation is so easy and simple that it commends itself strongly to us, and is in many cases used with great advantage. The

sounds proceeding from the walls of the chest are communicated to the ear, and especially to the air contained in the external meatus, and are thus propagated in the most direct and unmodified manner to the organ of hearing. Immediate auscultation is exclusively practised by some, both at home and abroad; and as it is much more easily learnt than the mediate

method, I have no doubt that it will always have its advocates among those who prefer ease to exactness. You will ask me, what is the use of the stethoscope, if we can hear the signs so well without it?

I will tell you first some positive objections to immediate auscultation; and on examining the principles of the stethoscope you will find that it has considerable advantages in many cases. To apply your ear, and consequently your nose, face, and so forth, to the chest of a patient who is dirty, or wet with perspiration, you will allow is not pleasant. To apply it to the chest of a patient labouring under an infectious disorder, is neither pleasant nor safe. To apply it to the person of a young female, whether pleasant or not, would certainly not be proper, and perhaps not always safe. Then, you cannot well apply the ear to some parts of the chest, such as the arm-pit, and below the clavicles, or between the scapulæ, in thin persons. Besides this, you will sometimes have disturbing noises from the contact of your hair or your clothes with the patient's chest; and unless your neck be pretty long and flexible, you will find this *easy* method after all more irksome and fatiguing than the mediate method. Still, in a great many instances, it may be used with advantage, especially in examining the regions of the back, and in young children, where the stethoscope might cause alarm, and could not be steadily applied.

We want an instrument, then, to transfer the sounds from the chest to our ear; and what sort of an instrument should this be? It must be a good conductor of sound; and as we found formerly that the power of bodies to conduct sound will depend on the strength and uniformity of their elasticity, and their capacity to vibrate like the body which communicates the sound, we must have an elastic material, of density resembling that of the sources of the sound within the chest, and of the walls of the chest through which they are transmitted. But the sources of the pectoral sounds vary; some, as the voice and the respiration, or at least the hollower sounds of respiration, are produced in air, whilst in others, such as the sonorous rhonchus, and the sounds of the heart, which we have yet to study, the solids are chiefly concerned: we shall therefore need a varied capacity in our instruments to receive these different sounds. It may perhaps occur to you that as all these sounds have to pass through the solid walls of the chest, their differences must here cease, and their character being identified by being made vibrations of solids, the instrument need only be adapted to these solids. But this view is not quite correct; for the thin and yielding walls

of the chest do not considerably modify the character of the vibrations which they receive. As we found by percussion that they derive the character of their vibrations from the nature of the material which lies under them, so when this material originates a sound, the walls vibrating with it do not materially change its nature, or the manner in which it may be communicated to other bodies. It is thus that one parchment head of a drum transmits its vibrations through the air rather than through the solids to the other head, which, in its turn, although a solid, yet freely transmits its motions to the air. In fact, solids, when thin, instead of vibrating only according to their own molecular elasticity, are carried or moved by the vibrations of the media on each side of them, and their vibrations partake of the character of these media. This is the reason why you can hear voices easily through the thin partition of a room; the sound passes from the air to the partition, and from the partition to the air, and you would not hear any better by substituting for one portion of air a conductor of the same density as the partition itself. I could bring many other illustrations of the same point, but I think I have said enough to shew why our instruments for conducting sounds from the chest to the ear should be constructed with regard rather to the origin of those sounds than to the density of the solid walls through which they pass.

To transmit the sounds originating in the solids within the chest, we must have a uniform solid, and the lighter it is the better, provided it be thoroughly rigid. Now nothing answers to this description so well as wood; and in the light kinds of wood with a stiff longitudinal fibre, such as pine wood, cedar, deal, and the like, we find these qualities in perfection. We formerly heard how well a long rod of deal answered as a conductor of the vibrations of a steel tuning fork. Chladni found by experiment that it is scarcely inferior to steel, glass, and other much denser bodies; whilst it has this advantage over them, that being so much lighter it can be much more readily affected by the vibrations of lighter bodies. Here is a cylinder of deal, about eight inches long and an inch and a half in diameter, adapted to the ear at one end, and through it you may hear most of the pectoral sounds; but those best which originate in the solids, such as the sounds of the heart, of friction, and sonorous rhonchi. You also hear the sounds of respiration and of the voice, but by no means so distinctly as you hear them with the naked ear.

But we want an aerial conductor for these sounds, because they originate in air, and can be best transferred through air. Well,

perforate this cylinder with a bore one quarter of an inch in diameter, so as to make a tube of it; if you then apply it you will have a column of air inclosed in the wood, and now you will find that you have the respiration and the voice sounding much more distinctly through it. But as this column of air is in contact with only a small spot of the chest, it can transmit only the sounds produced under or very near that spot, and you have thus an instrument better for exploring small parts of the chest than the ear can be.

We want, however, also, an instrument which will transfer to us the sounds of larger spaces: the sounds of so limited a spot are often too weak to be heard alone; and besides, it would be very tedious to go all over the chest, *dotting* in this way a quarter of an inch at a time. Well, then, let us enlarge our column of air at the base where it is in contact with the chest, by hollowing out the wooden cylinder into a funnel shape, as you see it in this instrument; and by this you will hear the pectoral sounds much more distinctly than with either the solid or the merely perforated cylinder. Need I explain why? The sounds produced under the whole space covered by the instrument are reflected by the funnel into the central bore, and thus conveyed concentrated to the ear. This also gives the instrument the power of concentrating or magnifying the sounds; and thus you may get them as strong at a distance of several inches, or even a foot or two from the chest, as they are to the ear in close contact with it; nay, in some cases, they are even stronger. But here we have another property required in our instrument, that of reflecting all the aerial sounds which have to be concentrated into the central perforation; and we must think of the shape and material that will do this best. As to the shape, you can easily perceive that a funnel-shaped or long conical cavity, with its apex terminating in the central bore, is the best, because it turns the sound at once in a right direction, without the need of repeated reflections. With regard to the material, we may find bodies that reflect aerial sounds better than wood; such are the metals, glass, porcelain, &c.; and if we wanted particularly to separate this class of sounds from others, these materials would answer best. I have found a stethoscope made with a hollow cone of tin plate or brass answer very well. But these materials are too dense to receive readily the weaker vibrations of the solids of the chest, which are much better transmitted by wood; and wood, although not the best reflector of air-sounds, is quite good enough; for what it does not reflect it transmits: and it matters little whether the sounds all reach the ear by the air, or

partly by the air and partly by the wood, so long as they reach it together, which for such short distances they will do. And this leads me to notice another advantage which light rigid wood has over other kinds of material. I told you in my lecture on sound, that the sounding-boards of musical instruments are made of this substance, because its rigidity enables it to receive the finest vibrations of denser bodies, and its lightness renders these vibrations extensive, and capable of impressing a large extent of air, and you saw this illustrated by experiment. Thus it becomes the means of transferring the vibrations from solids to air; and I may now add, the same property also enables it readily to receive vibrations from air. Hence, with a stethoscope of light rigid wood, all kinds of sound are pretty freely transmitted both by the sides of the tube and by the column of air within; and any stray reflections that fall too perpendicularly to be concentrated into the central canal, enter the solid, and are passed on by it; instead of, as in long metallic tubes, being reflected repeatedly from side to side, until they are converted into an echo, which by its tinkle would confuse the original sound.

You see, then, that light rigid wood is the best material for a *stethoscope* or chest-explorer; and the best shape is a perforated cylinder, hollowed at the chest end into a conical cavity, and the other end made flat or slightly concave to fit to the ear. But we still want sometimes to explore small spots of the chest; and for this purpose we fill up the cavity by a conical perforated plug, which reconverts the instrument into a simply perforated cylinder, which transmits sounds unconcentrated. To make this instrument more portable, the upper part of the cylinder may be reduced to a stem half an inch or less in diameter, leaving only at the top a sufficient width for the ear; or this top may be made separate of a harder wood to slip on and off. Wood is so excellent a conductor of sound, that when once the vibrations are in it, they can be conveyed by a very small body of fibres.

You see, then, that the stethoscope, although a simple instrument, comprehends a good many qualities; the chief of which I will recapitulate.

1. To conduct sound by its solid walls.
2. To conduct and concentrate sound by its closed column of air.
3. To transfer sounds from its column of air to its solid walls, or the converse, when circumstances impede their transmission by one of these ways.
4. To diminish this power of transfer, and contract the field of hearing when small spots are to be explored.

You will find a full description of the

stethoscope, illustrated by a plate, in my work on Diseases of the Chest.

If you are not provided with a stethoscope I should recommend you to get one from a turner of the name of Grounbridge, at 42, Poland street. He knows my rules for the construction of these instruments, and executes them well. Here are a great many stethoscopes of all sorts and sizes for your inspection. Do you see this clumsy-looking cylinder, made of walnut painted black? Do not despise it, for it was not only made by Laennec, but used by him for several years: and by its means he discovered many of those valuable signs which I rejoice to have to explain to you. With this other mahogany cylinder I began the study of auscultation fifteen years ago; it has done me some service; and I have no small regard for it. The only other instrument that I shall notice, is this flexible one, which is like a common flexible ear-trumpet, only shorter, and with a rather larger tube. As you may suppose, from its variable shape, it does not convey the sounds of the chest so simply or so distinctly as the wooden stethoscope; yet when you become accustomed to it, you can really hear pretty well with it, and its flexibility gives it a great advantage in many cases. It conveys sounds chiefly by the closed column of air which it contains; and the manner in which sound is reflected in curves, as I before explained, enables it to carry the sounds easily through its windings. Yet if you try this experiment carefully, you will find that you always get the sounds more distinctly by keeping the tube as nearly straight as possible. It is particularly useful for separating the sounds of the heart from their accompanying impulse, as I shall explain hereafter.

Before we proceed to our practical demonstrations on the living model, I must say a few words on the method of using the stethoscope. It is quite necessary that the instrument should be applied in close contact with the chest and the ear; the least tilting uncloses the column of air, and occasions great loss of sound outwardly, as well as a confusing entrance of extraneous noises. To prevent this tilting, it is best to hold the stethoscope by its pectoral end firm on its base, and then to apply the ear flat to the top. If the inequalities of the ribs leave apertures between the chest and the instrument, a fold or two of linen will obliterate these, or the instrument may be used with the stopper in. For the sounds generally it is best to use the concentrating instrument without the stopper; but when it is an object to determine whether a sound is produced in a limited space or over some extent of surface, the circumscribing power of the stopper is wanted. Thus it is often of im-

portance to determine whether a vocal resonance is produced in a small cavity, or merely transmitted by consolidated lung from several bronchial tubes distributed over some extent of surface. The simply perforated cylinder will often do this by showing the size and shape of the limited spot in which the resonance or pectoriloquy of a cavity can be heard in its full strength, while the bronchophonic resonance is transmitted less strongly, and may be traced over some extent of surface, generally in the known direction of these tubes. The stopper is useful also in shutting out the sound of respiration when it is an object to listen to the sound of the heart or arteries, and in many other circumstances which we shall notice as we go through the diseases which produce them.

In conducting your examinations, try as much as possible to avoid fatiguing or annoying your patient. There are cases in which a complete physical examination will do more harm than the information which it may convey can do good; but they are few; and I must leave it to you to hold the balance between too much and too little examination. You will soon in practice find out, too, that your own ease must be attended to in the act of auscultation; for a constrained or painful posture will prevent you from hearing well, and from attending well to what you hear. For this reason you may sometimes hear better with the flexible ear-tube than with the straight stethoscope, although the latter is, for general purposes, the best instrument.

[The remainder of the lecture was occupied in practical illustrations of the methods of the physical examination of the chest.]

LECTURES

ON

RHEUMATISM AND ITS EFFECTS;

By R. MACLEOD, M.D.

Physician to St. George's Hospital.

LECTURE V.

Pericarditis very rare in Synovial Rheumatism, which, however, may be transferred to the Membrane of the Chest or Head by Metastasis—Cases in illustration—the Eye sometimes affected, but not nearly so often as writers on ophthalmic surgery would lead us to suppose—other Complications which are described in books extremely rare—Tables and Calculations, shewing the Ages, Duration of Treatment, and other circumstances, connected with Acute, Chronic, and Synovial Rheumatism.

It is a remarkable fact with respect to this form of rheumatism—the synovial—that it has very little disposition to implicate the

heart. Of a large number of cases of rheumatic pericarditis which have fallen under my observation, only one has occurred where the synovial membranes had been the seat of the disease; and, on the other hand, in five cases of synovial rheumatism wherein patients have died from the transference of the inflammation to internal parts, the heart has manifested neither the slightest symptoms of disease during life, nor the least appearance of morbid change on examination after death. But I do not wish to draw from this any other conclusion than that such complication must be comparatively rare; and this inference, I may add, is strengthened by a reference to recorded cases; for where they are related with sufficient distinctness to enable us to determine the nature of the previous rheumatism, it will be found almost invariably that if the heart became involved in the disease, the synovial membranes had not been the seat of the primary attack.

Indeed, there can be no doubt that, on the great scale, there is a striking difference between the affections of internal organs in rheumatic fever, and in cases of synovial rheumatism, even in its acutest form. While the former implicates both the external and internal membranes of the heart by *extension*, and this in a very large number of cases, the latter (where it involves an internal part) becomes transferred to the pleura, or to the membranes of the brain, by *metastasis*, and this in but a small proportion of instances. Nor is the immediate result of such internal inflammations less remarkable; for while very few, if properly treated, die during the first onset of rheumatism of the heart, the mortality holds a very high ratio where the synovial inflammation becomes transferred to the serous membranes either of the head or chest.

There are too many instances on record to admit of the fact being questioned, that synovial rheumatism is occasionally associated with an inflammatory affection of the membranes of the brain; but there are various circumstances connected with such occurrence which require particular attention. So far as my experience goes, this combination is much less apt to attend the early than the advanced stage of this form of rheumatism; whereas I think the heart affection is most frequent at the early period of rheumatic fever. Again, the cases which I have seen have not been when the disease has been erratic as regards the limbs, but rather when it has fixed itself in a few joints, or perhaps in one particular joint: such joint suffers severely, and is generally considerably swollen, from effusion within it. After having been for several weeks, or even months, nearly stationary, the swelling may diminish without any obvious cause; the patient having either ceased to take

any remedies, or continuing to use those which had previously appeared to relieve, perhaps, but not to produce any striking effect upon the local disease. Wherever, under such circumstances, any (even the slightest) head-symptom presents itself, it must be viewed with the greatest suspicion.

Sometimes the affection of the brain is manifested by pain in the head, which is at one time acute, at another dull—at one time persistent, and another periodic, or at least paroxysmal, with almost complete remissions. Where there is pain in the head, however slight, attention is generally directed to the part; but occasionally there is no pain whatever, even although the disease has made considerable advances.

The following case may serve to illustrate the peculiarities of one form of this disease:—

A gentleman of literary habits, in the 37th year of his age, had suffered for above two months from pain, with swelling and fluctuation, of several joints, particularly the knees. The right knee was more especially complained of, and there was considerable effusion into the joint, evinced by a soft fluctuating projection on either side of the patella and across the lower part of the thigh, just above the knee. He took acetic extract of colchicum, in doses of gr. iij., and acetate of morphia, which last was increased gradually from $\frac{1}{4}$ to $\frac{1}{2}$ a grain; but without any effect in relieving the affected part. About the end of May, 1837, he complained of his memory being impaired, so that he had great difficulty in remembering words; but he was able to go out in an open carriage, and took a short airing on Sunday, April 2d.

Next day I found him in bed, unable to answer questions otherwise than by the monosyllables "Yes," and "No." He expressed his mortification at this inability to speak, by gestures, and when asked if he had any pain or giddiness about the head, answered "No," and shook his head in such manner as to show that he perfectly understood what was said to him. His pulse was 78, and soft; his tongue clean. No complaint was made of the knee, but on examining it I found the swelling almost entirely gone; indeed all fluctuation had disappeared, and there remained only some puffiness about the parts. He was freely purged, and a mustard poultice applied to the knee.

4th.—The same symptoms continued without perceptible change. Twelve leeches were applied to the temples, and the purging repeated.

5th.—In the early part of the day he seemed better; recollecting his sister's

name, which he had previously forgotten. Late at night was seized with a fit of screaming, accompanied by strabismus affecting the right eye, and followed by frequent moaning.

Leeches to forehead, and gr. ii. of Calomel every three hours. A blister to the back of the neck.

6th.—Strabismus gone, but mouth perceptibly, though slightly, drawn to right side, with much subsultus tendinum, and frequent sighing. Answers questions which require only monosyllables distinctly; says "No," when he is asked if he has pain in the head, and "Yes," when asked if he has any giddiness. Pulse 80, with some sharpness.

Twelve leeches to temples; Calomel, gr. iii. every three hours; strong mercurial ointment to be rubbed in very freely.

7th.—Very little change. Calomel omitted after the seventh dose, in consequence of purging. Mercurial frictions continued every two hours.

8th.—Much more sensible, and expression improved. Urine, which has been rather scanty, has become more abundant. Pulse 78. No mercurial fœtor, but a perceptible red line on the gums.

9th.—Continues apparently to improve; gums decidedly injected, but no mercurial fœtor. Takes beef-tea plentifully. Shows by his manner that he recognizes those about him, but cannot name even those most familiar to him.

Towards evening he began to sink. Frequent starting, and constant deep sighing; pulse becoming weaker, and rising in frequency to 100. These symptoms continued till the evening of the 10th, when he expired.

Autopsy.—The convolutions of the brain were flatter than usual; the arachnoid was injected, and there was a slight appearance of effusion. The ventricles contained from ʒiiss. to ʒij. of clear limpid serum, and each *corpus striatum* had the appearance of being smeared over with a thin layer like cream; but this could not be wiped off, and apparently depended on thickening and opacity of the lining membrane. The convolutions of the left fissura Sylvii were adherent, from inflammation of the interposed membrane, and the substance of the brain on either side of the fissure was of a yellowish colour, with patches like minute points of extravasation. The heart and other thoracic viscera were perfectly healthy. In the right knee, which had been the chief seat of the rheumatism, the membrane was thickened, rugose, and red all round, close up to the cartilages. The membrane had been pushed upwards, so as to increase the extent of the cavity,

but no longer contained any preternatural quantity of synovia*.

Now this is a case to which I have seen several more or less analogous, and evidently consisted in a slow and treacherous inflammation about the brain, producing fatal effects without exciting the general system into any of that sympathy which usually attends the progress of mortal diseases.

In another form of this metastasis the symptoms are more marked, especially as regards the occurrence of pain in the head, by which the seat of attack is clearly pointed out. A case of this kind lately occurred at St. George's Hospital, under the care of Dr. Seymour. A man had long laboured under synovial rheumatism, and there was great permanent distension of the capsules. One day the knees, which had been greatly swollen, were observed to be flaccid—reduced, in fact, by about one-half. He complained at the same time of pain in the head, became paralysed on one side, and expired in the space of 36 hours. On opening the body, a deposit of greenish-looking purulent matter was found smeared over the greater part of the surface of the left hemisphere, and there was considerable effusion into the ventricles. Here the disease had gone on silently up to a certain point, as there was not the slightest evidence of any increased action about the head till the paralysis came on. In yet other cases of this affection, there is pain of the head of several days' duration, afterwards giving place to symptoms of pressure, very much in the manner we see in hydrocephalus. A very good illustration of this is contained in Sir B. Brodie's work on the Joints.

A lad, aged 13, had inflammation of one knee, with effusion into the joint; thickening of the synovial membrane resulted, and the enlargement of the joint continued for several months. At the end of this time the swelling became suddenly reduced, and this was simultaneously accompanied by the supervention of acute pain in the head. This returned several times periodically at night, and after a week entirely ceased, when he became affected with somnolency, strabismus, and partial blindness, in which state he died.

In almost every instance wherein the cerebral affection assumes the insidious form described in the first case, the paralytic attack detailed in the second, or the form of hydrocephalus, as in the third, the attack proves fatal. But in some the disease more resembles common inflammation of the brain, admits of the ordinary active treatment, and more frequently than in the preceding instances also admits of a successful termination.

* This case was attended by Mr. Pereira, Dr. Clendinning, Dr. Hope, and myself.

Dr. Copland has recorded a case in which rheumatism was connected with inflammation of the membranes of the spinal cord; but as in the case alluded to the rheumatism alternated with *chorea*, there is at least room to suspect that it was this last, rather than the rheumatism, which directed the disease to that portion of the nervous system.

Another complication of this form of rheumatism is with pleurisy; and here, too, the internal inflammation depends upon a metastasis, the affection of the joints subsiding, or wholly ceasing, on the supervention of the thoracic disease. I have more than once witnessed this occurrence, and the phenomena were rather remarkable, having given rise to a copious sero purulent effusion into the bag of the pleura. This form of metastasis is quite distinct from that which takes place to the pericardium in diffuse rheumatism. In this last the inflammation may, and frequently does, extend over the covering of the heart to the contiguous pleura; but in the cases of metastasis of synovial rheumatism to which I allude, the pericardium was free from any participation in the disease.

Another part to which rheumatism occasionally extends is the eye; and I mention this here, because I have never seen it except in connexion with the synovial form of the disease. Certainly it is not common for patients labouring under any form of rheumatism, whether acute or chronic, to become affected with ophthalmia while under treatment; and it appears by the best works on diseases of the eye, that the cases which have been called rheumatic ophthalmia are principally instances of inflammation of the sclerotic coat or conjunctiva, in persons who have had no other form of rheumatism. The application of cold and damp, which in one person produces rheumatism, in a second may produce cynanche, in a third bronchitis, in a fourth ophthalmia; and if the latter be called rheumatic, I see no reason why we should not also speak of rheumatic cynanche, rheumatic bronchitis, &c. In order to prove that one disease is of a specific character, and similar to another, we ought to be able to shew, either that they occur together so frequently as to leave no reasonable doubt of their identity—which is the case in rheumatic fever and pericarditis—or that the sudden retrocession of the disease in the extremities is followed by the internal affection, as where synovial rheumatism is transferred to the brain; or, lastly, the internal affection, if it manifest itself first, ought to become mitigated, or to cease entirely, on the disease attacking the limbs—a phenomenon frequently witnessed in gout.

Now if these tests be applied to rheu-

matic ophthalmia, I have no hesitation in expressing my conviction, that the number of cases in which it is met with will be prodigiously reduced; in fact, of nearly 200 patients whom I have treated for different forms of rheumatism in St. George's Hospital, during the last four years, only one has had inflammation of the eye; and I observe that, in a table of 520 cases, given by Haygarth, the eye was only affected in two.

The case above alluded to happens to be, at present, in St. George's Hospital, in a state of convalescence; and it is remarkable that he has suffered, at several different times, from synovial rheumatism, and at each time has had ophthalmia. I would by no means be understood, therefore, as denying the existence of rheumatic, or rather perhaps of arthritic ophthalmia. All I maintain is, that the affection bears a very small proportion to the total number of cases, and that the term has been unwarrantably extended by writers on ophthalmic surgery, to inflammations of the eye having no connexion of any kind with rheumatism except that of being produced by the same exciting cause, viz., cold.

We read in medical authors of various other parts becoming affected with rheumatism by metastasis. One would expect such affections to be more common, in connexion with this, than the other forms of the disease, because it bears a very strong affinity to gout; and gout is proverbial for its erratic propensities. At the same time I must remark, that I have been looking out for such examples during twenty years, at the Westminster Dispensary and St. George's Hospital, and have never met with any unequivocal instance of the metastasis or extension of rheumatism except those I have mentioned above. We have abundant derangement of function in the alimentary canal and kidneys, but as out of many hundred cases, I have never seen rheumatism, when unconnected with gout, transferred to the stomach or the bowels, the kidneys or the bladder, in the shape of inflammation, I think I am warranted in inferring that such cases are of very rare occurrence.

No inconsiderable portion of the subject still remains undiscussed; but the very limited time allowed to these lectures has expired, and I must necessarily leave it incomplete. The Gulstonian lectures* are, or ought to be, connected with the subject of anatomy; and I have therefore considered it my business to dwell especially upon the effects produced by rheumatism, on the various structures which it attacks.

* It may be proper to state, that this and the preceding, though for convenience divided into five portions, constituted the Gulstonian Lectures, limited to three in number, delivered at the College of Physicians last summer.

Had time served, I should, in speaking of the treatment, have ventured to urge the advantage of general bloodletting and purging in the early stage of acute rheumatism, carried to a considerably greater extent than is usually done. Indeed, it is the only method by which I have ever seen the disease extinguished, like a common inflammation.

It would have been an interesting question to have examined the circumstances which give efficiency to particular remedies—stimulant diaphoretics, warm baths, and strong exercise, in some; narcotics, such as stramonium and aconite, in others. To have traced the specific influence of colchicum, where the synovial membrane is implicated, as well as the superiority here of local over general depletion—the use of mercury in the diffuse or fibrous form, its failure in the synovial—and the very remarkable effects sometimes produced by the hydriodate of potass, more especially in almost all those cases where there has been any thickening of the affected textures.

Such are a few of the practical parts which it was my intention to have discussed; as it is, I must content myself with adducing a short tabular view of the cases I have most recently treated, and from which the opinions above delivered have been in great measure deduced. In its present state, however, it is only offered as a unit towards forming a sum from which general calculations may be made at some future time.

Tables and Inferences drawn from 150 Cases of Acute, Chronic, and Capsular Rheumatism, treated at St. George's Hospital during a period immediately anterior to the delivery of the preceding lectures.

Cases of acute rheumatism, }
(Males, 55; Females, 30) } 85

Ages.	Males.	Females.	Total.
10 to 15	1	0	1
15 to 20	9	10	19
20 to 25	9	4	13
25 to 30	15	7	22
30 to 35	8	3	11
35 to 40	8	0	8
40 to 45	3	2	5
45 to 50	1	0	1
50 to 55	0	2	2
55 to 60	1	2	3

From this table it would appear that in both sexes from 15 to 30 is the time of life most obnoxious to acute rheumatism, 54 out of 85 cases having occurred within the period specified.

Periods during which the Cases of Acute Rheumatism were under treatment.

Cured.	Died.
In 5 days 1	Of pericarditis, 2; viz. at the end of three days, 1; at the end of three months, of a second attack, 1.
In 8 days 6	
In 10 days 4	
In 12 days 4	
In 2 weeks .. 7	
In 3 weeks .. 22	[Date of discharge not recorded in 3]
In 4 weeks .. 14	
In 5 weeks .. 3	
In 6 weeks .. 9	
In 8 weeks .. 6	
In 10 weeks .. 1	
In 16 weeks .. 2	

It thus appears that 58 out of 82 cases were discharged cured within a month; and when it is kept in mind that it is the custom of the hospital not to send out patients who have had acute rheumatism as soon as they cease to complain of pain, we may safely assert that the number specified were cured in little if at all exceeding three weeks. But 44, or rather more than one-half, were actually discharged within three weeks; and certainly the great majority of these were free from complaint at the end of a fortnight.

In a small number, perhaps about one-eighth of the whole, the disease was absolutely cut short at once, within two, three, or four days; and in two or three cases it was (even though accompanied by much swelling and redness of the joints) arrested within twenty-four hours. To effect this, however, various concurrent circumstances were required—viz. a constitution previously healthy, a very recent attack, and active depletion.

Of the 85 cases of acute rheumatism above alluded to, the heart was implicated in 18, or rather more than one-fifth.

But of the total number of patients, 55 were males, and only 30 females. Now of the men not more than seven had pericarditis, whereas of the women, eleven, or about one in three, had symptoms indicating that condition.

Of the eighteen cases in which the heart was affected, there was only one in which the pain of the limbs was simultaneously so alleviated as to give the least colouring to the idea of metastasis. In seventeen of the cases alluded to, the heart affection was preceded by rheumatism of the limbs; in one case the pericarditis came on first, and the rheumatism of the extremities subsequently, but within twenty-four hours.

The occurrence of inflammation about

the heart always added to the duration of the case. Thus, of the forty-four discharged within three weeks, none had pericarditis; but of the fourteen who were not discharged till the end of the fourth week, not fewer than five had had the heart affected; and this rule, that they who had such complication required to be kept longer than others in the hospital, held good almost uniformly. Thus of those who were discharged at the end of six weeks, amounting to nine, four had had pericarditis; of those who were eight weeks in the hospital, amounting to six, three had pericarditis. The one who remained ten weeks, and both those who remained sixteen weeks, had rheumatic affections of the heart. The fact of this complication being chiefly in those who remained longest in the hospital must not, however, be understood as implying that the pericarditis supervened chiefly after a certain residence in the hospital; on the contrary, in the great majority of the cases the rheumatism attacked the heart at an early period; while in about one-third of the number the symptoms of heart affection were fully developed at the time of their admission. Again, the prolonged stay of such cases in hospital is in great measure owing to this, that I invariably make it a rule not to discharge them until I have expended every effort to promote the reabsorption of any effused lymph, and to render the action of the heart as quiet as possible. Unless this be done, the damage goes on increasing very rapidly when the patients return to their active avocations.

The following are the ages of those who had rheumatic pericarditis:—

Age.	Males.	Females.	Total.
10 to 15	1	0	1
15 to 20	2	4	6
20 to 25	2	4	6
25 to 30	1	2	3
30 to 35	1	0	1
55 to 60	0	1	1
	7	11	18

Very few children are admitted into St. George's Hospital; but from what I have seen in dispensary and private practice, I am inclined to believe that the proportion of cases of rheumatic pericarditis is considerably greater in young subjects than in those of more advanced years. Of those referred to in the preceding table of cases of acute rheumatism,

but one was under 15 years, and he had pericarditis. Of cases in subjects between 15 and 20, there were twenty; and of them six had pericarditis. Between 20 and 25 there were thirteen; and of them six had pericarditis. After this the proportion diminishes, for while there were twenty-two cases of acute rheumatism between the ages of 25 and 30, yet they give but three cases of pericarditis; 30 to 35 give ten cases of rheumatism, and one of pericarditis; 35 to 40 give eight cases of rheumatism, and none of heart affection.

Cases of chronic rheumatism, } 42
(Males, 26; Females, 16) }

Ages.	Males.	Females.	Total.
15 to 20	2	1	3
20 to 25	2	2	4
25 to 30	4	4	8
30 to 35	3	2	5
35 to 40	3	2	5
40 to 45	1	2	3
45 to 50	5	1	6
50 to 55	2	2	4
55 to 60	1	0	1
60 to 65	1	0	1
65 to 70	2	0	2

So far as this table goes, it would seem to show a remarkable difference between the ages at which acute and chronic rheumatism are most prevalent. In the former instance, from 15 to 30 gave about two-thirds of the entire number of cases; here the same period of life gives only 15 out of 42, or little more than one-third. After the middle period of life, the tendency to acute rheumatism progressively and rapidly diminishes; not so with regard to the chronic form of the disease, which is more equally spread over the period of adult age. Of the acute cases, only 3 occurred between the ages of 45 and 55, or about 1-28th of the whole number; whereas the same period gives 10 cases of the chronic form, being about 1-4th of the whole.

Periods during which Cases of Chronic Rheumatism were under treatment.

1 week	1
2 do.	7
3 do.	7
4 do.	12
6 do.	10
8 do.	4

Date of discharge not recorded 1

In about 3 4ths of the above, the pain was entirely, or almost entirely, removed; in the remaining fourth the disease was

so far mitigated as to admit of the parties being made out-patients.

Cases of *Synovial Rheumatism* } 23
(Males, 16; Females, 7)

Ages.	Males.	Females.	Total.
15 to 20	1	0	1
20 to 25	1	1	2
25 to 30	2	1	3
30 to 35	3	2	5
35 to 40	1	1	2
30 to 45	2	1	3
45 to 50	3	0	3
50 to 55	2	0	2
55 to 60	1	0	1
60 to 65	0	1	1

According to this table, capsular rheumatism would seem to be considerably more prevalent among men than women, and much more equally diffused over different periods of adult age, than the form of the disease first described.

Periods during which the Cases of Capsular Rheumatism were under Treatment.

3 weeks	3
4 do.	3
6 do.	4
7 do.	2
8 do.	5
10 do.	2
3 months	2
4 do.	1
6 do.	1

The most remarkable circumstance in this table is the length of time during which the patients were under treatment, as compared to the other forms of the disease. Of the cases of acute rheumatism, as we have seen, more than one-half were discharged within three weeks; whereas here the number discharged in the same period scarcely exceeded 1-8th. Again, of the acute cases, less than 1 9th remained so long as two months in the hospital; whereas, of the synovial cases, within a fraction of one-half were in the hospital two months and upwards.

In one of the cases of capsular rheumatism, inflammation of the eye supervened, and it is worthy of remark that the same patient, in more than one previous attack, had suffered from the same complication.

In one case (a woman, aged 45), there were symptoms indicating a slight attack of pericarditis, which, however, was speedily arrested by the ordinary remedies.

Of the preceding cases, three were fatal, —viz. one from suppuration in the joints at the end of two months; one from pleu-

risy, at the end of seven weeks; and one from inflammation of the encephalon, at the end of three months.

The cases of periosteal rheumatism which have occurred within the period to which the above relates, amounts to 10; and those of arthritic or rheumatic pain, following the course of particular nerves (all of the lower extremities), to 7; — numbers too small on which to hazard any general inferences.

REMARKS

ON THE

EXTRACT OF BELLADONNA

GIVEN AS A PROPHYLACTIC AGAINST THE
CONTAGION OF SCARLET FEVER.

By WILLIAM MACLURE, Esq.

Surgeon; late President of the Harveian Society.

To the Editor of the Medical Gazette.

SIR,

THE following paper was read by me before the Harveian Society, at its sitting of the 7th of October, 1833. It relates to the subject of the Extract of Belladonna given as a preservative against the contagion of scarlet fever. If you should deem it of sufficient importance to entitle it to insertion in your journal, be so good as to award to it that privilege at your earliest convenience. I have delayed to publish it till now, in the hope of enlarging my experience of the practice which it recommends, and I have only farther to remark, that since the paper was written, I have employed the extract of belladonna as a prophylactic in several instances, and generally with apparent success; though no doubt the subject still requires investigation. The great importance of the subject, and the probable efficacy of the practice, were fully admitted by the late Sir David Barry, Professor A. T. Thomson, Dr. Theophilus Thompson, and several others of equal eminence, who took part in the discussion.

I remain, sir,

Your obedient servant,

WM. MACLURE.

14, Harley Street,
10th Nov. 1837.

I beg to offer to the Society some remarks on the Extract of Belladonna employed as a Prophylactic against Scarlet Fever.

Hahnemann, I believe, was the first

who proposed giving the extract of belladonna to persons exposed to the contagion of scarlatina, as a prophylactic against that disease. I am told that he published his views on this subject so long ago as 1801, but I have not been able hitherto to procure a sight of his work; what he said, however, with regard to the preventive efficacy of this substance against the exanthym in question, does not seem to have obtained much attention in Germany at the time, for I do not find any other author taking up the subject till Dr. Dusterburg did so in 1820*. In Hufeland's *Journal der Practischer Heilkunde* of that year, Dr. Dusterburg relates that during the epidemic prevalence of scarlet fever at Gutersloh, or Gusterslob, in 1820, he gave daily to such children as had not been attacked, from ten to twenty drops of a solution of three grains of extract of belladonna, in three drachms of canella water: and he assures us that none of the children who continued this medicine a week were attacked with the disease, though continually exposed to its contagion. It is also stated, that every child that did not take belladonna, and was exposed to the contagion, had scarlet fever. From this period it would appear that the subject has attracted a good deal of attention in Germany, and a great many physicians of that country have given belladonna a trial, and have reported favourably of its effects, in the way of shielding for a time the constitution against this often formidable disorder. The celebrated Hufeland, in a work written in German, "On the Preservative virtue of Belladonna against Scarlatina," and published at Berlin in 1826, has collected no less than thirty reports of as many German physicians, whose experience goes to confirm his own opinion of the efficacy of this substance in this particular way. The Professor believes that the efficacy of the plant depends upon its power of diminishing the nervous susceptibility of the human system, so as to render it for a time insusceptible of impression from the contagion†.

M. Martini, in a paper published in the 2d Volume, page 371, of the "*Révue Médicale*," also avows his belief in the preservative virtue of this substance:

and M. Ibrelisle, a physician of Metz, who writes in the "*Bull. de la Société d'Emulation*," for April 1823, p. 201, informs us that he has seen twelve children preserved by the belladonna from scarlatina, which had attacked two hundred and six others, in the midst of whom the twelve protected ones resided. Dr. Velsen, likewise, gave the substance in question to two hundred and forty-seven persons, of whom thirty-one only contracted the disease, although they were all equally exposed to its contagion. This physician prescribed two grains of the extract dissolved in two ounces of water combined with a little alcohol; and of this he ordered fifteen or twenty drops to be taken by each person daily. He remarks with justice that the employment of this means being attended with no danger, there can be little or no inconvenience in giving it a fair trial. In the "*Journal des Progrès des Sciences Médicales*," Vol. 1, p. 242, it is stated by Dr. Wagner, as the result of his experience, that comparing those who took the belladonna during the prevalence of an epidemic, with those who neglected to do so, of the former he lost only about *one* in *seventy*, whilst of the latter there died *one* in *three*. He states also that whole villages in Germany have been preserved from scarlet fever by the inhabitants taking the belladonna whilst the disease was raging in one at no great distance from them. I must confess, however, that I do not place much reliance upon the somewhat vague statements and loose experience of the last writer; but there is something more definite and conclusive, as well as more recent, in what M. Scoutetten has reported as being the experience of M. Friso, of Scierck, in his "*Compte rendu des travaux de la Société des Sciences Médicales du Département de la Moselle*," published at Metz, in 1830. M. Friso, who observed an epidemic of scarlet fever in the village of Reimling, canton of Scierck, and arrondissement of Thionville, during the months of December 1828, and January 1829, derived, he relates, great advantage from the belladonna as a preservative means. The mortality here was very great, since out of about one hundred patients, no less than eighty-three died. The extract of belladonna was given to twenty-two individuals, who, although they both

* Gooch's Study of Medicine, vol. iii. page 20. Cooper's Edition.

† *Gazette de Santé*, Mars 1826.

resided and slept with those affected with the disease, were not attacked. Four children, he continues, living in two houses where there were persons labouring under the malady, did not take the belladonna; whilst in one of these houses, three others took it regularly every day. The four children who had not the benefit of the prophylactic became affected with scarlatina, and what is very remarkable, not one of the other three, though they were equally exposed to the contagion, became ill." "I could," adds this author, "cite many other similar examples which were followed with the same results; and now I never hesitate, in my rounds from house to house, to carry with me my remedy, being well assured that I can always arrest the epidemic." "At this date," (23d Jan. 1829.) he adds, "I have no more patients with scarlatina, and recently I have only seen one individual die of it, and this through the fault of his parents, who obstinately refused all aid whether of a preservative or a curative nature*." In addition to these authorities on this interesting subject, it would be wrong not to add that of Dr. Randhaken, physician to the Orphan Hospital of Langendorf in Prussia, as quoted by Dr. A. T. Thomson†, who affirms that by the same means he exempted from the disease in question, one hundred and sixty children who were exposed to its contagion.

These authorities, it will be observed, are all foreign: indeed they all have a German origin. I have not been able to find that any trial has been given to the alleged preservative efficacy of the belladonna in this country; nor, indeed, in the course of my reading have I observed that the subject has been mentioned at all by any English writer, except in a short notice by Mr. Samuel Cooper, in his edition of Dr. Good's Study of Medicine, already quoted; and in another by our learned associate, Professor Thomson, in his Elements of Materia Medica, though from its manifest importance, not only in a scientific but practical point of view, to schools and families, it certainly deserves a calm examination, and ought to be submitted to further experiment, not only in scarlatina, but perhaps also in the other contagious exanthemata.

I have to lament not only this lukewarmness and neglect on the part of British practitioners, but also that my own experience on the subject is as yet extremely limited. Indeed, what I have observed of the prophylactic power of the belladonna would perhaps not deserve any attention, if it stood alone; but when taken in conjunction with the facts detailed by our continental brethren, it may be considered to have a value which otherwise might not belong to it. Such as it is, however, I now beg leave to submit it to the Society's consideration.

On the 4th day of July last (1833), being the second day of her illness, I was called to visit a young lady whom I soon discovered to be labouring under scarlet fever in rather a severe form; sloughing throat, typhoid fever, and delirium, being among the symptoms. She was attended from the beginning by her mother, a hired nurse, her mother's maid, her own maid, and a house-maid, in all five persons, none of whom, I was told, had previously passed through the disease. The patient's brothers and sisters, at my suggestion and request, left the house on the second day of the complaint; her father remained, but did not, during the whole course of the disease, ascend higher in the house than the drawing-room floor, and the men-servants and others remained below, not being permitted to have any immediate communication with the sick-chamber, which of course was on the bed-room floor. By these early precautions all the male individuals of the house escaped.

Recollecting the passage which I have quoted from the paper of Dr. Dusterburg, in Hufeland's Journal, I resolved on testing the prophylactic powers of the belladonna in the persons of my patient's attendants, thinking, with Dr. Velsen, that if it did no good, it could not do any harm, if given in minute doses and with proper precautions. In accordance with this idea, then, from the beginning to the end of my attendance, two of the maids whom I have mentioned, viz. the lady's-maid and the house-maid, regularly took the medicine in the dose directed by Dr. Dusterburg. The event was, in their cases, that, though they never had had the disease, and though they were as thoroughly exposed to the contagion as the other attendants, they completely escaped. Not so with regard to their

* Revue Medicale, 1830, tom. iv. p. 343. as quoted in the "Dict. Univ. de Med." tom. i.
† Elements of Materia Medica.

mistress and the other attendants. The hired nurse, who was a stout healthy woman, and little more than thirty years of age, in consequence, I suppose, of that faith in her good fortune which had carried her through so many dangers, and which is often so useful a quality in such persons, refused to take the belladonna. The apparent consequence of her refusal was, that in a few days after her attendance had commenced, she became affected with the disease, and being obliged to relinquish her charge, she was sent to St. George's Hospital (whence she had been sent to us) through the kind intervention of Mr. Keate.

Again, my patient's mother, who was most assiduous and exemplary in her attentions to her daughter, hoped to escape it, (though she never had had the disease) chiefly, I believe, in consequence of her age, which, I think, was betwixt fifty and sixty; and she also declined availing herself of the medicine. The apparent consequence here again was, that just eight days after the commencement of her daughter's illness, she, too, became affected with the symptoms of the disorder, and was obliged to give up her affectionate attendance, and be attended to herself.

The lady's maid (one of those who took the medicine regularly) during the course of her mistress's illness, was constantly with her night and day, doing every necessary office about the sick-chamber. She, as I said before, escaped the disease, though so much exposed to the contagion. Lastly, the young lady's maid, who happened to be a sort of fatalist in her philosophical opinions, and professed to believe that nothing could prevent her taking the disease if it was fore-ordained by Providence that she should have it, (forgetting that the *means* as well as the *end* are in all cases fixed and foreseen by the Deity) though she reluctantly took the belladonna occasionally, in partial obedience to my prescription and her mistress's injunctions, yet she did not use it with that regularity which seems to be necessary to its full success; for during the convalescence of the two ladies, whilst desquamation of the cuticle was going on in both cases, she was seized with fever and sore throat in a mild degree, with the usual white aphthous crusts upon the tonsils, which are characteristic of an incipient attack of scarlet fever. Dr. Chambers, who was in attendance

with me on the other cases, saw this patient, and agreed with me in thinking that the sore throat was that of scarlatina, and that the young woman would have the disease, though probably in a very mild form. At the period, however, when the eruption ought to have made its appearance on the surface, none disclosed itself, and none ever did appear; and there was no subsequent desquamation of the cuticle, except a very little from one of her thumbs; just enough, perhaps, along with the incipient symptoms, to prove the identity of the complaint. She recovered rapidly.

The following summary will place before the Society at one view the results of this experiment:

1st, The mother and the sick nurse who took no belladonna, and remained unprotected by it, both took the disease, the one severely, the other mildly.

2d, The young lady's maid, who took the medicine *partially*, seemed to remain only *partially* protected. She became affected with the first symptoms of the complaint, but in her case it seemed to be so modified and restrained as not to be able fully to develop itself.

3d, The two young women who began to take the belladonna as soon as it was prescribed, and continued so doing throughout their attendance, (which I may remark was extended to all the three patients) passed through the ordeal unaffected by it.

I require not to be told how fallacious very often such experience in medicine is; and it will be very easy for those gentlemen who may take a part in this discussion to give a plausible (and perhaps true) explanation of the various phenomena which I have detailed, upon the principle of the various predispositions and susceptibilities of the persons who were the subjects of the experiment, to this particular malady; yet, notwithstanding the force of this argument—strong, if my experience stood alone and unsupported—I cannot help considering the result, when viewed as confirmatory of the more extensive experience of our medical brethren in Germany, as very interesting, and fully to justify me in bringing the subject before this Society.

The dose of the medicine which I employed was twenty drops every night of a solution of eight grains of the extract of belladonna in an ounce of dill-water.

I did not perceive that the belladonna

in these cases produced its usual effects on the pupils; nor even "des coliques, de la diarrhœa, des sueurs, et des urines abondantes," which a writer in the *Dictionnaire Universelle de Matière Médicale* is disposed to attribute its prophylactic effects to; but in one of the cases some degree of head-ache and giddiness was complained of, which was probably caused by the narcotic.

Neither did any artificial eruptive disease, similar or analogous to the scarlet fever, and to the occurrence of which, Dr. Hahnemann, upon homœopathic principles, attributes the prophylactic effects of belladonna against the real disease, make its appearance in any of these cases; so that the belladonna, if it really had any effect at all, produced that effect silently and secretly, without evidencing its operation by any outward or visible sign. It would appear, however, from Dr. A. T. Thomson's remarks in the work I formerly alluded to, that this substance really has the effect, in some instances, of producing a scarlet efflorescence on the surface of the body, as stated by Hahnemann; for he states that he has given it as a remedy in hooping cough, and that while the efflorescence continues, the cough remains absent; but that it returns as soon as the redness, with the other symptoms, disappears. But the numerous German authorities whom I have quoted make no mention of the efflorescence being necessary to the protecting efficacy of the belladonna; and, therefore, we may presume that all that is required for the alleged effect is a sufficient influence being exerted upon the nervous system by the narcotic principle of the plant, as shall render it unsusceptible for a time of being affected by the contagious *effluvia*.

REMARKS ON RHEUMATISM.

To the Editor of the Medical Gazette.

SIR,

IN the year 1835, you published a paper entitled "Pathological Remarks on the Diseases of the Encephalon,"—a subject which had occupied my attention from January to December, 1834. I

now offer to your consideration an "Essay on Rheumatism," the materials of which have been gradually accumulating from the summer of 1835 to the autumn of 1837.

In this essay, a survey has been taken of the disease; but its natural history alone is the subject of the following pages. I decline entering on the treatment: 1stly, because it is not intended for the public, whose appetite for wonderful cures incites them to extol him who promises the most; and 2dly, because it is addressed to the profession, who, as a collective body, are the ablest and only judges of what is true in the history of disease. The history of its treatment opens a wide field; and that alone which relates to gastric and intestinal irritation, conjoined with irregularity in the distribution of the blood, is a subject which would afford materials for an essay of itself, as it offers a point of practical interest, involving the considerations of many principles both in pathology and therapeutics. But it is to be remembered, as a general remark, that right treatment can depend only in right notions of disease.

I am, Sir,

Your obedient servant,

J. A. HINGESTON.

48, Finsbury Circus,
Nov. 6, 1837.

The elementary causes of rheumatism are to be sought for, 1st, in the capillary circulation, and 2d, in the mucous surfaces.

1. *Of the Circulating System.*—Irregularity in the distribution of the blood through the abdominal viscera, the brain, or the spinal column, reflects a similar irregularity on the capillary system, and forms the ground-work of rheumatism. This error of the circulation is induced by fatigue carried to exhaustion; and the rheumatism, which may take place, is that of the large muscles, and not of the joints. Muscular rheumatism seems to have its rise in errors of the circulation through the liver—a tardy return of the venous blood from the abdomen; or more often from arterial injection of the brain or spinal marrow (their substance, or more probably their meninges) whereby there are manifested exalted nervous energy and erroneous local sensation. The cerebral disturbance is subacute and

marked; and depraved nervous sensation is the consequence.

2. *Of the mucous Surfaces.*—Irritation, excitement, subacute inflammation, (by whatever word or phrase we denote the disturbance of the mucous surface) of the stomach and intestines, operates in causing rheumatism of the joints rather than of the muscles.

Intestinal irritation is always connected with, and is caused by, or is dependent on, hepatic derangement—there being not merely venous congestion of the liver, but derangement of the biliary function as a marked and essential feature; and it is also connected with erroneous excretion from the kidneys, the urine either being pale and copious, or deficient, with a deposit of the lithates.

This intestinal irritation is induced by excess of nourishment and the habitual use of stimulants; likewise by inattention to the alvine excretion, allowing the colon to become and remain loaded; and especially by over-exertion and consequent exhaustion of the brain, whereby the stomach is deprived of its proper supply of nervous energy: in which state an attempt is made to revive the stomach by stimulants; but the stimulants cause excitement, with subsequent exhaustion and final disturbance of the mucous surface.

This form of mucous irritation is presented to us in the shape of subacute inflammation (gastritis), and is to be recognised by its usual symptoms as set forth in systematic works. This form, likewise, enters largely into the history of many maladies at their inceptive stage—of fever or indigestion, of visceral inflammations or cutaneous eruptions: it is at the root of rheumatic inflammation, whether acute or chronic.

But the mucous surfaces sympathise particularly, 1st, with the skin, and 2d, with the heart.

1. With the skin the closest sympathy subsists, and is the most visible in those whose skins perspire the most readily. Such persons are the most susceptible of rheumatic attacks; viz. those whose skins and mucous surfaces are excitable and prone to excess. The functions of these two surfaces are reciprocal, and the reciprocity between the skin and kidneys is familiar: so that, the skin, the mucous surface of the alimentary canal, and the kidneys, form a united chain in the causes and cen

operations of rheumatism. Cold suddenly applied to the skin, the changes of the weather, the decline from summer to autumn, are outward causes, operating on the sympathetic chain already noted. It most commonly happens that the mucous surface is already in a state of derangement or excitement when suddenly exposed to the effects of atmospheric changes, and is thereby rendered apt to evince its own derangement by disturbance of some remote organ or texture.

In common medical conversation too much is imputed to the liver, which has borne the burden that ought to be thrown, in part, on the mucous surfaces as well as on the liver itself, and frequently on the mucous surface of the stomach alone. The liver becomes involved secondarily: for the mucous surface of the stomach itself is never deranged long together, without at length implicating the hepatic function, and as soon as the liver is disturbed the stomach rights itself, and leaves the derangement of the liver prominent and alone. What are called "bilious attacks" begin generally in subacute gastritis. The mucous surface, thus subacutely inflamed, generates acid; slight rheumatism is caused by an acid state of the gastric fluid, and cured at once by neutralizing the acid. Whatever diet causes the gastric fluid to become acid, does, in some persons, induce rheumatism.

Rheumatism and gastric irritation co-exist and synebronise.

2. With the heart the sympathy is most immediate. The heart sympathises with irritation or excitement of the mucous surface of the alimentary canal more quickly than it does with the excitement or derangement of any other structure whatever, except the mucous surface of the air-tubes. The mode by which the heart makes known this sympathy, is by its acting irregularly, and distributing the current of blood through the capillaries with irregularity, or if the capillaries be endowed with independent vitality and action, then the circulation is irregular while it traverses these small vessels: but, according to either explanation, the current of the blood flows with irregular velocity. This is a pathological connexion between the heart and arteries, and the mucous surfaces.

Thus we see that the mucous surface of the stomach and intestines holds an important post in the functions of health, and that its disturbance, conjoined with that of the heart and arteries as above noted, forms the platform and elementary frame-work of rheumatism.

To describe the functions of health, we have only to regard the established truths of physiology. The functions of the stomach and intestines, of the brain and spinal marrow, of the heart and arteries, and of the skin, are among these established truths. On the contrary, to describe the erroneous functions of disease, we must watch and observe those errors at the bed-side, and carefully note their respective departure from the natural standard of health, and their sympathy, mutual correspondence, and inter-dependence with the one upon the other. Few, if any, diseases attack one organ without involving the integrity of other organs; and this connexion of one organ with another is different in different diseases, though in each disease that connexion, to whatever extent it may proceed, constitutes at once the respective base and outline of the disease itself. This connexion is to be learned from bed-side practice. The moving circular chain of diseased action stops at the point of death; and the dead-house reveals the still-life, not the movements of the living being—the last impression, not the train of successive operations by which the impression was fixed. There are many symptoms of disease which perish long before the hour of death; fleeting ailments, transient interruptions to life, which the physician is bound to understand and relieve upon the spot, but with which the morbid anatomist can have no concern.

In the foregoing preamble, I have endeavoured to confine my pen to the known truths of physiology and the conventional language of modern pathology. A time may yet arrive when the pathology of the present day shall be as dark to others yet to come, as the infantile pathology of Sydenham is now to us. But in our progress through the wilderness, we must be content to use the language of the natives, as we pass.

Rheumatism has shewn itself to me in three forms:—1st, adventitious rheumatism; 2d, rheumatism of the joints; and 3d, rheumatism of the muscles.

There are several other shapes in which it is met with: such as, senile rheumatism, mercurial rheumatism, &c.; but each of these will be found to include itself under one or other of the divisions above noted.

1. *Adventitious rheumatism* is that form which is met with of an uncertain and passive kind. There is a wandering and rheumatic pain of the large muscles, or of the joints, or of the loins; sometimes slight in degree, and sometimes sharp and disabling, especially in the muscles of the back. It gradually increases from a slight pain to rheumatism of some severity, and is always connected with derangement of the chylopoietic viscera. There are languor, hypochondriasis in the male, and hysteria in the female; indigestion, costiveness, a loaded tongue, and turbid urine. This form is very common, and during sudden changes of the atmosphere presents itself daily to the notice of the practitioner.

It is likewise induced by modes of life. The close, heated counting-house; the sitting upright on a stool from morning to night; the want of sunshine and the breath of heaven; each of them tend to mar the perfect aeration of the blood through the lungs, and to obstruct the return of the carbonized blood through the vena portæ system of vessels; hereby causing (among other evils) perpetual derangement of the chylopoietic viscera, and teasing rheumatic pains. The gay modes of life, in an opulent city, are not less deleterious. The late dinners and the late hour of retiring to rest; the daily use of wine, or of several wines, at one meal; the hot, close, lighted apartments; and the exhalation of the animal spirits, at midnight, when the body ought to be in bed and asleep, equally shake the tripod of life, namely, the brain, the heart, and the lungs, as well as derange the mucous surface of the bowels. Many diseases, besides rheumatism, are traceable to the well-furnished dinner-table: arterial injection of the membranes of the brain, visceral inflammations, bronchial turgescence, defective excretion of the kidneys, and the manifold forms of dyspepsia.

Simplicity of diet, however, is not always to be found apart from a metropolitan life—I have seen those from the country who suffer from similar evils; they exhibit the yellow conjunctiva, the engorgement of the abdominal viscera,

the excess of nutrition, and the rheumatism of the shoulder and loins.

Fixed and chronic rheumatic pains of certain parts, as well as muscular pains of a more transient character, are indicative of visceral organic disease, or of severe functional disorder:—

1. Every one knows that rheumatism of the right shoulder is a prominent symptom of organic disease of the liver: it is even a symptom of functional hepatic derangement. Likewise, derangement of the duodenum gives rise to rheumatic pain of the shoulder joint. Pain in the muscles of the loins is sometimes the only announcement of engorgement or incipient inflammation of the liver.

2. Rheumatism of the left shoulder and arm, and of the left shoulder-blade, is one among the signs of organic disease of the heart.

3. Rheumatism of the intercostal spaces accompanies the early development of tubercles in the lungs; and acute spasmodic rheumatism of the muscles of inspiration marks the locality of a tuberculous excavation: this rheumatic pain may be mistaken for pleurisy or slight peripneumonia.

4. Rheumatism of the anterior margin of the temporal muscle, of the levator palpebre, and of the superior oblique muscle of both eyes, or of one eye, is a common symptom in indigestion, or rather of that gastric irritation when acid is formed on the stomach, and when the colon is surcharged or disordered. Rheumatic crick of the sterno-mastoid muscle, and of the scapular portion of the trapezius, has its rise from the same source.

5. There is a rheumatic pain of the scalp which I have had reason to connect with subsequent disease of the brain, with apoplexy itself, or with apoplectic turgescence. In one case in which it happened asthenic paralysis followed. Is there ossification of the arteries at the base of the brain, or mere turgescence of the sinuses? There is a fixed pain over one orbit (it is not rheumatic, though it be similar to rheumatism) which, when connected with general derangement of the health, emaciation, and a quickened pulse, is said to indicate tubercles on the membranes of the brain(?)

6. Chlorotic females often complain of rheumatism, and they trace it along the large muscles, or they point to the

shoulder-joint. But chlorosis never happens without biliary derangement, there being a feeble and deficient action of the liver, nor without derangement of the mucous surface of the stomach and bowels; which we know to be a primary source of rheumatism.

7. Whatever disturbs the circulation, so as to alter partially and occasionally, and irregularly, the total current of blood, becomes a cause of rheumatism; and thus it is, that organic disease of the heart or arteries, by interrupting the equable jet of blood throughout the body, is a known and common cause or concomitant of rheumatism. In my opinion it is very probable that organic disease of the heart is at the root of inveterate chronic rheumatism; at least I have detected disease of the heart in cases of long standing. If the heart be diseased, the defect at the centre of the circulation would seem to throw the error upon the circumference of the circle. I have observed that confirmed organic change in the heart or large vessels, always causes, or is connected with, gastric disturbance, which again becomes a reflect cause of rheumatism.

Cases of rheumatism, however, do occur without the slightest previous derangement of the health. Two cases of this kind I have seen, one of which I treated. The first was pure fibrous rheumatism of the shoulder, from sudden exposure to a cold blast, after having been heated by riding hard; and the latter (which I treated) was muscular and fibrous rheumatism, from the top of the head to the sole of the foot, in a strumous youth who had rowed up the Thames to Richmond, and was drenched to the skin with rain while in a profuse perspiration. In both these cases mucous irritation quickly supervened. In the latter, there was neither pericarditis nor any approach to it; and the case recovered, after the suffering and close medical treatment of several months.

The most inveterate and afflicting rheumatism is that of spirit-drinkers, or those who are addicted to dram-drinking. It accedes slowly, becomes most energetic towards the decline of life, seizes different joints or muscles, persists for a long time, resists every mode of treatment, and at length cripples the patient. It successively assaults, ravages, and disables the large and small joints; and attacking the flexor tendons and the muscles of the back,

contracts and distorts the back, a leg, or the fore-arm. These unhappy chilly mortals bake themselves before the fire, exhibit a brown oily skin, a shrivelled aspect, and a moist, red, glazed, cracked tongue, a sure indication of organic change in the stomach. There are hypochondriasis, profuse excretion of urine, and muscular debility; there is often œdema of the feet—the forerunner only of anasarca and ascites. In such cases visceral organic disease is always discoverable.

To every disease there is a physiognomy, an attitude, and a gait; and the practised eye discerns the nature of a malady the moment it looks into the sick chamber. Compare the guardsman, who, in the maturity of age and strength, sits as a model to the pupils at the Academy, with the same man prostrate with the anxious aspect of rheumatism, or with the bended back, the lank form, the ominous flush, the bitter smile of phthisis. Other diseases are equally cognisable: who does not know the dull, red countenance of fever; the visage of exsanguineous exhaustion; the sallow haggard expression of scirrhus? So distinct is the physiognomy of disease; and as distinct is the physiognomy of health, from robust manhood down to a strumous boy!

Second form.—Rheumatism of the joints. This form is generally manifested with acute fever, and its history is to be found in every practical work on medicine. The object of this paper forbids me entering on its history at large: it is my intention to note only particular conditions of the body in connexion with a particular disease.

There is one remark on the nature of acute rheumatism which cannot be overlooked: namely, that its inflammatory condition is not the same as that of adhesive inflammation. For these reasons: 1st, it is not curable by bleeding and mercury; 2d, it does not run the same course as adhesive inflammation, when left to itself; and 3d, it is curable by quinine, which is the very antagonist to blood-letting and mercury. Two distinct inflammatory diseases which are not curable by the same remedies cannot be the same inflammations; but rheumatic inflammation is curable by different means to those by which adhesive inflammation is cured; therefore adhesive inflammation and the inflammation of rheumatism are not one and the same.

The only time when the remedies for adhesive inflammation are requisite, is when pericarditis supervenes; but if any one would attempt the cure of acute rheumatism of the joints by vigorous blood-letting, he might drain the body of blood before he accomplished his end: whereas quinine, the very opposite to blood-letting, will establish a cure.

In this manner, the nature of diseased action may be tested by the treatment; and in this instance an important discrimination is drawn which involves the grand principle of right remedial agency.

I have never witnessed an attack of acute rheumatism in which I have not discovered direct irritation of the mucous surface of the bowels, and marked biliary derangement; and also that, in most cases, an attack of acute rheumatism has been forming during several previous weeks or months, by loss of appetite, indigestion, erroneous alvine excretion, depraved urine, insomolence, and lassitude. During the attack, the skin acts profusely, the perspiration is acid, and alternates with the urine; but it is not critical. After a time the skin becomes morbidly dry and scurfy; and debility of the mucous surface of the stomach and bowels follows—a debility which resents the exhibition of tonics, and persists for a long time.

It is with this form of rheumatism, viz. rheumatism of the fibrous tissues, that pericarditis is most frequently associated. Neither my ear, nor my eye, nor my intellectual sagacity, enable me to discriminate between *pericarditis* and *endocarditis*. I recognise the excellent distinction drawn by M. Bouilland as a pathological truth, but it fails me at the bed-side. Time, however, may render me an adept. I discover only inflammation of the heart or its investing membranes. But my experience enables me to affirm that pericarditis is not *always* present in acute rheumatic fever; though I believe it to be more frequently a concomitant, in some degree of intensity or shape, than is generally suspected. By a practised eye its presence is recognized in the physiognomy and posture of the patient; and its existence is certified by the ear. The pulse is no guide to the detection of pericarditis, if it be relied on alone; neither is the tenderness of the intercostal spaces over the cardiac region any absolute criterion, inasmuch as the muscles of those inter-

costal spaces may be rheumatic, and therefore painful. In other acute diseases besides rheumatism, I have remarked, that when the velocity of the heart's action is greatly increased, the external surface of the chest over the apex of the heart has been tender, without there being discoverable any pericarditis. Does this tenderness arise from the muscular structure of the heart becoming tender through over-exertion, or from a blood-shot condition of the heart or its membranes from the same cause—a vascular condition short of inflammation? The same patients have likewise expressed uneasiness or slight pain in that region. To be certain of pericarditis, the ear is the only guide, provided that the ear has been led to the *particular signs* when the eye has recognized the *general symptoms*. When associated with the pathognomic signs in general, the total departure of the heart from its healthy action—its tumultuous irregular pulsations—the whirring sound—and the progressive remoteness of its beat—can never be mistaken.

But though pericarditis does not always accompany acute rheumatism, yet, I think, repeated attacks of chronic rheumatism, with venous congestion or arterial injection of the brain or spinal marrow, seldom originate except in organic lesion of the heart or large vessels. Concentric hypertrophy of the left ventricle, or disease about the mitral or aortic valves, seems to be the leading organic change. Hypertrophy of the right ventricle does not seem, (to me at least) to be so frequent a cause of rheumatism, but venous congestion of the liver is a close, if not constant, attendant on hypertrophy with dilatation of the right ventricle—indeed so close and prominent that I have known the hepatic derangement to be mistaken for the primary disease. Likewise with eccentric hypertrophy of the right ventricle, the brain and spinal column escape; whereas the pulmonary circulation becomes involved, and the patient eventually dies of asphyxia.

The morbid anatomist must do me the favour to recollect, that these observations which have been made at the bed-side are submitted to his judgment, and the undeniable facts of the dead-house.

There is one point upon which my mind is not resolved: it is, whether peri-

carditis arises from metastasis of rheumatism, or whether it forms a part and parcel of the disease itself. I have seen it happen without any retrocession of the inflammation from the joints; and I have seen it happen when the disease about the joints has been cured, or at least has disappeared. No anatomy can help us in this dilemma; which is one among the questions that remain open, concerning the very many maladies that arise and terminate without a fatal issue. Upon these cases we must pronounce judgment from the best evidence that we can collect, and this evidence can be taken only from the passing features of disease during life.

So awful is the propagation of unintentional error under the aspect of scientific truth, that I often pause to consider, lest a very fact should be distorted, discoloured, or obscured, by passing through the atmosphere of my mind. Since having committed the foregoing remarks to paper, my views have been corroborated by a fatal case, the dissection of which rests upon the testimony of Dr. Bull, of Finsbury Place, and Mr. Squire, late house-surgeon to St. Bartholomew's Hospital, as well as upon my own. It happened to a patient about 36 years of age, an inveterate drinker, and a man of gross habits, and ferocious temper. There had been disturbance of the alimentary canal for a long time past, and I had occasionally attended him during three or four years for transient rheumatic pains, gastric irritation, and exalted circulation. This last October he was seized with acute fibrous rheumatism, which yielded to moderate venesection, calomel and opium, and quinine. The heart was carefully watched throughout the ten days of his illness. On the 8th Nov. its action became tumultuous; and on the 9th, while the gums were still sore from mercury, there was precordial pain, and the sounds of the heart became irregular, tumultuous, and remote. There was recession of all outward rheumatism, and he died suddenly on the 10th. On the 11th he was dissected. The pericardium was externally vascular and fat: within, it was in a state of incipient inflammation, and contained about half an ounce of sero-purulent fluid. The heart itself was of formidable dimensions; its coronary veins as well as its substance turgid with blood.

Dark gramous blood filled the right auricle and ventricle, and the walls of the left ventricle exhibited concentric hypertrophy. The margin of the auriculo-ventricular opening, with the folds of the mitral valve, was prominent, rough, vascular, and cartilaginous, and so were the valves of the aorta, to a degree verging close on ossification. It is remarkable that he did not bear venesection well, and the six leeches which were applied to the chest on the 9th, on account of the supervening pericarditis, caused great exhaustion from only a slight hæmorrhage.

In this case we behold the march of rheumatism, viz. frequent alimentary disturbance, acute rheumatic fever, gastric irritation, retreat of the outward rheumatism, pericarditis, and (discoverable after death, and prognosticated before dissection) long standing organic disease of the valves of the left side of the heart.

3. *Rheumatism of the muscles.*—Systematic works do not dwell upon this form of rheumatism with the attention it deserves. Dr. Marshall Hall, the latest writer on the Theory and Practice of Medicine, has omitted it in his classification of rheumatism, or else includes it under meningitis of the spinal column (see § 1338, 1339, 1340, Principles of the Theory and Practice of Medicine, 1837,) which, I believe, is its true connexion.

Rheumatism of the muscles is by no means unimportant, nor does it seem to arise as a symptom of deranged stomach only, though it be, like each other form of rheumatism, accompanied with such derangement.

It accedes in a distinct attack upon the broad or long muscles, and often seizes on those of the back, or of respiration. The pain is acute and spasmodic, accompanied with slight fever, a slightly accelerated pulse, costiveness, broken sleep at night, and weariness in the day. Changes of the weather, unusual fatigue, a too stimulating diet, and a liability to gastric irritation, seem to be its predisposing causes. But there is another fruitful source of rheumatism in this form—defective nutrition. In this metropolis there are those whose minds are so intent on the occupations of the day, that they forget fatigue and cold, necessary repose, and the natural calls of hunger. With the body undefended, and the brain often exhausted,

the muscles are called into powerful exertion, and the stomach is silenced with nothing but scanty and distant rations. The brain is worked to the utmost, and the heart labours to supply the undue nervous energy by an undue jet of blood. The fatigued muscles demand a greater supply of nourishment than the stomach is enabled to cook, and the meninges of the cerebro-spinal system become, at the same moment, exhausted and subacutely injected with blood. Meningitis is a consequence of this state of things.

Such a pathological condition of the body I have had occasion to note. I have seen the pale expressive countenance, the keen and fluid eye, the jaded figure, and the tremulating muscles, worked to their utmost bent. I have seen acute spasmodic rheumatism in such a patient, and arising from such causes.

Notwithstanding the gastric irritation, and the general inflammatory diathesis thus acquired, the urine remains clear, perhaps more abundant than usual, while, at the same time, the skin is prone to act with excess.

Its most distressing mode of attack is that upon the muscles of respiration; and I have seen a patient so seized as to be unable to raise himself from his bed, and unable to breathe without pain and anguish, until he was bandaged, as it were for broken ribs.

In examining the circulating system, the heart and arteries will be found to be slightly accelerated and beating with increased force. The brain has evidently been unduly excited by too great an arterial jet, and the spinal column is so distinctly the seat of increased vascular activity, that a tender vertebra may be detected over the point from which the nerves, going to the rheumatic muscles, originate. I have not discovered any pericarditis; but the question has presented itself to my mind, whether there might not exist hypertrophy of the left ventricle—some incipient organic change about the heart or valvular apparatus. If, in such cases, there be any organic change, it will be found, I venture to predict, on the left side of the heart, and not on the right; but as I have no facts to determine my judgment, so the opinion floats only in the form of a fair inquiry.

The nature of this kind of rheumatism is subacute. Its duration is uncertain;

and the extent to which it may involve the larger muscles cannot be foretold. I have known the following muscles simultaneously affected, namely, the lower edge of the latissimus dorsi, the lower intercostals, the scalenus posterior inferior, the quadratus lumborum, the depressors of the ribs, the pillars of the diaphragm, and the external oblique of the belly. Such a case has been one of great suffering.

A peculiar feature in this kind of rheumatism is, that it takes on a periodic character. The attack will come on at a certain hour in the evening with fever; then spasmodic rheumatism of the foregoing muscles, perhaps during one or two hours, followed by copious perspiration and subsequent ease. The same attack will be repeated towards the dawn, and end, like that in the evening, with a profuse acid perspiration. The brain seems to be exhausted by the attack, and profound sleep is the consequence.

I have known such an attack as this more than once.

The nature of the inflammatory condition is that of the adhesive inflammation, for the disease tells under blood-letting and the use of mercury. Now the attack is directed to the investing membranes of the brain or spinal marrow—serous membranes which are the known seat of adhesive inflammation.

Two symptoms direct my mind strongly to the spinal column alone; namely, first, that during and subsequent to the fit, the belly becomes tympanitic, the stomach being distended with air, the small intestines riotous, and the colon inflated; and, secondly, the sphincter ani has been likewise afflicted with spasmodic constriction, and the rectum has become so inert that the excrement has been expelled only with great effort, and even then of a small taper-like shape. There has been no stricture of the rectum; for the bowel has returned to its duty as soon as the rheumatic attack has subsided. Likewise there has been priapism. Now these are symptoms analogous to those which arise in surgical cases of injured spine.

In each attack which I had to treat, the colon has been torpid, and cerebro-spinal system unduly excited.

The three forms of rheumatism which I at first proposed to treat of, namely, the adventitious, the fibrous, and the muscular, are here concluded.

Concerning the rheumatism of *Old Age* little need be said, except that, independent of the universal rigidity of fibre acquired under the load of years, there is often to be found in old persons disease of the aorta and other parts of the arterial system.

But of all the formidable diseases that I have hitherto witnessed, mercurio-syphilitic rheumatism is the most fearful. Like the rheumatism of drunkards, it is inveterate; and though it may be alleviated by appropriate treatment, yet it never departs entirely, and often revisits its victim at unexpected seasons. In its accession it puts on a periodic shape; the exacerbations appear at the evening twilight, and remain visible till midnight. In extreme cases the features are shrunk and sharp, the countenance is pale, the cellular tissue lax, and the skin scurfy. The sleep is light and broken, interrupted with moanings, and frequently roused with a sudden paroxysm of pain. There is wasting, vigilance, loss of appetite, and an accelerated pulse. At the evening appearance of the fit there is fever; after midnight the pain goes away, the fever follows, and repose ensues till the dawn. The next morning is one of ease; the day passes away without much suffering, till the close of the evening, when "the scourge inexorably and the torturing hour call him to penance."

The seat of pain is along the muscles of the limbs, in the joints, and across the forehead. The swellings of the knees, ankles, wrists, elbows, and shoulders, as well as of the joints of the phalanges, are pale, indolent, hot, with a faint blush; such swellings are often connected with periosteal inflammation, and the rheumatic pain is easily confounded with the pain of diseased bone—the most excruciating pain there is.

The disease generally presents itself in a much less terrible form than that which has just been detailed, and is open to the more permanent benefits of remedial agency. The great art is to discover whether it be pure syphilitic rheumatism, or rheumatism from the conjoined virulence of syphilis with mercury. Both forms are relievable by mercury: but the mere syphilitic form is curable by it; whereas the mercurio-syphilitic is at first relieved, and afterwards aggravated by its specific action.

The latter is to be treated on the general principles of restoring health, flesh, strength, and spirits—a long process, often interrupted, and, after all, dependent in a great measure on the patient himself. But in its mildest form, it presents the same periodicity of fever and pain.

When the disease has been aggravated and long continued, under the paramount ascendancy of the venereal poison, permanent contraction of the flexor muscles ensues. A limb is gradually drawn up and bent by the flexor muscles, at first disabled, then fixed, and finally rendered completely useless. This calamity falls on one or more limbs in succession; and I have beheld a patient of this kind exhibit a hideous and appalling spectacle, as he lay distorted in limbs, agonized with pain, mutilated in features, and blackened with the foulest sores.

They who have been once necessarily subjected to a protracted course of mercury, never recover entirely from its effects. A poison has been introduced into the system, and its specific action maintained for a length of time. The syphilitic taint is met by the mercury; but the remedy allies itself with the disease in overthrowing the strength of original health. Under the most careful exhibition of mercury, for a length of time, the alimentary canal can scarcely escape. The powers of digestion are weakened; and the patient suffers, for ever after, from the imperfect play of the assimilative process. This imperfection alone would favour the advance of rheumatism, and much more support it in the march of a disease, of which it forms a leading division. I have certainly beheld these evils from a previous course of mercury to which patients have been severely and unavoidably subjected, and I consider that, independent of the broken constitution at large, there has been partial disorganization along the line of the alimentary canal.

And here I bring to a conclusion my thoughts on rheumatism. Whatever truth or error they may offer to the reader, it rests entirely with myself. My own observations, inquiries, experience, and meditation, have worked out into form the materials as here given; and the materials themselves have been extracted from private practice. I have endeavoured to render them in execu-

tion both minute and concise, and also to shew that disease may be both studied and treated in the chambers of a private dwelling with the same scientific precision as it is within the wards of a public hospital.

It is sometimes objected, that to take too minute a view of disease is an overwrought refinement in medicine. This objection cannot be true. The more intimate we are made with any subject, the more minute our information becomes; and therefore the more correct and the more valuable. The discretion, it may be the difficulty, lies in not allowing the mind to fix itself upon any one particular, whereas the judgment is to be formed upon a consideration of the whole. But that mind which cannot separate and combine the parts of a subject in a single survey, is not only defective for the practice of medicine, but is also in principle unfit for other offices in life—for instance, that of an arithmetician.

The foregoing essay is but a skeleton and outline of the subject. The leading points and divisions are marked out and defined, and the whole is put together in a tangible shape. But I have declined each opportunity, and therefore resisted several temptations, to diverge and expatiate on many collateral and important branches.

SOME ACCOUNT OF MESMERISM.

BY BARON DU POTET DE SENNEVOY.

[Continued from page 340.]

Theory of M. Rostan.

WE believe that the whole of these phenomena belong to the nervous system, all the functions of which were not as yet known; that it is to a modification—to an extension of this system, and of its properties, that the effects we are now treating of must be ascribed.

In the present state of science, every thing leads us to consider the brain as an organ secreting a peculiar substance, the principal property of which is to transmit and to receive volition and sensation. This substance, whatever it may be, appears to circulate in the nerves, some of which are appropriated to motion (to volition); these proceed from the encephalon or its dependencies to the

extremities: the others belong to sensation, and these proceed from the extremities to the encephalon. The first are active, the second passive.

We may consider the following propositions as established:—When we wish to move any limb, our brain sends to the muscles destined to execute this movement a certain quantity of the nervous agent, which produces contraction. This transmission is effected by means of nerves shewn by anatomy; and if we cut them or tie them, it becomes impossible to execute the motion; paralysis ensues. The same phenomenon takes place with the nerves of sensation; if they are destroyed, sensibility is annihilated in the part from whence they proceed. These facts, known from time immemorial, are incontrovertible, and are generally adopted. They had given rise to the opinion, that the function of innervation was an actual circulation. There were nervous vessels *afferent*, those of the will; and *efferent*, which were those of sensation.

The labours of M. Bogros seem to prove materially that which had been previously arrived at by reasoning.

But of what nature is this agent? The researches of MM. Prevost and Dumas lead us to believe that the strongest analogy exists between this agent and the electric fluid. These physiologists have demonstrated that muscular contraction was the result of a real electric shock.

M. Beclard affirmed, that having laid bare and cut through a nerve of considerable volume in a living animal, he had frequently caused the pole of the magnetic needle to deviate, by bringing this nerve in contact with the needle.

It is known by all persons, that galvanism, substituted for the nervous influence, forces the muscles submitted to its action to contract. Galvani and Volta saw and proved the existence of a peculiar fluid which, at a later period, has been recognised as identical with electricity.

It is known, also, that certain animals possess the singular property of secreting, by means of an apparatus fitted by nature for that purpose, a large quantity of the electric fluid, with which they give violent shocks; so violent sometimes as to occasion death to other fish, or even to human beings, within a certain distance of them.

The *Gymnotus electricus*, the *Silurus electricus*, the *Tetraodon*, the *Torpedo unimaculata marmorata*, and many others, possess this faculty.

The quantity of their electric fluid has been appreciated by means of very delicate electroscopes and electrometers. It has been ascertained that this fluid was secreted by the brain of these animals, since by removing it, or the nerves which go to the apparatus, the electric effects were annihilated—a result which did not take place when the organs of circulation were removed.

Thus it is satisfactorily demonstrated, that in some animals the brain secretes a portion of the electric fluid; that muscular contraction may be produced by electric excitation, &c.—a consideration which gives us strong reason to presume that the nervous agent is either electric fluid, or a fluid very analogous to it.

We shall pass over in silence the proofs that might be deduced from acupuncturation and *perkinism**.

“But this agent does not confine itself within the muscles and the skin; it throws itself off with a certain degree of force, and thus forms a real nervous atmosphere—a sphere of activity absolutely similar to that of electrified bodies.”

This opinion is also that of some of the first physiologists of our time—Reil, Authenrieth, and M. Humboldt. This being admitted, all the phenomena of magnetism appear to be susceptible of a plausible explanation.

The nervous and active atmosphere of the magnetiser increased, no doubt, by the impulse of his volition, mixes and is brought into contact with the nervous and passive atmosphere of the magnetised person, which latter it augments to such a degree, that in some cases there seems to be an actual saturation of the nervous system, capable, when in excess, of putting itself in equilibrium with surrounding bodies; and there is no way of explaining by any other

* “I confess I have always been much disposed to think that electricity, modified by the vital action, is the invisible agent which incessantly traverses the nervous system.”—*Cabanis*.

“Organised beings, and especially the human body, composed of the assemblage of a great number of heterogeneous substances in contact with each other, present to us real and complicated electrical apparatus, in which the principle, of which the nerves are the conductors, appears to act in a manner analogous to that of electricity.”—*Physiologie de Richerand*, t. ii. p. 263.

hypothesis the *secousses* sometimes experienced by the patients.

The nervous system of the magnetised person being thus influenced, and undergoing modifications in proportion to his peculiar sensibility, would suffice to explain all the perturbations observable, and would perfectly account for the communication of the desires, the will, and even of the thoughts of the magnetiser.

"These desires, this will, being actions of the brain, it transmits them by means of the nerves, as far as the periphery of the body, and beyond it."

In this sketch the full mechanism of the production of magnetic effects has probably not been unveiled. This hypothesis, however, which does not depart much from the physiological and physical facts generally admitted, would explain, to a certain extent, the greater part of the innumerable effects produced by what is called animal magnetism, and may, perhaps, assist in revealing some of the most astonishing mysteries of animal life*.

Of all the phenomena produced by the magnetisers, a few only were added to the acquisitions of science; for magnetism, as we have already said, was excluded from its pale, notwithstanding the numerous efforts we have recorded which were made to procure its admission. Good works upon magnetism are in consequence scarce, although many books have been written on the subject, as they are, for the most part, the production of men unacquainted with the physical sciences. The first which created any sensation was that of Mr. Deleuze, librarian and professor of natural history at the Jardin des Plantes, in Paris, written with simplicity, but with considerable logical force. It was difficult to reply to it, for the author was a man of high probity and acknowledged science. No one dared to deny the facts which he attested, but he was nevertheless forced to pay the penalty of his veracity. He possessed all the qualifications which entitled him to be a member of the Institute, yet he did not obtain that honour. His *Histoire Critique du Magnétisme* was the cause. It was only from this epoch that the progress of magnetism became manifest. An academical society of magnetisers was formed, for the study of

the discovery of Mesmer, composed of persons of consequence and of some distinguished medical men. M. de Puységur was elected its president. The society published a journal, and from that time the magnetisers ceased to be on the defensive; they now began to attack their adversaries, and to provoke them anew to a serious investigation, encouraged by the favourable opinions respecting magnetism expressed by Ampère, La Place, and Cuvier. This was in 1819. I was then young, and a medical student. A stranger to all the prejudices of another age, I was ignorant that physical facts could be rejected because they were inexplicable, or because they were contrary to received doctrines. Never doubting, however, that truth must be triumphant, I dared, inexperienced as I was, to demand publicly that I might be permitted to make experiments at the Hôtel-Dieu, at Paris. My proposal was immediately accepted, and I had to appear before incredulous persons, who, perhaps, were so in sincerity, because the truth which I announced appeared to them incredible. It would be useless to relate here all the details of these experiments, which have besides been published in a pamphlet which went through three editions, and has never been attacked. It suffices to say that my success was complete; for a patient who had been thirteen months in the hospitals, and who, for a very long time, had had hæmatemesis, which nothing had been able to stop, being subjected to magnetism, in the presence of forty physicians, experienced such marked effects that she quitted the Hôtel-Dieu twenty-seven days after, perfectly cured. In this patient the vomitings, which had immediately subsided on the first magnetisation, returned on its being suspended, and again ceased, without again recurring, when the operation was resumed. This treatment was attended by extraordinary phenomena of somnambulism, and every trial that incredulity could suggest was employed to ascertain that there was no collusion. An account of all the phenomena which had been produced was drawn up, and deposited with a notary in Paris. This account was signed by all the medical men present, amongst whom were Husson, Geoffroy, Recamier, Briche-teau, Delens, &c.

Some time after, I repeated the same

* In giving the above hypotheses, I wish it to be understood that I by no means consider them sufficient to explain satisfactorily the various phenomena.

experiments at l'Hospice de la Pitié. These led to further trials, and it was then that MM. Georget and Rostan ventured to publish their observations; the former in a work on the Physiology of the Brain, the latter in the article *Magnetism*, in a Medical Dictionary, printed in 1823. They were prepared for all the consequences of their avowal. They knew that the philosophic spirit of the age would be strongly opposed to their assertions, but, confident in futurity, they trusted to time for the justification of their opinions.

M. Esquirol, physician to La Salpêtrière, authorized experiments on the epileptic patients, of whom there were great numbers in his hospital; and there also all the magnetic phenomena were produced, in presence of a crowd of medical men, anxious to verify personally facts of so extraordinary a nature. Bicêtre was also the scene of this kind of scientific investigation, but it was principally at La Charité that magnetism was more extensively prosecuted. About 300 medical men witnessed the experiments made by Dr. Foissac—experiments which were conclusive on the greater part of the controverted points.

Nearly at the same period, experiments were being made at Val-de-Grâce. I magnetised there, in the presence of many students and medical men, an epileptic soldier, who experienced very marked effects. A young physician, Mons. Desruelles, attached to the hospital, submitted himself to magnetisation, and the action was so violent that, after a few minutes, I was obliged to cease operating.

Experiments still more conclusive, if possible, took place before M. Broussais and seven or eight other physicians. Dr. Frappart threw into somnambulism a soldier affected by a nervous complaint; and the trials that were made upon him left no doubt respecting the power of magnetism. M. Broussais was so convinced, that he authorized the publication of these facts.

At this period a very decided movement was again observable in the public mind. Works upon magnetism were in request, and acquired a degree of popularity they had never before possessed.

A very distinguished young physician, Dr. Bertrand, of whom science was

too early deprived, established conferences upon magnetism at his own residence, which were attended by many enlightened men. Although his opinions were contrary to the explanations of the magnetisers of that time, he contributed much towards the establishment of magnetism by his philosophic mind, and by his profound researches into the phenomena which have some analogy with magnetism, such as ecstasy, catalepsy, &c.

Many of the members of our parliament employed themselves in spreading magnetism, both by their writings and by experiments. M. Chardel, the Comte de Gestas, M. de Lascases, and some others, opened their saloons to those who were desirous to be instructed in magnetism. Experiments were made upon horses by the Marquis de la Roche-Jacquelin, colonel of a cavalry regiment in garrison at Fontainebleau, which were perfectly successful. The *corps médical* of Paris began to give serious attention to magnetism, and one physician felt sufficient confidence in himself to invite the Royal Academy of Medicine to a new investigation.

The Academy knew not yet which side to take. It contained many individuals convinced of magnetism, and others who desired to be enlightened on the subject; but fearful of suddenly giving too much importance to the magnetiser—fearful even that the nomination of a committee to examine into it would lead to the opinion that it was favourably disposed towards magnetism, it adopted a middle course; it commissioned MM. Adelon, Burdin aîné, Marc, Pariset, and Husson, to investigate the subject, and to make a report by which the Academy might decide whether it could, without compromising itself, appoint a committee for the proposed examination.

These commissioners, faithful to their charge, presented themselves some months afterwards (on the 13th of December, 1825) at the Academy, to relate what they had seen and heard of magnetism. On that day there was a great tumult in the council-room, which did not terminate with the sitting. Three days were occupied in hearing all the opinions, and the Academy at length decided for the investigation. All the members voted on this question, which was considered a very important one,

and there was a majority of ten in favour of those who wished to settle their opinion on the subject of magnetism.

A committee of eleven members was nominated; it was composed of physicians, whose names were as follows:—Bourdois de la Motte (the President), Fouquier, Gueneau de Mussy, Guersent, Husson, Itard, Marc, J. J. Leroux, Thillay, Double, and Magendie. This was a great step towards arriving at a definitive conclusion. By naming the most honourable of its members to examine into magnetism, the Academy forced those persons to be silent who thought it was influenced by sentiments hostile to a truth which seemed to attack received systems.

The committee immediately began its labours by an address to all magnetisers. It invited them to produce, in the presence of its members, the effects they professed to be able to work: some hospitals even were placed at their disposal. Very few replied to this amicable appeal, because this inquiry gave a publicity to those who took a part in it; and although believing in magnetism and practising it, they yet were not willing to put themselves forward, as a failure would have exposed them to ridicule.

The Chair of the Faculty of Medicine of Paris resounded for the first time with the language of magnetism. Professor Rostan pronounced an apologetic discourse on Mesmerism, and on its wonderful effects, in the presence of more than 400 young men. He dared to relate facts which overthrew received opinions, and which justified the assertions of the magnetisers, who were looked upon as enthusiasts.

Lhullier Vinslow, in a treatise on *Materia Medica*, acknowledged the existence of magnetism without any restriction. Professor Segalas, in his private lectures, also undertook the defence of magnetism, frankly acknowledging to his pupils that he had been incredulous, but that having magnetised, he had produced extraordinary phenomena. Marjorlin recommended the employment of magnetism in certain nervous complaints; Fouquier also. Magnetising physicians were then seen for the first time since 1784. At that period it occurred to me to give public lectures on magnetism, and more than 800 young men received instructions,

very imperfect, no doubt, but nevertheless they learned what was meant by animal magnetism.

The antagonists of magnetism did not consider themselves defeated, but they were more timid in their attacks. One alone threw off all restraint: Dr. Virey, a man of *esprit* and imagination, asserted that all magnetisers were imbecile and impostors; but it was a singular fact, that his own works were found to contain the most solid arguments in favour of our opinions; and the magnetisers took care to bring them into notice by means of the press.

Magendie, the colleague of Rostan at l'Hospice de la Salpêtrière, maintained his contrary opinions with less force, and requested to be appointed one of the committee for the examination.

In a subsequent article it will be seen that new antagonists of magnetism arose, opposing the most formal denials to the most positive assertions. But these scientific struggles, which are considered as injurious, are on the contrary favourable to truth; certain names only are compromised by them.

[To be continued.]

HINTS FOR ACCOUCHEURS.

To the Editor of the Medical Gazette.

SIR,

I THINK practitioners do not avail themselves of the advantage which medical periodicals ought to afford, of seeking that information from the profession generally, which their medical acquaintance cannot furnish them with.

I have asked several of my friends, what special reason there is that women should be invariably delivered on the left side. I have got no satisfactory answer from any one, and some treat the bare idea of delivering a woman on her right side as an insufferable medical heterodoxy. It appears to me, that in all common cases the right position has equal advantages with the left, and in many difficult cases superior. I assume that the right hand is generally the most practised and *dexterous*, and (if I may so term it) intelligent. Now where the os uteri is canted up obliquely back-

wards, and there is need of accurate examination, how well does the back of the right hand adapt itself to the concavity of the sacrum, if the patient lie on her right side. Again, in certain transverse presentations, turning will be much facilitated by placing the patient on the right side, and using the right hand.

There is one point more I wish to allude to, which is, the inconvenient mode of passing the female catheter generally practised. I never used to set about the common way with the certainty that I could accomplish it without exposing the patient. This, perhaps, arose from want of natural adroitness—it certainly did not from want of instruction and practice, for I was taught by that excellent lecturer, Dr. F. H. Ramsbotham, and attended the *cours d'accouchemens* of Madame Dutillieux, who (as I was the only English pupil she then had) took me under her especial patronage, and (besides an unusual quantity of *touchés* and speculum practice) insisted on my always passing the catheter "*tout-à-fait à l'Anglais*."

The mode I always now pursue is this. I place the patient close to the edge of the bed in the usual midwifery position; I get a utensil put by the side, and raised nearly to the level of the bed; I easily find the meatus urinaris from its proximity to the vagina; I then introduce a male flexible catheter, and direct the other end over the side of the utensil. The bladder gets thoroughly emptied without further trouble, and not a drop of urine escapes, so that even a napkin underneath the nates is unnecessary. If I experience difficulty, I raise the edge of the clothes, and look, and the patient is not conscious she is at all exposed. Let a person compare this with the ordinary mode, in respect of ease, decency, and cleanliness, and once try it, he will then keep his female catheter for extracting plugs from the nostrils, as recommended by one of your correspondents some years since.

I remain, sir,
Your obedient servant,
HENRY COLES.

Cheltenham Nov. 21, 1837.

GRATUITOUS SERVICES OF MEDICAL MEN.

To the Editor of the Medical Gazette.

SIR,

SOCIETY is divided, I think, by political and statistical writers, into two orders, viz. the *easy* classes, and those others who, as less comfortably circumstanced, may be called the *uneasy* classes. Now I apprehend that the *Medici* family cannot, with propriety, be ranked with any other than the second order. All professions and pursuits have, undoubtedly, their drawbacks and grievances; but the above-mentioned family seem peculiarly uncomfortable and uneasy, and particularly industrious in the exercise of the Englishman's privilege of *grumbling*. I speak, of course, of the masses of each section. Those, of whatever branch of practice, who are well to do in the world, are, as usual, pretty well satisfied with their status, or are at least comparatively so; but the masses, the unhonoured, unbeneficed, unpaid, or underpaid masses, are, as heretofore, uneasy: of this fact, the course of discussion in the medical journals at the present moment affords, I conceive, ample evidence; the uneasy class is, of course, the bulk and body of the profession, and consequently of the reading medical public. The opinions, wishes, hopes, and fears of the uneasy portion of the *Medici* family, are thus pretty fully represented in the medical journals, more especially in those that approach nearest to the character of newspapers. To shew this it would be sufficient, I believe, to glance at the journals of any week or month—for I know not how many weeks, months, or years past, perhaps I might say nearly a dozen, dating from the second or third year of "*The Lancet*," when that journal, determining to swim without the cork of patronage of hospital magnates, threw itself on the masses. But my object at present is not to shew the extent of the dependence of the press on the uneasy professors of the *ars Apollinuris*, but to offer a few observations on the grievances at present most complained of by the profession, viz. gratuitous professional service. This topic has been of late much discussed, in conversation, in medical cir-

cles, and by editors and contributors in the medical periodicals.

Now the question of the policy of gratuitous service presents itself in two forms, more particularly at present, viz. that of affording gratuitously to the Assurance Societies professional statements as to the health of parties desiring to effect insurances on lives; and that of accepting of official appointments of an onerous nature, such as the office of physician or surgeon to a dispensary, without salary or remuneration. As to the first, the grumblers argue, that the private practitioner's statement or certificate of health, usually looked for by assurance societies, in addition to the opinion of their own medical officers, should be given only in exchange for a full professional fee, to be disbursed by the society requiring such statement; and that the prevalent practice on the part of assurance societies of asking such statement without fee, is unjust to the Faculty, of whose members the societies take advantage, so as to enhance the profits of their trade by at least one guinea per policy, at the expense of a too generous and easy profession. And it is held by many highly respectable authorities, that medical men should resist such practices, and in all cases decline answering assurance office secretaries' letters, unless previously themselves *assured* of touching their consideration. Now the whole question between the profession and the assurance offices seems to me to be a question of manner rather than of matter. The question is not whether the medical practitioner should be remunerated by the party indebted to him for whatever he does of work strictly professional, for that cannot be the subject of dispute, and is not really under discussion—nor is the question, whether the assurance office should be reimbursed for its outlay and use of capital, for it is clear that the assuring society are entitled to adequate remuneration. The real question is not by whom or from whose funds, or at whose expense, the private practitioner's statement, so essentially necessary to the continuance of the established system of selection of lives for assurance, if not afforded gratuitously, is to be paid for; it is as clear as light, that the party seeking to obtain the policy, and he only, is bound to pay all expenses: as purchaser, on him must

necessarily, as well as justly, fall all the costs of production. The whole inquiry lies in a very small compass therefore, and there is no question open for dispute but this, viz., whether it is more convenient for the practitioner who will not act gratuitously, that he should settle for his statement with his patient who desires to insure, *directly* or *indirectly*, through the medium of the office. Now, looking to the pecuniary convenience of the Faculty only, the indirect mode of settlement would be much preferable. In that way the practitioner might generally reckon on his fee, at least in all cases in which a policy should be effected. In that way the real biter, if biting there be, would generally be bitten; for the office would, if the fee were considered of any importance to it, raise its charges, and the applicant must (under another name perhaps) disburse the additional 1*l.* 1*s.* But what would be the effect on the mind of the patient of a failure, owing, he might know or conjecture, to an unfavourable statement purchased by the office? or what the effect on the morals and character of the practitioner of the fear of offending a valuable connexion? Take the case of a practitioner requested by a secretary, for a regular fee, to certify as to the health and habits of a wealthy drunkard, opium-eater, or other suicide—of a patient liable, by inheritance or otherwise, to any one of several diseases that frequently occasion sudden or unexpected death: and is such a referee not in a situation of great difficulty? Most would, under such circumstances, risk the custom, and tell the truth: but some might condescend to conspire with the patient, who, in such cases, is often devising fraud against the office, and certify falsely: and in either case difficulty, and probably disadvantage, to the profession would result.

Take now the other case: suppose the practitioner feed regularly by the patient desiring to insure: and how, I ask, is his position mended? Commercially he stands as in the former case, only that he is feed directly by the person that needs the beneficial result of the proceedings in which the practitioner is required to take part. But is he not equally exposed to bias of judgment and suspicion of partiality? If the health and habits of his patient be

unfavourable, how is he to escape the embarrassing choice between good faith toward the office, which by the supposition consults him in confidence, and good nature and friendly feelings towards his patient, and a prudent regard for his own interest as a dealer or practitioner. For my part I have been more than once in the position alluded to, and have therefore practically experienced the difficulty; and the conclusion I have come to after some consideration, and, indeed, repeated reflection on the subject, is, that the practitioner should consider himself as simply a witness, and should be content to answer gratuitously all questions not requiring a personal examination, or re-examination, of the applicant for the policy.

The queries usually put to the practitioner, with one or two important exceptions only, I think all have relation to the past, and run thus:—Has the applicant had this, that, or the other disease, or liability to disease, or habit of life generating tendencies to disease, during your knowledge of him? Such questions fill the paper of inquiries nearly, and may all of them be answered from memory. But then comes a sweeping question, demanding an opinion strictly practical—not an historical statement, viz. is the party a good or bad life for his years? Now this last query, if understood as applying not merely to the past but the present also, may in many cases require a personal examination; in cases, for example, where the medical referee has not professionally examined the party for months, or years, as may sometimes happen. In such instances I readily admit the practitioner is entitled to a fee, not, however, from the office, but from the applicant—not from the vender, but from the purchaser of the policy; and it is against the applicant, and not against the office, that the stand should be made, for he is the party that seeks the benefit of the cost of which the medical referee's fee is an essential part. (*See concluding paragraph.*)

On the whole matter, my conclusion decidedly is that the profession would do better by wholly declining fees for such services.

The advantage of this would be this at least—viz. the practitioner would have, with respect to either party, no temptation to over or understate the facts. I mean, the transaction would of itself, and irrespectively

of previous and extrinsic causes of mental bias, bring into play no sinister interest or feeling that might mislead the practitioner: whether his report were favourable or unfavourable, accurate or inaccurate, as proved by future events, neither party, however affected by the result, could reproach him with more than error of judgment. Applicants generally at once, and the public at large, very soon after the adoption of such a rule by the profession, would come to know that in applying to their medical friends for a statement of health they were asking for a favour which the practitioner might properly, should he see reason, refuse, and which, in granting, he would concede without any lien upon his judgment procured by value received from either party.

The second form of gratuitous service above referred to, is the practice of doing the duty of physician or surgeon to dispensaries without salary or other fixed emolument. Now this case seems to me clearer even than the former. I conceive that junior practitioners are fully justified in seeking dispensary and other gratuitous appointments, and that the profession gains rather than loses by the practice.

I am aware that many disadvantages are alleged to flow from gratuitous labour in this as in other forms, and that the Editor of the MEDICAL GAZETTE has thrown his sword into the scale against the practice under consideration. But I am not the less of opinion, though fully sensible of the weight of opposing authorities, that the practice is, under present circumstances, unobjectionable, and I hope, and am sure, it will be continued. I have been myself a gratuitous labourer for years, amongst the sick of public institutions, and have had occasion to think over the matter many times, and have no doubt that the pecuniary interest of the profession suffers nothing, and that its respectability rather gains than loses by the practice so often referred to.

In the first place, it is an error to suppose that gratuitous labour is less honourable than hired; that the exact contrary is the fact, under parity of circumstances, I am well satisfied. The vicious habits of thought and feeling that have generated the contempt for daily labour and humble industry, so

prevalent amongst all classes of mankind, not excepting even the industrious classes, are conciliated rather than offended by the circumstance that the labour is voluntary, and may be declined at pleasure. It is the commercial pride, the traders' tastes, rather than the pride or tastes of the fastidious gentleman, that revolt against service without pay. Now social rank and respectability are, in this country, infinitely more under the influence of the pride aristocratic than of the pride commercial. I hold, then, that the dignity of physic and surgery is nowise compromised by gratuitous labour in public institutions, but is, on the contrary, better provided for than it could be by any engagements for salary, or other remuneration, that any dispensary, &c. could afford; for I repeat it as a fact, that volunteer labour is, *ceteris paribus*, more respectable and respected than hired labour, even at full wages, and infinitely more so than the same at low charges.

In the second place, the pecuniary interests of the profession suffer nothing by it; and for this position, likewise, I fancy I have good grounds. The truth is, the junior practitioner (I allude to unmixed practice) has to choose between absolute idleness, *quoad* medical practice, and gratuitous labour: lucrative practice is quite out of the question, for many years after his graduation, or before his head is bare, or at least well grizzled. The exceptions to this observation are so few as to prove the rule, and they are attributable, in every instance, to uncommon advantages, and in almost every instance to more than usually effective patronage. The junior practitioner, then, has a powerful motive to induce him to seek unsalaried official employment. He has to escape *ennui*; he has to provide against the inroads of time upon his stores of knowledge; he has to convert into practical impressions his scholastic notions, into skill his mere learning, and, in a word, to complete his education.

In what condition, I ask, do our Universities and Colleges usually turn out their alumni? Are physicians and surgeons finished artists at starting?—or is there any such thing as a physician or surgeon, green and fresh from the University or College, that any man of experience in life would consult with confidence as a competent practitioner?

Certainly not. Nor in asserting this, as I do without hesitation, as an undeniable fact, do I in the least impugn the respected and often eminent persons that preside over our educational institutions; for it is to assert this, and no more, that either the construction of the latter institutions (and not merely the management) is so defective, or else the difficulties impeding their more successful operation are so considerable, that a few years of college or university training are not sufficient for the production of accomplished practitioners. If, then, the young medical man must spend a long apprenticeship or novitiate as a probationer candidate for lucrative practice, as he undeniably must, why, I ask, should he hesitate to prefer self-improvement by gratuitous labour to an idleness wholly unprofitable? It has been alleged, that in this commercial country a craft suffers in popular estimation whose professors, in any considerable proportion, labour without remuneration; and the clergy have been referred to as able to do little, if any thing, for their Protestant fellow citizens, that any other adult and equally well-educated males might not do as well, if parliament had not meddled in spirituals in right of succession to his Holiness of Rome; yet the clergy, it is said, are, on the whole, handsomely paid for their labour, and do no ceremonies or service without remuneration. And the bar has also been cited, as, though enjoying few sinecures, yet nearly quite guiltless of service without pay.

But the question has only, I conceive, been mystified by those references to other professions, placed under very different conditions. The pay of the endowed clergy is, for the most part, a tax paid by all indiscriminately, whether with or without return in the way of service; in fact, a national cess for the use of gentlemen of a certain calling and opinions. The pay of the dissenting and independent clergy consists of free-will offerings under various names, which men may give or withhold, according to their opinion of the value of the service. The lawyers are commonly considered the most rapacious of all; but their work is open, for the most part, to the light of day, and can be judged of, more or less accurately, by all parties concerned, that will be at the trouble of a little attention and in-

quity. Unlike the victims of the doctors' clerical and medical, the barrister's "dead man tells tales." Want of sound learning, or of practical knowledge, in a barrister, cannot escape detection. No ignoramus, and few ungifted men, have attained eminence at the bar: so that if any man of a learned profession is excusable for rapacity, it is the barrister. Yet, if the barrister declines usually to work without reward, it should be recollected that in his case there is and can be commonly no feeling enlisted to give interest to his exertions, but the desire of self-advancement. He is probably ignorant, personally, of his client; who is, further, no object of pity or sympathy in general, but a man in full health in most cases, and often in most prosperous circumstances; sometimes also a branded rogue, oftener an unscrupulous litigant, and at all events he is seldom presented to the practitioner in circumstances to excite compassion, or other emotion of generous interest. How, then, should it be a common practice amongst legal practitioners to work without remuneration? their work, unlike the cure of sickness, excites no popular interest or natural sympathy sufficient for a time to make legal practice its own reward.

Thus the examples of the church and bar are not in point: what then remains in the way of pertinent argument against gratuitous official service? Will it be said that the familiar experience by the public of voluntary unrequited medical labour must tend to cheapen medical service in the general estimation, so as in ultimate effect to lower the average rate of remuneration? But this cannot be said; as I have already shewn that gratuitous labour is more honourable, *ceteris paribus*, than hireling labour. Will it be said that, rather than countenance a practice which many consider at least problematical, for the sake of self-improvement, or other private motive, it would be better to take measures to obtain a home gratuitous practice of sufficient magnitude, an end which I know to be not difficult of attainment in this city? Well, neither can this be maintained by the opponent of unsalaried service, for gratuitous labour is the object of his reprobation; and a practice, whether public or private, that is without reward, is in either case of the character he condemns.

There is one consideration not yet alluded to, which in many minds must weigh heavily in favour of the prevalent practice of junior practitioners rendering gratuitous service in public institutions. It is this: as no class of persons not possessed of competent means could be expected, under any circumstances, to enlist in the public service without remuneration; and as no unpatronized practitioner of pure physic or surgery can hope to cover his annual outlay for years, perhaps a dozen, perhaps many more, by practice alone, it follows, first, that the patients of public institutions usually have the care of the best-educated class of practitioners almost exclusively; and, secondly, that the departments of unmixed practice—viz. medicine and surgery—are filled more nearly, if not exclusively, by men of comparatively independent fortunes, and consequently, it may be presumed, of ampler opportunities and more varied attainments, as well as of more gentlemanly habits. For such persons gratuitous official service affords a resource against the tedium of an otherwise idle life, and a security against retrogression; and, more than that, a sure means of advancement in professional knowledge, and an opportunity of manifesting their practical capacities, which is of no slight value. There is also one other advantage of gratuitous service in early life, that seems wholly overlooked or condemned by the grumblers: it is the tendency that it has to counteract the rapacity so natural to the trader, and so conspicuous in many other professions and callings. It is commonly said by the Faculty, and I believe firmly it is truly said, that no body of men in existence do so much work without receiving or expecting remuneration, as do the members of the medical profession; nor, on the whole, though some grumble, have I seen reason to think that the majority grudge to the poor, or the charitable rich, the gratuitous service rendered by themselves or their fellows. Nor is this an unwise liberality. If medicine is honourably distinguished in a moral view above almost every, or I should say simply, above every other calling, it is so distinguished especially for its disinterestedness. Now if there is any feature of character more advantageous than another, it is precisely that readiness to volunteer service to suffering

fellow men that constitutes, on the part of the medical profession, that disinterested beneficence for which it is so distinguished; and it is inconceivable that a reputation for liberality and humanity, resulting from the daily manifestation of those qualities, should not, directly or indirectly, redound to the advantage of its possessor. It appears to me absurd to suppose that the profession could fail to gain, according to the share of credit it might obtain, for the highly and universally esteemed qualities above named. Indeed, I have no doubt that the maintenance of the old scale of fees (so little diminished, notwithstanding a change of circumstances amounting in some respects to a revolution, especially in the increased number of competitors of superior qualifications, and the diminution of expense and of other difficulties of professional and general education), is not a little owing to the high respect entertained by the public for the medical status and character; to which feeling the practice of gratuitous official service must very considerably, whether directly or indirectly, contribute: so that, to conclude this rather long letter, I have no doubt at all that it is highly advisable that junior practitioners of medicine should accept of unsalaried official practice, and enlist as volunteers, and without hesitation, in the public service, when so circumstanced as to have to choose between a practice not lucrative, and an idleness wholly unprofitable.

In the hurry of writing I find I have omitted one consideration quite pertinent to my argument; it is this:—How will the *protestant* practitioners of Leeds proceed if the assurance offices should meet their threat by a change of mode in transacting business, viz. by calling on the applicant to furnish, or have furnished to the office, a statement of his health such as that in use at present, authenticated by the signature of his medical attendant? In that case it is obvious that those protesters will have the painful choice forced on them, of either refusing their patients, or demanding an unusual fee.

This change of proceeding is obvious and easy of execution, and if adopted will soon shew which is the strongest. Can any one but a medical practitioner doubt which of the parties would have reason on his side? It is not still less

open to doubt on whose side might would be found.

My advice, then, to my professional brethren is this: certify for your patients who seek policies of life assurance, without fee or remuneration of any kind, or from any party; but if you will insist on payment, be cautious how you attempt to enforce it. Appeal to the sense of justice of your patient, or of the office, or of both; remonstrate as much as you will; the more perseveringly you do that, the better for your purpose. But beware of threats; beware of strikes and trades' unions like that of Leeds. To enforce payment from the office would be impossible, as I have shewn; to enforce it from the patient seeking to insure must often be inexpedient. Trust to time and discussion, and avoid the sulks; and in the meantime do not forget your duty to your neighbour, whether the same be a Pharisee or a Samaritan; nor abandon the good old medical practice of rendering service when called on, unconditionally, and pay or no pay.

I am, sir,
Your obedient servant,
MEDICUS.

Nov. 15, 1837.

MEDICAL GAZETTE.

Saturday, December 2, 1837.

"Licet omnibus, licet etiam mihi, dignitatem
Artis Medicæ tueri: potestas modo veniendi in
publicum sit, dicendi periculum non recuso."
CICERO.

NEGLECT OF THE SICK POOR.

It is now about three years since the utilitarian faction began to enjoy the long-coveted privilege of treating the wretched, the destitute, and the broken-hearted, according to the maxims deduced from the "population principle." They succeeded in persuading the two chief parties which divide the state, that it would be an admirable thing to allow them to "grind the faces of the poor," and treat indigence with a rigour scarcely deserved by ordinary crime. Nearly all England has now tasted the

bitter fruits of this cruel experiment—such as infanticide, death by cold and hunger, workhouses depopulated by typhus fever, and the blood-stained introduction of the new tyranny into Bradford. Death from neglect of the sick and starving poor has become so common, that we fear the constant recurrence of the same or similar incidents in the columns of the newspapers, will produce a kind of callousness in ordinary readers, and they will turn aside from the detailed account of Englishmen put to death within the dreary walls of some Union Bastile, with the unconcern with which they might run over the list of killed and wounded at a tithe-battle at Limerick or Tipperary. With medical readers the case is widely different. When a homicidal dietary is enacted in a workhouse, it is always with the express or implied sanction of a medical practitioner; when the exhausted labourer attempts to obtain an asylum for a few weeks in an infirmary, it is to some member of our profession that he applies for a certificate; and when he at last reaches that haven where commissioners cease from troubling, it remains for some honest surgeon to unfold the history of the case, and to ascertain, as far as the skill of the dissector may, whether the result was inevitable, or the consequence of Malthusian laws administered in their true spirit—whether, in other words, the deceased was incurable when he applied for relief, or was rendered so by cold, hunger, and ill usage. Hence no one of these cases ought to be uninteresting to a practitioner of physic; and the more frequently they occur, the more ardently ought he to wish the stigma of concurring in the new poor-law to be obliterated from the annals of his profession.

These observations have been naturally called forth by a very painful case

reported in the *Times* of Monday, November 27th. It is an inquest held on Saturday, the 25th, upon the body of Charles Morris, aged 32, who died in the Clapham Union Workhouse, on Tuesday, the 21st. Before commenting on the evidence of the several witnesses, we will just give a brief abstract of the history of the case of the unfortunate patient. On Wednesday, the 15th of November, he applied to Mr. Powis, the surgeon of the Union, from whom he obtained some medicine, and a certificate to be shown to Mr. Burgess, the relieving officer. On Thursday, the 16th, Mr. Powis saw the patient in the workhouse, where he was not admitted, but working at a hand-mill during the day. Friday, the 17th, the patient was reported, by the superintendent of the mill, as too ill to work, but was not seen by his surgeon, and was turned into the streets at night.

Saturday, the 18th. — Patient again not seen by his surgeon, and still not admitted into the workhouse. Sunday, the 19th, ditto. Monday, the 20th, not seen by surgeon; at half-past ten at night he obtained an order of admission into the workhouse. Tuesday, the 21st, is found in the workhouse by his surgeon, in a state of collapse, and dies shortly afterwards. This journal of the case will enable our readers to understand more clearly the evidence of the witnesses on the inquest, as far as it is intelligible. The first witness called gave an account of the appearances on dissection; and though we must confess that his evidence, as reported, is quite unsatisfactory, we are ready to attribute this to the report having been furnished by an unprofessional person. We have no doubt, however, that so good an anatomist as Mr. Solly will think it due both to the profession and the public, to furnish an account of what he really did find in the body of

Charles Morris, instead of the following vaguenesses:—

“Mr. Samuel Solly, surgeon and lecturer at St. Thomas’s Hospital, deposed, that he was applied to, by the parochial authorities of Clapham, to perform a post-mortem examination of the deceased, on Friday; when he found the internal appearances generally healthy, but the cavities of the chest were charged with blood. The appearances were not the result of long-standing disease. The body was not in an emaciated state.”

At the end of the inquest, he was asked whether, from the evidence he had heard, in conjunction with the *post-mortem* appearances, “he could account for the cause of death?” To which he is made to answer, “that the complaint of the deceased appeared to be influenza, and that death was caused from suffocation, by the sudden departure (?) of the fluid into the windpipe. Many similar cases have occurred during the last six months.”

This answer still leaves the *post-mortem* appearances wrapped up in mystery; but by telling us what the disease of Charles Morris probably was, throws considerable light upon the evidence of the others.

The next witness called was Mr. Henry Powis, surgeon and medical officer to the Clapham and Battersea Union. He stated that the deceased called upon him on the 15th November and said that he had a bad cold, which he had caught from sleeping in the open air. Mr. Powis now gave him a certificate to the effect (as appears from the statement of Mr. Burgess, the relieving officer,) that he was ill, but not seriously. Now, the question which Mr. Powis is bound to answer is—what were the symptoms under which this unfortunate man was labouring when he applied to him, after sleeping in the open air in November. What was the state of his pulse, respiration, &c.?

In fact, was he, or was he not, clearly suffering from “influenza,” and in a state in which, had he been a comfortable Battersea shopkeeper, he would have been immediately sent to bed, and visited daily by his medical attendant? The next day, the 16th, Mr. Powis found him in the workhouse, and still thought him not seriously ill, though in addition to his former complaints he now observed that his patient had a large boil upon the inside of his left thigh, which was painful. It appears, that in this agreeable asylum, this test-house for the sick and destitute, they have a hand-mill to grind flour; and strange—most passing strange to say, the said Powis did not think Morris unfit to work at this machine! We certainly had always thought that in every inflammation repose was the first and second and third things requisite for recovery; and that, in so depressing a disease as influenza, severe exertion must be peculiarly dangerous.

The witness did not see his patient again for five days, that is to say, till the following Tuesday, the 21st of November, when, on passing the workhouse in his gig, he was called to by the porter, who told him that Morris was very ill. Having just then an urgent appointment, he was unable to stop, but saw the deceased in about two hours, when he was in a state of collapse. Shortly afterwards he expired. In answer to a question from a juror, Mr. Powis is made to say, “Death might possibly have been the effect of exposure to the night air.” *Possibly*, indeed! We should be glad to know how many persons could stand sleeping in the open air in November, with the thermometer considerably below the freezing point, as it has frequently been during the last month?

We must again repeat that a professional report of the evidence given

at this inquest is imperiously required; for Mr. Powis's evidence, as we find it reported, is not a whit more satisfactory, though somewhat more intelligible than Mr. Solly's. From the patient's account of himself it was clear that he had been exposed to circumstances fitted to ruin the toughest constitution. It does not appear that Mr. Powis took him to be a malingerer; there was the boil, at any rate, plain enough; the symptoms of influenza too, are not obscure; and yet the unhappy patient was set to work at the mill by day, and turned out of the house at night!

Mr. Burgess, the *relieving* officer, was the next witness. He stated that the deceased applied to him on the 15th of November, and asked to be admitted into the workhouse, saying that he had a bad cold in his limbs. The witness gave Morris a medical order, and desired him to bring back a certificate: he did so, and it was to the effect that he was suffering from a bad cold, but not seriously. "He asked the deceased if he thought he could work, who replied, that he thought he could not. Witness referred to the certificate, which stated that he was not seriously ill, when he gave him a loaf, and directed him to attend the board the ensuing day. He said that he had been sleeping at Whitechapel the last few days."

This witness's phrase of "sleeping at Whitechapel" is a good instance of euphemism, or the expressing an ugly thing by an innocent term; just as when Alice Bean, in Waverley, talks of her father's *misfortune*, i. e. his being hanged; for poor Morris's "sleeping at Whitechapel" was, as we have already seen, in fact, bivouacing at Whitechapel, with the thermometer ranging from 21° to 40°.

Mr. Burgess proceeds to state, that the deceased attended the board as di-

rected, on Thursday, the 16th; but it appears that this was only a constructive attending, as the board did not see him, and refused his application on inspecting his certificate. On hearing this decision, Morris asked for a day's work, and was put to the mill. On leaving, he said, "Where am I to sleep at night? I have no lodging. Will you oblige me by advancing me sixpence?"

We do not know what the triumvirate at Somerset House would say to such reckless generosity on the part of a relieving officer; but Morris actually obtained this handsome loan.

About half-past twelve the next day (Friday, the 17th,) Read, the superintendent of the mill, reported that the man was too unwell to work, and had a carbuncle on his thigh.

If we may be pardoned an apparent digression, we would remark, that great writers have so instinctive a perception of reality, that even when they do not draw their descriptions from facts, yet facts seem afterwards to follow their descriptions; so that sometimes Smollett copies nature, and sometimes nature seems to copy Smollett. The poor-law dictators and their understrappers will excuse us, therefore, if we observe, that when they make the sick and dying work till they drop, they are not altogether original; the same feat is to be found recorded in the pages of *Roderick Random*. Captain Oakham, of the *Thunder*, is so indignant at finding that there are no less than sixty-one sick on board his ship, that he determines to diminish the list, and accordingly reviews all the patients on the quarter-deck. They crawl to the place appointed, and as it is settled by the surgeon, under the direction of the captain, that there is nothing the matter with most of them, they are sent back to work.

"Some gave up the ghost in the

presence of their inspectors; and others, who were ordered to their duty, languished a few days at work among their fellows, and then departed without any ceremony."

The third case is wonderfully to the point, and might almost make one forget dates, and imagine that the novelist physician was satirizing a union workhouse:—

"The third complained of a pleuritic stitch, and spitting of blood, for which Dr. Mackshane prescribed exercise at the pump to promote expectoration; but whether this was improper for one in his situation, or that it was used to excess, I know not, but in less than half an hour he was suffocated with a deluge of blood that issued from his lungs*."

In the language of the evidence at the inquest, death was caused "by the sudden departure of the fluid into the windpipe!"

On hearing Read's account, the witness advised that Morris should be employed at lighter work, and that if he grew worse he should be sent to the surgeon. On Saturday, the 18th, witness was in town on duty all day. On Monday night (the 20th), at half-past ten, he called upon the witness. He said that he had been in bed since Saturday, and had been spitting blood. He had an order of admission to the workhouse. The witness, in answer to some questions, put by Mr. Field, clerk to the Board of Guardians, now says:—

"They had no room in the workhouse for the deceased; they are about to build a larger workhouse. The deceased might have gone to the overseer, or in a case of emergency the master of the workhouse can admit a person."

These answers, squeezed out by Mr. Field, are to us quite incomprehensible; for it appeared before that originally the deceased was refused admission on ac-

count of the Board having rejected his application, and not for want of room; and on Monday night, when he had an order, he *was* admitted.

Thomas Read, the superintendent of the mill, deposed that on Friday, the 17th, he made the remark, that the deceased was too ill to work. On that day he had a loaf of bread, and 8d. for his day's work. "About ten o'clock on Saturday, witness's attention was called to him. He scarcely worked a quarter of an hour."

Unless, therefore, the reporter has made some strange mistake, it would seem that after the patient had utterly broken down in the vain attempt to work on Friday, the experiment was renewed on Saturday! Why, this is a step beyond Captain Oakham. "The deceased," adds Read, "came to work about seven o'clock on Saturday morning." On this, the coroner observed, "that when the man was so bad, the surgeon ought to have been applied to." We have next a pauper, who deposes to the deceased tottering as he came along the room where the mills were; and the master of the workhouse, who *now* knows that in an urgent case he can admit a person without an order. The verdict was, "The deceased died from influenza; and the jury cannot separate without recommending, that in all cases of a similar nature prompt measures should be adopted."

Our readers will agree with us, that a more deplorable case, or one less creditable to the parties concerned, has very rarely come to light. Callous, indeed, must be the heart that can weigh the savings produced by the new system against such a catastrophe as this, and exult in the balance of profit! Flimsy, indeed, must be the head which fails to perceive that these melancholy incidents (now, alas! so common) are the plain and inevitable results of the

* Roderick Random, chap. xxvii.

instructions issued by the commissioners, and of the bill on which those instructions are founded. You may know the tree by its fruits, and trace the genuine character of the new edicts in the horrible waste of life to which they have led:

— "scires è sanguine natos!"

THE LATE CASE OF BAYONET WOUND.

To the Editor of the Medical Gazette.

SIR,

IN obedience to my promise, I inclose some observations on the case of bayonet wound, lately brought to University College Hospital. Most of them were delivered last week to the students attending my lectures on surgery, by whom they were received in a style that must have convinced all present, that not a single member of the class is an admirer either of misrepresentations, or of the pretensions of a man to be a judge of surgery, whose ignorance of it is here so fully exposed.

I am sir,

Your very obedient servant,
S. COOPER.

7, Woburn-Place,
Nov. 26, 1837.

Addressed to the Surgical Class of University College at the close of the Lecture on Punctured Wounds, on Tuesday Evening, Nov. 21, 1837.

GENTLEMEN,

IN some numbers of the *Lancet*, recently published, a charge is brought against me of having neglected a man, who lately died of a bayonet wound, in University College Hospital. By a fortunate concurrence of circumstances, this accusation was made only a few days before it became my duty to deliver the lecture which you have just heard on punctured wounds; so that I am enabled to notice the matter on what appears to be a most suitable occasion.

At first I had doubts whether the false, contradictory, and ignorant assertions, made by my assailant, were worthy of any notice, as coming from the foul quarter which they did, and more particularly as in these days every man holding public

appointments, like mine, ought to avoid being too sensitive, or, (as I have heard it expressed) too *thin skinned*. I contemplated also the well known despicable character of the publication, and the little importance which really belongs to any statements made in it. Just as it is desirable to avoid fighting with a chimney-sweeper, not only because there is no honour to be gained by a victory over him, but because any contact with his dirty hide will be sure to soil; so it must generally be expedient to avoid any *rencontre*, or dispute, with a writer, who has nothing to lose, and who, probably, if he had the choice, would prefer any kind of exposure to the decision to leave his aspersions unnoticed.

Some years ago, gentlemen, I used to take in the *Lancet*; but perceiving that it was really itself a *take in*, that one odious purpose of it was to abuse, one after another, many of the most distinguished men in the profession; and that the trash and falsehoods doled out by it every week, instead of being worth any silver coin, were not worth a brass farthing, I soon determined not to be numbered amongst the dupes, who promoted it at the expense both of money and principle. Gentlemen, I can have no hesitation in declaring it as my sincere belief, adopted long prior to the present little *chimney-sweeper* skirmish, that the *Lancet* is a publication completely undeserving of the countenance of a liberal and gentlemanly profession; in which its chief aim is to excite jealousies and discord; to raise undeserving parties into notice; and, by means of falsehood, base insinuations, and the perversion of facts, to lower the characters of almost all the hospital surgeons in London. If one or two of them are made exceptions, in return for value received in the shape of clinical and other communications; if one or two of them are, for the present, not only suffered to continue unmolested, but are actually puffed up to the skies, the day will not be less surely come when their guardian angel will no longer lend them his wings, and they must then either continue to fly of themselves, or fall, like the late poor victim of a balloon-trip, never to rise again.

Gentlemen, as your friend and teacher, I will take the liberty of offering you a few cautions with respect to this publication: place no confidence in it. Look with suspicion on the motives and principles which actuate it. They are not, perhaps, very difficult to scan, even on the present occasion. Waste not a moment of your time, or a farthing of your money on it; and be assured that if you ever suffer your minds to be drawn away from the quiet pursuit of science, and of prac-

tical information, by the glaring nonsense and misrepresentations which it contains, you will never attain eminence or respectability in your profession. I am happy in feeling convinced, from the manner in which you have received my advice and remarks, that you all understand the tricks and roguery of the *Lancet* as well as I do.

Now for an illustration of the mendacity, inconsistencies, contradictions, and wilful misrepresentations of the celebrated agitator of our profession.

"The points of most importance," says he, "are, that the case was *not treated by any one of the surgeons of the hospital*; that symptoms of strangulated intestine set in; and that the cause of the strangulation was not removed.

"On examining the body after death, it was found that the bayonet had penetrated the cavity of the abdomen obliquely downwards, the point of the instrument having just reached the wall of the rectum. *A small quantity of blood was effused into the abdominal cavity*; a knuckle of intestine was strangulated in the external wound." It is added: "*The immediate cause of the man's death evidently was strangulation of the intestine, and not internal hemorrhage or inflammation*!"

Who would believe that the writer of these statements had actually sent his reporter to copy my clinical notes, about a week before the foregoing assertions were published? and that in those notes the following particulars were recorded:—

"When I first saw the patient, between 10 and 11 o'clock of the forenoon following the night of his admission, I found the external wound closed, and a soft swelling behind it, *which, on gentle pressure being made, slowly and gradually returned*." I was then unable to decide whether the swelling consisted of intestine or effused blood.

"The man had been in a state of collapse, with cold skin and cold extremities, a pulse scarcely perceptible, and a pallid cadaverous countenance from the first. Such condition never ceased, and implied the wound of some important organ, with or without effusion of blood, or other matter, into the cavity of the abdomen." I frequently observed, on the same day, *in the course of which I visited the patient not less than three times*, that the pulse and other symptoms indicated extravasation of blood, rather than intestinal effusion, though probably the intestines might also be wounded.

In the record of the *post-mortem* examination, it is stated: "*the cavity of the abdomen was found extensively occupied by effused blood, which amounted altogether to three or four pints. The intestines and peritoneum were*

highly vascular, and in some places lymph was poured out, so as to render the intestines adherent to one another." It is true that a knuckle of intestine had insinuated itself into the wound again; but this had happened subsequently to its reduction in the morning, and probably from the efforts of vomiting. When this piece of intestine had been disengaged, it was found to have been transfixed by the bayonet, and without such lesion of it having been at all discernible in the examination of the inner orifice of the wound, or of that in the muscles under the skin.

"In the progress of the dissection, a wound of the rectum, and another of a vein going into the internal iliac, were detected," whence the copious extravasation of blood had proceeded.

The patient died in less than twenty-four hours from the period of his admission, never having revived from the general collapse. *He had had one motion from a clyster*; and was occasionally troubled with vomiting. When I visited him, about two hours before his death, the sickness had been appeased by opium, and this notwithstanding the swelling had made its appearance again in a diminished form; and as, from the feel of it in the morning, it was doubtful whether it might not be blood, and the man was sinking fast, it was not judged advisable to interfere with it.

Mr. Tayloe, the house-surgeon, justly observes: "The man *died principally and inevitably from the copious extravasation of three or four pints of blood in the abdomen, and the peritonitis thereby excited*. The fact was proved by the trivial change in the condition of the knuckle of intestine entangled in the wound, and the violent inflammation of the peritoneum, more especially in those situations where the blood had accumulated. As Mr. Cooper caused the body to be opened in the hospital theatre, in the presence of Mr. Quain, Professor Carswell, and about 200 individuals, your statement that the man *died of strangulated hernia, and that only a little blood was found in the abdomen*, is at all events known by these numerous witnesses to be incorrect."

Considering, then, that the reporter of the *Lancet* had access to my notes, and actually copied them several days prior to the publication of the foregoing misrepresentations, I may safely leave the world to decide whether the mendacity of its editor has been accidental or intentional.

I have next to prove his ignorance of surgery and pathology. He says, "the immediate cause of the man's death was evidently strangulation of the intestine, and not internal hæmorrhage or inflammation." Like a perfect ignoramus in practical matters, with which I conclude that he

can have had little to do*— * * * *
 * * * * He seems here to be ignorant of the fact, that if the man had really died of strangulated hernia, *death would still have been caused by inflammation.* But, together with that inflammation, there would also have existed certain changes in the protruded intestine, tympanitic distention of the bowels between the stomach and the stricture, a contracted state of the intestinal tube between the stricture and the anus, less severe inflammation in this direction, &c. Now none of these circumstances characterised the case before us, as they would have done, had the protrusion had any share in producing death.

The same ignorance of practical surgery, I remember, was a candidate a few years ago for the office of coroner (great laughter); a place for which he conceitedly appears to fancy himself superiorly qualified on account of his fine *medical abilities.* He rated himself higher however than the public did, and notwithstanding his harangues from carts and ladders, so flattering to the multitude, and altogether so appropriate, he was thrown out. Yet he still fancies himself well cut out for this office of coroner, and in his last number gives a sample of what his talents would be in this way, by putting a few searching interrogatories to me, which, in reality, only manifest still further his own professional ignorance. Amongst them I find this one, "Would mere effusion of blood have produced constant vomiting and constipation?" Constipation indeed! Why the man, as the note-book has recorded, had one motion during the short time he lived: how many would this sapient coroner have desired? According to the rule of practice, no purgative was given, from an expectation that the intestine was wounded.

As for constant vomiting it did not exist; there was occasional vomiting: but *our mock coroner is fortunately ignorant that an effusion of three or four pints of blood in the abdomen, and a wound of the alimentary canal in two places, if he should ever happen himself to meet with these lesions in his political squabbles, would not leave even his stomach, which, I presume, is not a very delicate one, in a perfectly quiet state.* "Where ignorance is bliss, 'tis folly to be wise."

As for some of the *ifs* and *suppositions* which accompany some of his other ridiculous questions, they appear to have been stolen from my clinical lecture, and this will be a sufficient apology for my not commenting on them. Other points have been effectually replied to by Mr. Tayloe.

SECOND REPORT

OF THE

LONDON COMMITTEE OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE,

ON THE

MOTIONS AND SOUNDS OF THE HEART;

Communicated at the Meeting at Liverpool, September 1837.

[For the Medical Gazette.]

THE Committee appointed in London to investigate the motions and sounds of the heart, have to present to the Association a short account of some investigations of the abnormal sounds of the heart and arteries, in which they have been recently occupied, and which were not comprehended in their former Report.

Before describing these, the Committee would remark, that although their inquiries have not been specially directed to that subject, yet they have had many opportunities of verifying the conclusions on the *natural* sounds, as presented in their Report of last year; and these conclusions not having been since shaken by any experiment or rational objection, it may be considered as fairly established, that the first, or systolic sound of the heart, is *essentially* caused by the sudden and forcible tightening of the muscular fibres of the ventricles when they contract; and that the second sound, which accompanies the diastole of the ventricles, depends solely on the reaction of the arterial columns of blood on the semilunar valves at the arterial orifices. It further appears, that the first sound may be increased by an additional sound of impulsion against the walls of the chest under certain circumstances of posture, of increased action of the heart, and of particular stages of the respiratory act. It is also obvious, that the character of the first sound may in some measure depend on the closure of the auriculo-ventricular valves, and on the quantity of blood, inasmuch as these determine the nature and time of the resistance, against which the muscular fibres of the ventricles tighten. So likewise the vigour of the ventricular systole, the quantity of blood propelled by it, the sudden and complete character of the diastole, the fullness of the arterial trunks, as well as the perfect, mobile, and membranous condition of the semilunar valves, will determine the character and loudness of the second sound. An experimental illustration of one of these conditions was observed by the Committee during one of their experiments on the ass—a great diminution of the second sound on the carotid artery being freely divided.

* We have omitted a reference here made by Mr. Cooper to an event not connected with the subject under discussion.—*Ed. Gaz.*

As additional illustrations of the production of a sound like that of the heart, by muscular contraction, the Committee have noticed that accompanying the action of the panniculus carnosus of the ass during life, and the quivering contraction of muscles immediately after death. The sound produced in the latter case, in nature and frequency, closely resembled the first sound of the heart of the fœtus or of small animals.

In investigating the morbid sounds of the heart, the attention of the Committee has been chiefly directed to the causes of those remarkable and various accompaniments of the heart's action, called murmurs, which were happily compared by Laennec with the noises of blowing, filing, rasping, sawing, purring, cooling, &c. This inquiry consists of two parts.

1. *What is the essential physical cause of the phenomena in question?* and 2. *How does the apparatus of the circulation develop this cause in the various instances in which these phenomena are known to occur?* To the first of these inquiries, the experiments of the Committee supply what they trust will be deemed a satisfactory answer: the second is to be answered by extensive clinical and pathological observation, rather than by direct experiment; and although a few physiological experiments will be quoted to this point, yet the Committee do not profess at present to do more than open this inquiry to all those who have the means of pursuing it.

Experiments on the production of Sound by the motion of Water through a Tube.

1. A caoutchouc tube, 18 inches long, and 3-8ths of an inch in diameter, was attached to the stop-cock of a reservoir, in which there was water to the depth of from 8 to 10 inches.

When the water flowed perpendicularly through this tube (the air being first expelled*, and the lower end of the tube kept under water in the recipient below), no sound was heard; but on pressing any part of the tube, so as to diminish its calibre, a blowing sound was heard at and below the point of pressure, and this sound became louder and more whizzing as the pressure was increased. The loudest sounds were obtained at the lowest end of the tube, where they were sometimes quite musical; and by increasing the pressure at regular intervals, a periodic increase and raising of the sound was produced, which closely resembled the sound heard in the

neck, to which the French have given the name of "bruit-de-diable."

2. A pin being stuck transversely through the tube, a slight blowing was heard. A similar phenomenon more distinctly resulted from the use of a split goose-quill placed in the same way. A stronger blowing was produced by two threads across the diameter of the tube, especially when they were rather loose; and a still louder and shriller sound ensued when a knot of string was fastened to the threads.

3. The same tube being adapted to the stopcock of a water supply-pipe, through which the current could be let to pass with great force, it was found possible to imitate every variety of blowing, whizzing, and musical murmurs, by varying the pressure on, or obstruction in, the tube, and by altering the force of the current. When the current was strong, the least obstruction caused a murmur, but with weaker currents, greater obstructions became necessary for the same effect. An obstruction, which, with a weak current gave a blowing sound, produced with a stronger current a sound of a more whizzing character. Grating or rasping sounds were best obtained by the effect of a strong current on a knotted thread across the diameter of the tube. The musical or uniform sounds resulted from a moderately strong current through a considerable impediment; increasing the force of the current or the degree of obstruction, rendered them whizzing and imperfect; diminishing the current or the obstruction, converted them into a simple blowing. When a sound was of an appreciable pitch, its note was high in proportion to the force of the current and the amount of the obstruction,—a fine forcible stream producing the highest note. Sometimes, however, with a strong current, a loud trumpet-note would be set up, which was not altered in pitch, but only in force, by changing the strength of the current. This kind of note produced visible vibrations of the tube below the obstruction, and seemed to have relation to the length of the tube. In many instances these vibrations resembled closely the purring tremor and thrilling vibration sometimes felt in the region of the heart and large arteries. Musical sounds of a more variable character, like the cooing of a dove, the humming of an insect, or the whistling of wind, were produced with weak currents passing through a tube much obstructed. The pressure of a column of water only two or three inches high was sufficient to give acute whistling notes, which were sustained, although varying, even when the water that passed only fell in drops from the end of the tube.

* As long as any air remains in the tube, a loud crepitation accompanies the current. In most of these experiments a flexible ear-tube, without its cone, was used, as being more easily applied than the common stethoscope.

4. Bending the tube to an angle produced a murmur; but no sound resulted from any curve that did not infringe on the calibre of the tube. A circular constriction, by a thread drawn round the tube, caused a murmur, which was blowing or whizzing according to the strength of the current.

5. The current issuing from the end of a tube, or from the mouth of an India-rubber bottle, produced a blowing sound when it impinged directly on an opposite surface, such as the side of the recipient or the end of the stethoscope; but unless the current were very strong, this sound was not produced when the current played on the surface very obliquely.

6. When the tube, with a weak current, was pressed on at two points, the murmur was heard at the point where the pressure was greatest; and by increasing the pressure at one point, the sound was stopped at the other. When the current was strong, it was easy, by a pretty equal pressure, to cause a murmur at both points.

7. With a strong caoutchouc tube, two feet long, and one inch in internal diameter, the same results were observed, but in a more remarkable degree, in consequence of the increased size of the tube. When the current was strong, and the pressure on the tube considerable, sounds were produced loud enough to be heard without applying the stethoscope to the ear, and the vibrations of the tube below the obstructions were so strong that they threw the water in little jets from the outside of the tube.

8. In making the last experiment, the pressure of the water suddenly distended a portion of the tube into a globe about three inches in diameter, constituting a good representation of a circumscribed true aneurism. As long as the force of the current was sufficient to keep the walls of the dilated portion distended, no murmur was produced; but when these walls became flaccid, the passing current caused a kind of dull fremitus in them. Slight pressure on the dilatation or bending the tube to form an angle at the point, also sometimes occasioned a murmur.

9. A globular India-rubber bottle, three inches in diameter, being adapted to an aperture in the side of a tube, so as to form an elastic sac communicating with it, and the air being expelled, the current of water was directed through the tube. The same was done with a smaller tube, and a bottle one inch and a half in diameter. In some positions the current, in passing the lateral sac, occasioned a slight whizzing; but in others, as when the tube was straight, there was no sound. A sudden increase of current, or the removal of pressure from the sac,

caused a whizzing, by the entry of water into the sac. Independently of the current, sudden forcible pressure on the sac occasioned a whizzing with the expulsion of the fluid; and a similar whizzing attended its rapid reflux into the sac on the removal of the pressure.

10. Some of the preceding experiments being repeated with water made slightly glutinous with size, it was found that the various sounds were not quite so readily produced as with plain water, and required a greater force of current.

From all these results (1, 2, 3, 4, 5, 6, &c.) it is sufficiently plain that a certain resistance or impediment to a liquid current is the essential physical cause of all murmurs produced by the motion of fluids in elastic tubes. That any condition of the walls of the tube beyond the obstructing point is not, as it has been supposed, essential to the production of these sounds, is proved by the fact, that they may be produced by an obstruction at the *terminal orifice* of the tube, or at the *mouth* of a gum-elastic bottle, where there is *no tube or wall beyond* to cause them (1, 5); and usually this is the situation where they can be produced best, because here the current has acquired its greatest momentum, and finds a freer passage beyond the obstructed point. The more flaccid state of the portion of a tube beyond an obstruction, is a necessary effect of the impediment caused by that obstruction to the passage of water. It is therefore a necessary concomitant of the obstruction, but it is not the *cause of the sound*. When, however, the sound occasioned by the obstruction is strong, its vibrations may be communicated to the whole contents and walls of the tube beyond (3, 7,) which will then vibrate in system with it, and be capable of modifying its note; just as the tube of a reed instrument *affects* the note which is *generated exclusively in the reed*. On the other hand, when the sound is weak and varying, the condition of the tube or walls beyond it will not affect it. In short, the laws of the production of sound by liquids, so closely resemble those which regulate the same phenomena in air, that it is unnecessary to enter into further detail respecting them. It may be necessary to advert to an objection to this view, that a murmur is sometimes caused where there is no impediment to the course of a liquid, as when it passes suddenly from a small into a large tube or into a sac. In the first place, it is not quite correct to assume that in this case there is no impediment; for the liquid in the large tube or sac having less velocity than that in the small one, must itself be an impediment. But besides this, the course of the small swift current becomes changed by spreading into

the larger channel, and instead of running smoothly parallel to the tube, now strikes its walls at an angle, causing a series of impulses and resistances which, if forcible and rapid enough, constitute the vibrations of sound. It is in the same way that a current produces a sound by impinging against an opposed surface (5.) It may be, observed, however, that these indirect kinds of impediment to a moving current, are not so constantly attended with the production of sound, as the direct obstacle presented by a narrowing of, or projection into, the calibre of the tube.

Experiments on the production of Murmurs in the Living Body.

11. About two inches length of the common carotid artery of a young donkey was laid bare. Different degrees of pressure, either by the stethoscope, or by a probe pushed under the artery, occasioned a variety of murmurs, blowing, sawing, filing, and musical or cooing, at each pulse. When the stethoscope was merely placed in contact without pressure, no murmur was heard, but only a simple impulse and sound, which was distinct only when the heart acted strongly.

12. The artery was scratched for a few seconds with the point of a scalpel; it gradually became sensibly smaller for the length of half an inch about that point. A strong solution of salt being applied, the contraction increased; but it was still of a gradual and tapering kind, and the stethoscope could not detect any murmur in it; very slight pressure was enough to cause a whizzing. The pulse at this contracted portion was felt to be much harder and sharper than above or below it.

13. A small incision being made into the artery, a jet of blood issued, and a whizzing, sometimes continuous like the bruit-de-diable, sometimes only in pulses, was heard beyond the orifice, but no sound on that side of the orifice nearest the heart; the sound being, as usual, carried in the direction of the current. The incision being made larger, the blood spouted out with whizzing to the distance of more than six feet, and the animal died in the course of ten minutes. After this last incision the beats of the heart were frequent, short, and pretty loud, but without a second sound, and without any murmur to the last. They continued for nearly two minutes after the respiration and consciousness had ceased, becoming gradually slower.

14. The Committee repeated the observation that has often been made before, that a murmur can easily be produced by pressure on the subclavian, carotid, or femoral artery of the human subject. This murmur is generally of a grating or

filing character, and is prolonged in proportion to the degree of pressure.

15. Whilst making the observations on the carotid, they found that a continuous murmur of very remarkable and variable characters could be produced by pressure on the jugular veins. The most common sound thus produced was like the humming of a gnat or fly, but occasionally it resembled the whistling of wind, the singing of a kettle, the cooing of a dove, and sometimes it was perfectly the remittent whirring noise, which the French have called the bruit-de-diable. Dr. Ogier Ward, of Birmingham, had previously come to the conclusion that this sound is produced in the jugular veins; and the observations of the Committee confirm this inference; but they do not agree with this physician in the opinion which he adopts from MM. Andral and Bouillaud, that the presence of these sounds always denotes a chlorotic state of the system, in which steel is indicated, or that they are essentially morbid symptoms at all. They may be produced in the healthiest subjects by moderate pressure applied to the lower part of the jugular veins, and are then found to be modified by various circumstances, which can only affect the venous current. Thus, they may be arrested or diminished by pressure on the vein above, by the horizontal posture, or hanging down the head, and by forced efforts to expire with the glottis closed. They may be restored in increased degree by suddenly desisting from any of these acts or circumstances. The occasional pulsatory or remittent character of these sounds seems to depend on the momentary increase of pressure caused by each pulse of the neighbouring artery; and when, as it sometimes happens, these pulses are attended with a whizzing, this is in a measure incorporated with the venous sound, and increases its periodic swell. The size and downward current of the jugular vein peculiarly adapt it for the production of sound, but it is probable that sounds may be produced by pressure on other veins, when circumstances accelerate the current within them. The Committee have succeeded in detecting an obscure murmur in some of the large superficial veins in the arm and thigh. It is to be distinguished from muscular sound, which it resembles, by its being heard only when the small end of the stethoscope is applied on a vein, and by its being stopped by pressure on that vein. A louder continuous sound is sometimes heard at each side of the upper part of the sternum, which, from its resemblance to these venous sounds, and from its being stopped by forcible expiration, may be supposed to have its seat in the large venous trunks underneath.

Although it appears from these facts that the venous sounds are not necessarily signs of disease, yet the circumstance proved by the Committee (10), that water is thrown into sonorous vibrations more readily than a liquid of a more glutinous character, renders it probable that these, and other sounds depending on the motion of liquids in the apparatus of the circulation, may be more easily produced where the blood is thin and deficient in quantity; and under these circumstances they may occur in the neck from the mere pressure of the muscles on the jugular veins.

The Committee had planned several experiments for the further elucidation of the second part of the inquiry, namely, *by what changes, functional and structural, does the apparatus of the circulation develop the physical causes of abnormal murmurs and sounds, in various instances in which they are known to occur?* Having failed in obtaining animals in time for this Report, the Committee propose to resume at a future time this part of the inquiry, so important for the elucidation of several obscure points in pathology, diagnosis, and practice; and to report the result of their labours at the next meeting, if the Association should think fit to reappoint them for that purpose*.

(Signed)

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ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Nov. 28, 1837.

THE PRESIDENT IN THE CHAIR.

Removal of the Clavicle, with a Tumor situated in that bone. By BENJAMIN TRAVERS, F.R.S., Senior Surgeon to St. Thomas's Hospital.

THE patient, in this case, was a young gentleman, æt. 10, a native of the East Indies, but resident for the last five years in England. In the summer of 1836, a swelling, of the size of a hedge-nut, was observed in the centre of the left clavicle, which was attributed to a fall he had had ten days previously, and treated accordingly. At about two months after this discovery the author was consulted, when the swelling had attained the size of a pigeon's egg, was elastic, and painful when compressed. Various means were

had recourse to with the view of dispersing the tumor, but without avail, as by May, in the present year, its base, from the scapular extremity, occupied 3-4ths of the bone. About 2-3ds of its bulk were supraclavicular, so that in the erect posture it was seen far behind. It being determined to remove the bone with the tumor, the operation was performed on the 6th of June, twelve months after the commencement of the disease. The operation was effected without difficulty, and no considerable blood-vessels were injured. The entire clavicle was removed, with the exception of a small portion of its sternal extremity. At the date of the present narrative (Oct. 1837) the wound had been very completely cicatrized; there was no falling forward of the shoulder, nor any restriction of the motions of the arm: indeed, such facility does the patient enjoy in this respect, that one of his amusements is rowing a boat upon the Thames. There is a production of bone from the truncated extremity of the clavicle, extending at least two inches, and terminating beneath the centre of the cicatrix. A section of the tumor, in its longest diameter, presented an arrangement of cells filled with a dark solid coagula of blood.

The author conceives that there can be no reasonable doubt of two points, viz. that the tumor began in the cellular structure, and that it was the result, direct or indirect, of the fall. The explanation offered by the author is, that a medullary extravasation had taken place from the concussion attending the fall, or from the fracture within the periosteum, in either of which cases the effused blood which, in a perfect solution of continuity, would have been absorbed, acted as a foreign body; and by effectually stopping the osseous secretion and starving the bone, became the instrument of the absorbing process. He is induced, by the phenomena of such cases as the present, to entertain an opinion, that many anomalous fixed tumors on record arise from changes within the medullary canal of the bones, and are exasperated into intractableness by their confined situation. The paper concludes with a categorical summary of the diseases of bone, which the author's observation and the records of surgery enable him to verify.

ST. BARTHOLOMEW'S HOSPITAL.

Tuliocotian Operation.

SARAH JACKSON, æt. 20, a native of Hertford, came up to St. Bartholomew's Hospital, in the beginning of last August, to

* The Committee was reappointed, and £25 placed at their disposal for the further prosecution of these researches.

undergo an operation for the reparation of her nose, which had been destroyed three years previously, by lupus. The disease commenced when she was a child, eight years old, on the right cheek, and gradually extending its ravages to the integuments of the alæ and septum of the nose, destroyed the cartilages of the apertures, and removed the whole of the integument as high as the triangular portion, which it left subcutaneous. A small part at the anterior and posterior part of the fleshy columna still remained. After a time, the left side of the face became similarly involved, and medical aid was unable to arrest the progress of the disease for a period of eight years. The face at this time presented an irregular rugous appearance, much thickened from a deposit of lymph, with patches of bright-red vessels ramifying in the skin, and had an unhealthy aspect, although quite free from any appearance of the disease itself. She was admitted into the hospital, under the care of Mr. Stanley, who, after a careful examination, fearful of the operation again lighting up the original disease, refused to subject the patient to the liability of its return.

In this conclusion he was somewhat strengthened by Professor Dieffenbach, of Berlin, who visited the institution about this time. He was of opinion, that although it was an unfavourable case, there were still sufficient grounds to warrant the performance of an operation. Personal appearance being an important consideration with the young woman, she was importunate in her solicitations to take advantage of the opportunity surgical means offered; and after being duly informed by Mr. Skey of the liability of failure (to whose care she had been previously assigned), the operation was determined on, and performed on Saturday, the 21st of October.

The first step consisted in paring off the skin on both sides the alæ, over a surface of about one-third of an inch wide; to which treatment also the anterior and posterior part of the columna nasi were subjected. A perpendicular incision was then made down the bridge of the nose, commencing a little above the junction of the frontal with the nasal bones, and the sides were reflected sufficiently to admit of the insertion of the stalk of the flap brought down from the forehead. A low state of the forehead rendered it necessary to divide the flap of integument obliquely; the direction of it, therefore, was upwards, outwards, and to the right side. It was of a crucial shape, consisting of three portions situated at right angles

to each other: a perpendicular portion, or tongue, from half an inch to three quarters of an inch in diameter, to be applied to the septum, and two lateral ones, of rather larger dimensions, to be adapted to either alæ. The meeting of these formed the prominence of the nose. The tongue, or perpendicular portion, advanced close upon the hair; and the transverse ones extended from the upper part of the eyebrow below, to the hair also above. A thin stalk, of an inch and a quarter long, and a quarter of an inch broad, was then carried down to meet the incision made along the dorsum of the nose. A little smart bleeding followed, and occasioned a delay of about a quarter of an hour. As soon as this was repressed, the stalk was twisted upon itself towards the right side, introduced between the two reflected portions on the dorsum of the nose, and kept in its situation by ligatures applied at proper intervals. The same method was also employed to confine in their proper situation the alar and columellar portions.

At eight o'clock the same evening the transplanted piece of integument was quite warm, but rather pale. She was conscious of its being touched, and strange to say, referred it, not to that part of the forehead from whence it was reflected, but to the very identical spot on the nose on which it was fixed. The ligatures were removed on the third day. The whole of it had united, except a small piece just at the meeting of the left alar portion with the cheek, which afterwards filled up by granulation, and the junction of that part going to form the septum with the upper lip. No attempt has been made again to approximate this; it is just sufficiently pendulous to appear natural, and no separation is observable without looking carefully underneath. The only artificial means resorted to have been the introduction of some flue of dry lint up either nostril.

In a month after the operation was performed the twisted part of the stalk was divided, and a small elliptical piece removed, to take away the projection it produced. It is remarkable how much the new part has contracted; what appeared at first a superabundant bulging mass is now converted into an aquiline organ of ordinary dimensions, and, as far as one can judge, corresponds as near as possible with what would have been the shape of the healthy one.

The patient is still in Sitwell's ward of St. Bartholomew's Hospital.

ACCIDENTS ADMITTED AT THE
LONDON HOSPITAL.Mr. LUKE, *Surgeon*.Mr. CURLING, *Assistant-Surgeon*.

Nov.	Sex.	Age.	Case.
14.	M.	44	Extensive ecchymosis of the scrotum, from a blow.
	F.	13	Contused head.
	F.	22	Contused side.
	F.	49	Contused hip.
15.	M.	16	Fractured tibia.
	M.	14	Fractured tibia.
16.	M.	60	Contused foot.
	M.	33	Contused ankle.
	M.	28	Fractured clavicle.
17.	M.	29	Retention of urine.
	M.	65	Contused elbow.
	F.	36	Scald on the arm.
	F.	21	Sprained ankle.
18.	M.	21	Contused leg.
	M.	14	Contused side, with extensive extravasation (a).
19.	M.	12	Bite on the nates by a dog.
	F.	40	Severe burn on the face and hands.
20.	F.	14	Contused knee.
	M.	46	Retention of urine.
	M.	13	Contused face.
21.	M.	17	Fracture of bones of the forearm, and contused hip.
	M.	23	Concussion, and various contusions.
			In-patients 22
			Out-patients 33
			—
			Total 55

(a.) This boy was thrown down and kicked by a horse. When admitted, there was a considerable swelling in the situation of the right pectoral muscle and axilla, which had a fluctuating feel. No pulse could be perceived in the right humeral or radial arteries, and the forearm felt cold. On the following day the swelling was not so prominent, but appeared more diffused, and occupied the back, near the posterior boundary of the axilla. The swelling and discoloration, which were clearly caused by extravasated blood, are now disappearing; but the pulse, which has always been distinctly felt in the left arm, is not yet (Nov. 27th) perceptible in the right. Whether the absence of the pulse on the injured side was owing to the pressure on the main artery, caused by the large quantity of extravasated blood, or to obstruction consequent upon some lesion of the vessel itself, or whether any irregularity existed before the accident, could not be clearly made out. The first supposition is improbable, since, now that the

tumefaction is almost entirely subsided, no pulsation in the principal arteries of the limb can be detected.

CASE OF UTERINE HEMORRHAGE
CURED BY ERGOT OF RYE.

A WOMAN, aged 24, of very weak constitution, was admitted into St. Thomas's ward, at the hospital of La Pitié, under the care of M. Piorry, on the 21st of July, 1837. After having been in a sickly state during the whole period of pregnancy, she had been brought to bed, a month before her admission, of a fine child, which was her first. The delivery was not very laborious for a first case, and was terminated by the efforts of nature alone. The catamenia have always been regular, and she has never had any uterine affection. Ever since her delivery she has continually lost blood from the genitals, and these floodings have gone on increasing, till the patient determined to enter the hospital. The blood discharged is generally fluid, but is sometimes mixed with black clots.

The general state of the patient, at the time of admission, was satisfactory, with the exception of the pulse, which was 110.

On placing the hand upon the hypogastric region, there was nothing irregular, excepting that the uterus seemed rather larger than it ought to be a month after parturition.

On percussion, there was resonance and elasticity of the abdomen, and a dull sound followed stronger percussion in the situation of the uterus. On examination *per vaginam*, it was found that the cervix of the uterus was dilated and gaping, so as to allow of the introduction of the first phalanx of the fore finger. The cavity of the cervix was partly occupied by a tumor of the size of a hazel-nut, and of irregular surface, which projected into the vagina. On tracing the tumor with the finger into the interior of the uterine cavity, it was easy to perceive that its size increased in proportion as the finger advanced farther. It could be circumscribed with the finger, so as to ascertain that it adhered neither to the neck nor to the most sloping part of the uterus.

Diagnosis.—A foreign body in the uterus; either a polypus, a portion of placenta, or organized clots of blood.

Prescriptions.—Twenty-four grains of ergot of rye, in two doses; emollient injections; the use of the bidet; barley water; a basin of soup.

July 22.—The patient has had shivering fits followed by heat, and colic during the night. On examination, it was found that no change had taken place with respect to the tumor: the neck of the

uterus and the external parts were extremely flaccid. Forty-eight grains of the ergot were prescribed, and quarter diet; the rest as on the preceding day.

23d.—No shivering. After the administration of the last dose of the ergot, the colic returned with more violence than on the preceding day. On examination, it was found that a larger portion of the tumor projected through the os tincæ; so that it appeared that an attempt at expulsion was beginning. By the help of the speculum, the tumor was seen to be greyish. A drachm of the ergot was prescribed; the rest as on the preceding day.

24th.—The uterine hæmorrhage and the colic continue; the os tincæ is not so open as yesterday; no other change: same prescriptions.

25th.—The patient is cold; the pulse small; the tongue white. The hæmorrhage has ceased: instead of which there is a white discharge, which is sanious, thick, and fœtid. A drachm of the ergot, the bidet, and emollient injections, were prescribed; with two basins of soup as the sole nourishment.

26th.—The tumor is larger, and projects more than it did yesterday. Prescriptions: a drachm of the ergot, a vapour bath, emollient injections, and quarter diet.

29th.—The patient is weak; her face has a worn-out look; the leucorrhœa continues, but is less than on the preceding days. Prescriptions: forty eight grains of the ergot, with four grains of subcarbonate of iron, and three-quarters diet.

30th.—A substance like an organized clot of blood has come away from the neck of the uterus; it is of the size of a large walnut, soft, and easily torn. Prescriptions: a drachm of the ergot; four grains of subcarbonate of iron; a vapour bath; and quarter diet.

31st.—The patient has suffered from severe colic; after which another substance, like the one of yesterday, was again expelled. Prescriptions: a drachm and a half of ergot; the rest as yesterday.

August 1st.—The colic continues as severely as before; nothing has been expelled from the uterus; the cervix is rather contracted; the patient has no discharge, either red or white, and, with the exception of the colic, does not suffer at all. Prescriptions: a drachm of the ergot, and four grains of subcarbonate of iron.

2d.—The colic has been accompanied by shooting pains in the loins, which the patient compares to those which she feels before the appearance of the catamenia. The os tincæ has completely contracted, so that the introduction of the fore-finger into the interior of the uterus is now impossible. Every thing combines to show that the uterus is entirely freed from the

foreign body which it contained. The general state of the patient is satisfactory; her physiognomy has improved; her complexion is returning; there is no discharge, except a slight leucorrhœa. Prescriptions: the use of the bidet; three-quarters diet.

3d to 8th.—The state of the patient is becoming more and more satisfactory; in fact, she has no pain. Prescriptions: the use of the bidet; twelve grains of carbonate of iron; half diet.

10th.—The patient requests to be dismissed, although the premonitory symptoms of the catamenia seem to be present. The leucorrhœa has entirely ceased; her general state is good. Cured.—*Gazette des Hôpitaux*.

INDECENT EXHIBITION IN CHARTER-HOUSE SQUARE.

To the Editor of the Medical Gazette.

SIR,

My attention was directed to an article in the MEDICAL GAZETTE of Saturday week last, on the subject of the offensive exhibition in Charter-House Square, to which you have referred. I am happy to find that an honourable medical tribunal denounces this system of publication, which, as regards the public, is highly indelicate, and is in no less degree offensive to the common and well-understood proprieties of medical society. When we reflect on the daily routine of medical practice, entailing as it does a necessary familiarity with scenes and circumstances often offensive to the natural and true English delicacy of either sex, we cannot but commend the good taste which, notwithstanding their familiarity with such scenes, enables our professional men to maintain the exterior of a natural refinement, and which rarely or never oversteps the modesty and decency of cultivated society.

Here and there we find a contrary example; and we have one before us. The individual in question has received abundant evidence, that his mode of publication is most objectionable and offensive. He has been told that it has too frequently raised a blush on the cheek of modest and respectable inhabitants, while the bold and unabashed vulgarity of the exhibition attracts the attention, and elicits the comments, of the numerous youthful members of resident families.

We are told that public law has overlooked an evil of this description, and that, through its instrumentality, we can obtain no redress. But surely the proprieties

of social life are not to be outraged with impunity, and I earnestly hope that you will not withhold from us your aid in the endeavour to remove an evil that has received the unanimous denunciation of every householder around.

It is stated that our complaint is founded on personal dislike to an individual; that a conspiracy exists among, at least, the medical inhabitants of the Square, having for its real object the ruin of the rising institution. Certainly, the conduct of the individual alluded to is not, in this instance, such as to claim much general respect,—and most assuredly have we a sufficient amount of provocation, without the necessity of inferring hostility to any object, having the *real* interests of the poor at heart, and for which idea of hostility there is not, as far as I know, the slightest pretence. At all events, whatever be the sentiments of the individual under whose fostering hand the institution has obtained its unenviable notoriety, surely its Governors at large would never willingly connive at so gross a breach of common decency. They might possibly be induced to consider, whether their ill-bestowed philanthropy may not be directed into some more profitable channel, on learning that the diseases, for the cure of which “the Infirmary” was instituted, are, as I am informed, the subject of daily treatment in every hospital in London.

I remain, sir,

Your most obedient servant,

AN INHABITANT OF

CHARTER-HOUSE SQUARE.

Nov. 27, 1837.

To the Editor of the Medical Gazette.

SIR,

THE observations in a late number of your journal, on the indecent mode of advertising resorted to by a surgeon in Charter-house Square, entitle you to the warmest thanks of the inhabitants in this neighbourhood. I am sorry to say that, even so recently as Friday last, there was an advertisement in the *Times*, with “Surgeon to the Infirmary for Diseases of the Rectum, 38, Charter-house Square,” appended to the name.

Trusting that the observations which you have made will abate the particular nuisance referred to by you, and above all prevent the repetition of such advertisements from regular professional men, I am, sir,

Your obedient servant,

A SURGEON.

Charter-house Square,
Nov. 13, 1837.

PORTRAIT OF MÜLLER.

MR. SCHLOSS, of Great Russell Street, has just published a capital likeness of Professor Müller. It is in the same style, and is as well executed, as any of those to which we have lately had occasion to direct the attention of our readers.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED
CERTIFICATES.

Thursday, Nov. 30, 1837.

Edward Briant, Kennington.—Thos. Wilmott.—Charles Richard Nicoll, Cherington, Warwickshire.—John Yorke Wood, Gloucester.—John Breach, Aston, Berks.—Thomas Jones Saunders, London.—George Willis, Cambridge.—Roger Gilbert Cooper Gardiner, Chelsea.—Jas. Fortune, Edinburgh.

WEEKLY ACCOUNT OF BURIALS.

From BILLS OF MORTALITY, Nov. 28, 1837.

Abcess 3	Inflammation 23
Age and Debility 43	Bowels & Stomach 5
Apoplexy 12	Brain 3
Asthma 16	Lungs and Pleura 9
Cancer 3	Influenza 5
Childbirth 6	Insanity 2
Consumption 64	Liver, diseased 2
Convulsions 30	Measles 9
Croup 1	Miscarriage 1
Denition or Teething 3	Mortification 2
Diarrhoea 1	Paralysis 1
Dropsy 12	Rheumatism 1
Dropsy in the Brain 6	Scrofula 1
Dropsy in the Chest 6	Small-pox 11
Erysipelas 1	Sore Throat and
Fever 22	Quinsey 2
Fever, Scarlet 4	Stone & Gravel 2
Fever, Typhus 7	Stricture 1
Hæmorrhage 3	Unknown Causes 26
Heart, diseased 6	
Hoopng Cough 10	Casualties 9

Increase of Burials, as compared with }
the preceding week } 60

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.

Nor.	Thermometer.	Barometer.
Thursday . 23	from 50 to 55	29.74 to 29.67
Friday . . 24	39 49	29.83 29.88
Saturday . 25	29 42	29.97 30.12
Sunday . . 26	22 48	30.01 29.43
Monday . . 27	30 43	29.43 29.40
Tuesday . . 28	36 45	29.39 29.17
Wednesday 29	26 39	29.37 29.65

Winds, S.W. and W.

Except the 25th, 28th, and 29th, generally cloudy; rain on the afternoons of the 23d and 26th. Rain fallen, .3125 of an inch.

CHARLES HENRY ADAMS.

WILSON & SON, Printers, 57, Skinner-st., London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, DECEMBER 9, 1837.

LECTURES

ON

FORENSIC MEDICINE;

Delivered at the Aldersgate School of Medicine.

By SOUTHWOOD SMITH, M.D.

Physician to the London Fever Hospital.

LECTURE III.

Evidence the Source of all Human Knowledge.—

A Means to an End.—Legal Evidence.—

Medical Evidence.—Legal Decision founded

on Evidence.—Consequence of fallacious

Evidence.—Probative Force of Evidence.—

How Incorrectness or Incompleteness produces

false Judgment.—False Judgment, what.—

Degrees of Probative Force.—Standard De-

gree.—Qualities by which the Probative Force

of Evidence may be raised above or below the

Standard.—Perjury.—Bias.—Expression of

Strength of Persuasion.—Bentham's Scale.—

Direct and Circumstantial Evidence.—Facts

and Inferences.—A single Fact proved by

direct Evidence sufficient to warrant Con-

viction.—A single Fact proved by Circum-

stantial Evidence not sufficient.—Chain of

Circumstantial Evidence.—Cumulative Na-

ture of Circumstantial Evidence illustrated

by the Case of Capt. Donnellan.

Evidence the source of all human knowledge.

—Evidence is the source of all human knowledge, and the guide of all human action. All that we know of natural philosophy, of natural history, of moral and political science, of medicine, we derive from evidence—that is, from the collection, verification, and comparison of facts: classes of facts observed by ourselves or others, either as they arise spontaneously, or after the bodies in question have been put for the purpose into a certain situation.

Evidence a means to an end.—Evidence

being thus merely a means to an end, and being conducive to a great variety of ends, we are now to consider it with a special relation to legal objects: and more especially we are to consider medical evidence as it is applicable to a judicial purpose.

Legal evidence.—Legal evidence is a general name given to any fact which is submitted to the cognizance of the judges of the law. In the English courts, the tribunal of justice consists of the jury and the judge. Legal evidence is the name given to any fact which is submitted to the cognizance of the jury and the judge—submitted with the view that it should produce in their minds a conviction of the existence of some other fact; a conviction to be followed, on their parts, by a legal decision.

Medical evidence.—Medical evidence is a general name given to a class of facts the knowledge of which is derived from the science of medicine and its collateral branches; and which, in like manner, is presented to the jury and the judge, in order that it may form the basis of a legal decision.

Legal decision founded on evidence.—The legal decision relative to a question of fact, in as far as it is just, entirely depends upon, and is governed by, the evidence.

If evidence of a sufficient degree of probative force to satisfy the jury and the judge, of a matter of fact which is necessary to be known, be wanting, the law, in that instance, fails of receiving its due execution and effect.

Consequence of fallacious evidence.—If the effect of the evidence actually presented to the jury and the judge be to produce in their minds a material and decisive deception—that is, the persuasion of the existence of some matter of fact which was not in existence—the consequence of such persuasion must necessarily be an unjust decision; and the ultimate effect of such fallacious evidence is precisely the same as the result which would be produced by the

failure, the want, or the deficiency of evidence.

Hence the extreme importance of guarding against the deception liable to be produced by fallacious evidence; and the necessity that you, who, as skilled witnesses, are to collect and state evidence in relation to numerous questions, some of them of the greatest delicacy, difficulty, and importance, which are ever presented to the judges of the law for their decision, should be able to form a correct judgment of the probative force of evidence, and should take the necessary securities that the evidence you deliver possess the highest attainable degree of probative force; that is, be in the highest attainable degree trust-worthy.

Probative force of evidence.—The properties which give to a mass of evidence its highest degree of probative force, or which constitute its perfect trust-worthiness, are two—correctness and completeness.

Correctness consists in the perfect conformity of the statement given in relation to a fact to the fact itself. In proportion as a statement fails of possessing this perfect conformity to the fact in relation to which it is delivered, in the same proportion will the mass of evidence fail of attaining the maximum of trustworthiness; and precisely in the same proportion will be the danger of deception, and of consequent misdecision on the part of the judges.

Completeness consists in the perfection with which the statement includes all the circumstances that are essential to the justice of the decision.

How incorrectness or incompleteness produces false judgment.—If the aggregate mass of evidence be deficient in completeness, its correctness, instead of being a cause of trustworthiness, may be the cause of the most entire delusion; instead of being a security against deception and consequent misdecision, deception and misdecision may be the inevitable results of it.

Evidence may be incomplete as respects a single article of it—such as the testimony of a single individual; or as respects the whole body of it, considered in the aggregate. In the latter case, the body may be rendered incomplete either by incompleteness on the part of any one or more of the articles of which it is composed, or by the entire absence of any one or more of the articles which ought to have entered into the composition of it.

Incompleteness and incorrectness tend to produce deception, by producing partiality. Evidence has for its object, rectitude of decision; but wrong decision necessarily results either from incorrectness or incompleteness. One object, then,

essential to the attainment of the ends of justice, is the prevention of deception, and consequent false judgment.

False judgment, what.—Judgment regarded as false, is so regarded with relation to some other judgment taken as a standard. This standard can never be any other than the judgment of the individual by whom the term deception is employed.

A mass of evidence is produced, affirmative of a fact, tending to satisfy the tribunal of the reality of the existence of this fact. This evidence produces the wished-for persuasion in the minds of the judges. In my opinion, the evidence in this instance is deceptive, and consequently misdecision has taken place. In other words, of the body of evidence in this instance presented to the judges, the probative force is not, in my judgment, great enough to warrant the conclusion which they have drawn from it—namely, a conclusion expressive of their belief in the existence of the fact in question.

Degrees of probative force.—The very terms in which this my opinion is expressed, assume that evidence delivered in relation to a matter of fact is susceptible of degrees in point of quantity. In my own opinion, in the present case, the quantity of this quality was not sufficient to produce the effect which it did produce on the minds of the judges. This being the case, it is impossible for me not to regard the judges as having, in this instance, been deceived.

Standard degree.—The quantity of probative force incident to a body of evidence, is, then, susceptible of degrees. Of these degrees let us, for the purpose of discourse, form a nominal standard of comparison. Suppose a witness an individual taken by lot, and unknown to the tribunal, deposes that a certain matter of fact, under the circumstances stated by him, presented itself to the cognizance of his senses: Suppose that there be no counter-evidence: the probative force possessed by an article of evidence of this description may be called the ordinary degree of probative force.

It is precisely by evidence of this description that belief is commonly produced. In the great majority of cases, the belief thus produced is right judgment, not deception.

It is, however, notorious, that of the belief thus produced, deception is occasionally the consequence.

But in another case in which the quantity of probative force is, to a certain degree, greater than in the one first mentioned, deception is not so frequently the consequence.

Here, then, we have an assumed no-

minimal standard of comparison for the probative force of evidence. A mass that comes up to this standard, but does not rise above it, is what is meant by an ordinary body of evidence; a mass that rises above it may be termed a body of super-ordinary or superior evidence; a mass that falls short of it may be termed a body of infra-ordinary or inferior evidence.

The greater the quantity of probative force in the mass of evidence produced on one side, deduction made of that produced on the other, the more certain in the eyes of a by-stander will be its effects on the minds of the judges, and the greater in the minds of the judges will be the ease with which their judgment of belief, pronounced on the strength of it, will be accomplished.

Qualities by which the probative force of evidence may be raised above or below the standard.—A quantity of probative force being thus marked out for a standard, what are the qualities by which that quantity is capable of receiving increase and decrease?

1. One source of increase is derived from the quality of the supposed percipient witness. In the case here supposed, the deponent was taken from the middle rank, in respect of qualities intellectual and moral. But, suppose that the station of the witness, whether constituted by education, opulence, rank, power, profession, official function, or by any combination of these circumstances, is, by general experience, found to render a man less apt, on the kind of occasion in question, to deliver a statement in any respect incorrect or incomplete than a man of a different condition; here, then, in the condition of life of the witness, we see one source from which the probative force of an article of evidence may receive increase.

On this ground stands whatever superior degree of credence is commonly given to official evidence in general, or to the testimony of persons invested with judicial offices in particular.

2. Another source of increase is that derived from the number of witnesses. Here the mode of increase being of the utmost simplicity, the degree of it is susceptible of measurement with mathematical exactness.

3. To the number of witnesses, and a more than ordinary degree of presumable trustworthiness on their part, may be added a third source of increase, capable of augmentation to an indefinite amount,—the sort of evidence termed real; evidence derived from some object or objects belonging to the class of things, in contradistinction to the class of persons.

On the other hand, the circumstances which tend to diminish the probative

force of evidence are any manifest imperfection in the intellectual faculties of the deposing witness; any imperfection in his moral qualities, more especially any circumstances which indicate that he is under the influence of sinister interest; the word interest being understood here in its largest and most comprehensive sense, as including not only self-regarding interest, but the interest caused by sympathy or antipathy towards other persons, whether taken as individuals or as classes. Such motives as tend to dispose a witness to yield to the force of interest acting in a sinister direction, are referable to the head of improbity.

Perjury and bias.—When the deposition of a witness is incorrect or incomplete, he is either conscious of such incorrectness and incompleteness, or he is unconscious of it. If he be conscious of it, his testimony is the result of mendacity; which mendacity, if the ceremony of an oath have been applied to it, constitutes perjury; if he be unconscious of it, the imperfection is said to result from *bias*.

Persuasion, then, exists in different degrees of strength. The strength of persuasion on the part of the judges, will, if the testimony of the witness be believed, be as the strength of persuasion expressed by the witness.

The strength of persuasion expressed by the witness will, if clear of wilful falsehood, be exactly the same in degree with the strength of persuasion actually felt by him at the time.

Expression of strength of persuasion.—Suppose, on comparing together the testimony delivered by three witnesses, it appears to the judges that these witnesses all joined in regarding the existence of a certain fact as more probable than the non-existence of it, whereas, in truth, the force of the persuasion of the witnesses lay on the opposite side, here an instance of misdecision will have taken place on the part of the judges, and no worse could have happened had the testimony of none of these witnesses been forthcoming; or had all the witnesses, after joining in a tale of wilful falsehood, obtained credence for it, as if it had been true. Hence the paramount importance of a correct mode of expressing degrees of persuasion.

The language of common parlance is, in this respect, deplorably defective:—I think; I know; I believe. The event happened so and so. I think it happened so and so. I believe it happened so and so. Here the gradation ends.

In the language of the law little better is found. The terms by which the degrees of probative force are expressed in English law are:—1. Positive proof. 2. Violent presumption. 3. Probable pre-

snupition. 4. Light or rash presumption. By positive proof is designated direct evidence; below it stands circumstantial evidence. Violent presumption, it is said, is many times equal to full proof; probable presumption hath also its due weight; while light or rash presumption has no weight or validity at all.

Bentham's scale.—In order to afford the means of expressing degrees of persuasion, or of probative force, with greater exactness, Mr. Bentham proposes the formation of a scale consisting of two parts; the degrees of positive persuasion, or the persuasion affirming the existence of a fact, to constitute one part of the scale, the positive part.

The degrees of negative proof or persuasion, denying the existence of the same fact, to constitute the other part of the scale, the negative part.

Each part is divided into the same number of degrees, suppose ten. At the bottom of each part of the scale stands 0, by which is denoted the non-existence of any degree of persuasion on either side, the state the mind is in when the affirmative and the negative of the fact in question appear as probable the one as the other.

Three persons appear as witnesses. Each expresses his degree of persuasion by this scale. Suppose that persuasion to be on the affirmative side; each indicates the same number, the number one.

In these three instances the force of persuasion is at the least amount at which it can stand on either side.

Suppose, in relation to this same fact, two other witnesses appear, who state their force of persuasion to be at its maximum, represented by the number 10.

Of these two witnesses, the persuasion may be on the same side as that of the three witnesses, or it may be on the opposite side.

Suppose it on the opposite side, the negative. Out of 30 degrees of persuasion which the three witnesses might have had, they have but 3; while of the 20, the utmost number which the two were capable of having between them, they have the whole.

According to the ordinary mode of expressing degrees of persuasion, these differences are unascertainable; they cannot be presented to the minds of the judges; the judges can, therefore, do no otherwise than decide according to the number of witnesses; consequently their decision will be, that the fact does exist.

By the scale now described these differences are ascertainable, and are capable of being presented to the minds of the judges. Suppose, accordingly, the judges

to be guided in their decision by the force of persuasion on the part of the witnesses on both sides taken together; the decision will be that the fact does not exist.

Here, then, it is manifest that a directly opposite decision will be formed by the judges, according as the witnesses have or have not at their command the means of expressing clearly and accurately the degrees of persuasion produced upon their minds by the facts of which they have been cognizant. Where men have been in earnest to express degrees of persuasion, they have found no difficulty in inventing a language capable of doing so with the nicest precision, as is manifest in the case of wagers, and is hourly exemplified in the language of betting. Were benevolence as general and powerful a spring of human action as the love of pleasure or of profit—had men taken the same interest in the administration of justice as they have in providing for their amusement or their gain—the courts of law would not, in this stage of civilization, be so utterly destitute of every form of expression by which the witness can communicate to the judge a perception of the degree of force with which the persuasion to which he testifies may exist.

Direct and circumstantial evidence.—Evidence is either direct or circumstantial. It is important that you should have a clear conception of the difference in the nature of these two species of evidence, and of their different degrees of probative force.

Direct evidence is evidence which relates immediately to a principal fact; circumstantial evidence is evidence which does not relate immediately to a principal fact, but to some other fact evidentiary of a principal fact.

Direct evidence is derived immediately from sense, and does not depend on inference; circumstantial evidence is never derived immediately from sense, and is always the result of inference. I see a man with a sharp weapon inflict a wound upon another man, of which I also see that the latter instantly dies. In this case I am what, in the language of jurisprudence, is termed a percipient witness; and in bearing testimony to this fact I give what is called direct evidence. That a mortal wound was inflicted by a particular individual, with a particular weapon, is in this case not a matter of inference, it is a matter of sense; and the fact being attested by a percipient witness, it is said to be proved by direct evidence.

But it often happens that the principal fact, the fact sought, has been perceived by no human eye—has fallen within the observation of no human being. In this case

it is left to be inferred from other facts, of the existence of which there is no question, and which are so connected with the principal fact, as to be evidentiary of its existence—evidentiary in such a degree as to prove its existence.

Evidentiary facts thus connected with the principal fact, constitute what, in the language of jurisprudence, is called *circumstantial evidence*. He, then, who gives direct evidence, avers, that at a specified time and place, the principal fact in question came within the cognizance of his senses; he who gives circumstantial evidence states the existence of some fact which induces the inference, which leads to the conclusion that, at the specified time and place, the principal fact did exist.

In every case of circumstantial evidence, then, there are always at least two facts to be considered: 1. The fact to be proved; that is, the principal fact—the fact evidenced to. 2. The fact proving the evidentiary fact; the fact from the existence of which that of the principal fact is inferred.

Facts and inferences.—Direct evidence is a species of evidence hardly ever to be obtained. It is exceedingly rare that a principal fact is established by direct evidence alone, without any admixture of circumstantial evidence. Even in the case of evidence adduced by a percipient witness, by an individual who deposes that he saw the fact, still it is extremely rare that the belief of any matter of fact is produced, even in relation to an object of sight, without the judgment having been more or less at work in the production of it. The operation of sense is restricted to the simple perception; inference is the operation of the judgment.

You see a wound inflicted; you see that death immediately follows. Even in this case, all that is properly an object of sense is the infliction of the wound and the death. That the death is the consequence of the wound is a matter of inference; but in this case the sequent is so closely connected with the antecedent, that the evidence is as direct as can well be conceived. It is rare, indeed, that any evidence so direct as this is obtainable for a judicial purpose.

A man was found in his bed-room, at an inn, weltering in his blood; he was speechless, and died in a few minutes. The innkeeper was discovered in the room with a weapon in his hand, with which it was manifest that the wound had been inflicted. When discovered in this situation the innkeeper appeared confused and terrified, and betrayed in his manner all the signs of conscious guilt.

You must be careful not to mistake a case like this for a case of direct evidence. All the direct evidence here is, that a man is seen to die; that a wound is perceptible on his body; that another man is found in the room with a weapon in his hand; and that the deportment of this second man is of such and such a nature. If you had seen the innkeeper inflict with the weapon in his hand the wound upon his guest, and had actually witnessed the immediate death of the latter, then the murder would have been an object of sense, and you would have been able to give, according to the strict meaning of the term, direct evidence of the fact. But you saw no such thing. That the innkeeper was the murderer is an inference deduced from the evidentiary facts—the only facts of the case, namely, that he was found in the room, that he was armed with a weapon, and that his deportment on being discovered in this situation was such as in your conception is natural to a man detected in the commission of a great crime. This inference may be true, or it may be false. The evidentiary facts may be sufficient to warrant your conclusion, or they may be altogether fallacious. The guest may have inflicted the wound upon himself: the innkeeper having heard an unusual noise in the room, may have entered it to see what had happened; and the shock produced by so horrible a sight may have occasioned that appearance of confusion and alarm in his deportment which you falsely attribute to conscious guilt; or the wound may have been inflicted by some other person, who had made his escape unseen.

I shall have occasion again to advert to this case for another purpose; I mention it now merely to direct your attention to the importance of distinguishing between facts and inferences.

However completely a certain fact or event may be a matter of sense, it is impossible that any fact of a criminal nature can be established by direct evidence alone. In order to constitute a criminal act there must be intention—intention to bring about the obnoxious event; but no human being can judge of the intention of another but by an inference deduced from his action.

While it is thus scarcely possible to establish the existence of a principal fact, and not possible if this fact be of a criminal nature, purely by direct evidence, yet it is often established by circumstantial evidence alone, without the aid of any direct evidence whatever. Were it otherwise, it would be seldom possible to prove the commission of the most atrocious crimes, for the evil-doer takes care to

choose an opportunity for perpetrating his crime when he believes that no human eye is on him; and often no human eye is on him. But for circumstantial evidence, therefore, it would be extremely rare that the commission of the offence could be brought home to him.

Evidence, then, is direct in regard to every fact in which a deponent reports himself as having been a percipient witness—circumstantial in regard to every fact not thus a matter of sense, but the existence of which is matter of inference.

The truth of any legal decision, grounded on direct evidence, depends altogether upon the verity of the testimony. If, consciously or unconsciously, the facts stated to have been matters of sense are misrepresented, the decision will be erroneous.

The truth of any legal decision, grounded upon circumstantial evidence, does not depend upon the verity of the testimony, but upon the justness of the inference deduced from it—on the real connexion between the fact evidenced and the fact inferred. If the inference grounded on the testimony be a just inference, the decision grounded on that inference may be a just decision, though the testimony upon which it is based be false. A man is suspected of murder; he is interrogated on the subject of it by a judge. Suppose him guilty: he confesses the fact. There is here no demand for inference; the testimony amounts to a full confession; it operates purely in the character of direct evidence. He does not confess the fact; question upon question is pressed upon him; his replies are evasive; they contain, as is always the case under such circumstances, a mixture of truth and falsehood. The testimony now, that portion of it which is true, not being full enough to operate of itself with a conclusive force in the character of direct evidence, it is consulted, it is made to operate further in the character of circumstantial evidence. In this character it may be full enough to operate even conclusively; it may afford full satisfaction; it may generate a full persuasion, though in the character of direct evidence it may be altogether deficient.

In this case such parts of the testimony as are false may contribute to the support of the conclusion just as much as the facts that are true. Taking the narrative as a whole, if any falsehood be introduced into it, such falsehood will afford a strong evidence of delinquency. On examination of the details of the transaction, this or that particular falsehood may afford an inference establishing this or that particular truth. Thus an assertion representing this or that fact as existing at a certain time

and place, which did not exist at that time and place, or representing as not existing a fact which did exist, may afford a perfectly satisfactory inference as to the existence, or the non-existence of this or that particular fact.

A single fact proved by direct evidence sufficient to warrant conviction.—When a principal fact is an object of sense, and is proved by the direct testimony of a percipient witness, it is universally admitted that sufficient ground is afforded for the decision of the jury and the judge. Accordingly, under the law of England, whenever a single person, in the character of a deponent, declares that such a fact came under the cognizance of his senses, conviction follows as a matter of course, provided the jury and judge be satisfied that the senses of the witness were not deceived, and that his testimony is not false.

A single fact proved by circumstantial evidence not sufficient.—But this is by no means the case with circumstantial evidence. One fact, evidentiary of a principal fact, is rarely, if ever, sufficient to warrant conviction. One single circumstance may beget a suspicion of the fact; a second may render it probable; a third may render it highly probable; and a fourth may induce such a strong persuasion of it, that the thing may be considered as proved. Of two evidentiary facts, take one by itself, its probative force may be scarcely worth regarding—take both in conjunction, they may now afford a very considerable degree of probative force—add to them a third, the probability may be greatly raised—add still a fourth, a fifth, you will at length come to a point where the evidence can be no longer resisted. Circumstantial evidence is thus cumulative, and the completeness of the proof often depends on the number of independent facts which thus connect themselves with each other, and bear upon the point in question.

Chain of evidence.—And this constitutes what is termed a chain of evidence. A number of facts independent of, but closely connected with, each other—each taken by itself proving next to nothing—all taken together forming perhaps complete proof. Such is what is termed a chain of circumstantial evidence.

Cumulative nature of circumstantial evidence illustrated by the case of Captain Donnellan.—For the purpose of affording an illustration of the cumulative nature of circumstantial evidence, I recal your attention the celebrated case of Captain Donnellan, to which I adverted in the first lecture.

On March 30th, 1781, at the assizes at Warwick, Captain Donnellan was con-

victed of murder, committed by poisoning Sir Theodosius Boughton. This case may be taken as an example of an aggregate body of circumstantial evidence, approaching, perhaps, as nearly to completeness as the nature of circumstantial evidence admits; while the individual facts taken separately can be scarcely said to prove any thing.

In right of his wife, Captain Donnellan had an interest in the estates of Sir Theodosius Boughton. He formed the determination to take away, by some means or other, the life of this young man. To shut the door against suspicion, he began by propagating the notion that the state of the young man's health was desperate—that his death would certainly take place at no distant period—that his imprudence was continually heaping up causes upon causes. All this time he was only labouring under a trifling complaint, and was guilty of no imprudence manifest to any one about him.

The poison fixed upon by the murderer was distilled laurel water. It was in his statement relative to the effects of this poison on the human body, that Mr. John Hunter appeared to such disadvantage as a medical witness. To avoid the suspicious circumstance of the purchase of poison, Donnellan provided himself with a still, for the fabrication of it. He practised distillation frequently. The room in which he operated he kept locked up. The poison was now prepared; how was it to be administered?

The young man was taking medicine for a slight complaint under which he was suffering. The phials, as they came in, were usually placed by the invalid in an inner room, which he had been in the habit of locking up. He happened once to forget to take his medicine. "Why," says Donnellan, "don't you set it in your outer room, you would not then be so apt to forget it?" The fatal advice was followed. Soon afterwards a draught taken from the shelf of the outer room, marked "Purging draught for Sir Theodosius Boughton," was given to him by his mother, Lady Boughton. She observed to her son at the time she gave him the draught, that it smelt very strongly of bitter almonds. In two minutes after taking the contents of this phial, the young man was seized with violent symptoms, and in a few minutes more he expired.

Now in all this there is no direct evidence attaching guilt to Donnellan; and if the evidentiary facts are examined singly, how feeble are they—if viewed in the aggregate, how conclusive!

Donnellan practised distillation. As a proof of poisoning, to what did that

amount? Next to nothing. At that rate all distillers would be poisoners.

Not engaged in distillation—not engaged in any other occupation, with a view to profit—not engaged in chemistry in any other shape, still he practised distillation. To what did that amount? Something more perhaps, but still very little more. At that rate all the Lady Bountifuls in the country would be poisoners.

He distilled what there was reason to think was laurel water, a known poison—a substance not known to be used for any other purpose; the proof strengthens, but still it is very far from conclusive.

Thus much as to preparations, though there were others, which I pass over. Go on next to motives. The relation of the defendant to the deceased was such, that upon the death of the latter a large property was to devolve upon the former. Does it follow that, at the expense of so horrible a crime, he endeavoured to occasion such death? At that rate the most common of all causes of death would be parricide.

Now observe how the addition of another circumstance—how the addition of another link in the chain, affects all the preceding. In less than five minutes after the fatal draught had been swallowed by the young baronet, Donnellan came into the bed-chamber where the dying youth lay. On the trial, Lady Boughton deposed as follows:—

"On coming into the room, Capt. Donnellan asked me in what manner Sir Theodosius was taken. I told him. Then he asked me where the physic-bottle was? I shewed him the two draughts. He took up one of the bottles, and said, 'Is this it?' 'Yes,' said I. Then he took it up, poured some water out of the water-bottle, which was just by, into the phial, shook it, and then emptied it out into some dirty water, which was in a wash-hand basin. Observing this, I said, 'What are you at? You should not meddle with that bottle.' Upon that, he snatched up the other bottle, poured water into it, and shook it; then he put his finger to it, and tasted it. I said, 'What are you about? You ought not to meddle with those bottles.' Upon which he said, 'Oh! I did it to taste it.' The first bottle, however, he did not taste."

Now the circumstance that laurel-water was contained in either of these phials was a most material fact; it was a piece of real evidence of the last importance. Even the smell of laurel-water in either of the phials was an evidentiary fact of the greatest moment. Donnellan knew that the smell of laurel-water had been recognized in the draught taken by the deceased; he

saw that observation was directed to the remaining phials; conscious, apparently, of the importance of destroying this object of real evidence, he emptied the bottles of their contents, and rinsed them out.

I add two other links to the chain, of scarcely any strength when considered by themselves, but of considerable strength when connected with the preceding.

In about a quarter of an hour after leaving the bed-chamber of the deceased, Capt. Donnellan met the maid-servant in the passage. He said to her, "Sir Theodosius was out very late over nights a fishing; it was very silly of him since he had been taking such physic." And some days afterwards, he brought to this maid-servant a still which she deposed had been well washed, and which he desired her to put into the oven to dry it, that it might not rust; "but I said, if I put it in, then it would unsolder it, as it was made of tin."

Observe, now, how the probative force strengthens with the addition of every link to this instructive chain of evidence! How the subsequent facts shed light upon, and add importance to, the anterior events! The forewarning that the young man was to die; the distillation of one of the most potent poisons then known; the opportunity sought and procured of administering this poison; the recognition of this identical poison by its smell, when the draught was given by the mother to her son as a purging draught; the death of the young man in a few minutes after taking this draught; death attended with all the symptoms which this very poison is known to produce; the conduct of the suspected murderer in the bed-chamber; his anxiety to destroy the evidence of the existence of the poison in question, by throwing away the remaining portion of it, and destroying even its smell; his attempt to assign a false cause for the death of the deceased; his subsequent *cleaning* of the still; his wish to get it placed in a heated oven, that no suspicious smell might by possibility remain attached to it; and lastly the large property that would come into his possession on the death of the deceased. Take any of these circumstances by itself, how inconclusive! take each in connexion with the rest, and view the whole combined, forming together an aggregate body of evidence, how conclusive!

Such, then, is a chain of circumstantial evidence, composed of a number of independent but closely connected facts, all concurring to produce in the mind a persuasion of the existence of some other fact—a fact sought—a persuasion which may be said to be irresistible.

ON THE COMPOSITION AND ABSORPTION OF PUS.

BY M. BONNET,

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Examination of pus by re-agents.—Parts soluble in water of the ordinary temperature.—Parts soluble in boiling water.—Parts soluble in boiling alcohol.—Parts insoluble in cold water, boiling water, and alcohol.—Pus examined by the microscope.—Examination of pus by exposing test-papers to its vapour.—On the immediate principles always found in pus.

Pus is one of the products most commonly secreted in disease; and is one of those which, by the compression which they cause, the ulcerations which they produce, and the injurious principles which they supply when absorbed, give rise to the most numerous and serious symptoms. Nevertheless, in spite of the importance of its effects, it has been scarcely studied. We have but imperfect notions—

1st, As to the methods of ascertaining its composition;

2dly, As to the immediate principles always found in it;

3dly, The differences in the composition of its several varieties;

4thly, The changes which it undergoes when it is in contact with the air and putrefies;

5thly, The nature of the principles which it supplies when absorbed;

6thly, and lastly, We have no certain method of preventing its contact with the atmosphere, and its consequent putrefaction, after the opening of great abscesses of the joints and pleura.

I have endeavoured to supply these deficiencies, and shall set forth in this memoir the results at which I have arrived.

SECT. I.—On the Methods to be adopted to ascertain the Composition of Pus.

I have studied the composition of pus—1st, by re-agents; 2dly, by chemical analysis; 3dly, by microscopic analysis; 4thly, by exposing test papers to its vapour.

(a.) *Examination of pus by re-agents.*—The only immediate principle which can be recognised in pus by means of re-agents, is soluble albumen; for the precipitates caused by this substance mask every other one, or even hinder them from being produced, as one may convince oneself by the following experiments:—

1. Prepare a soluble albuminate of deutoxide of mercury according to Berze-

lius's method*; mix the serum of the blood with small quantities of the deutochloride of mercury, and then add rather more caustic potash than is required for the decomposition of the salt; you will obtain a transparent solution, and the deutoxide of mercury will form a soluble combination with the albumen. In this state the metallic salt can no longer be detected by the most delicate tests; for alkalies, hydriodates, and soluble hydrosulphates, which detect it so readily in aqueous solutions, produce neither precipitate nor change of colour when it is dissolved in combination with albumen. Reagents are equally powerless in detecting albuminates of lead, copper, and deutoxide of tin: it is incineration alone which enables us to recognise metallic oxides combined with albumen, whether the latter be derived from the white of egg, from blood, from pus, or from dropsical fluids.

2. I mixed soluble albumen with a few drops of hydrosulphate of ammonia. This salt, though I could recognise it with the greatest facility in watery solutions that were five times weaker, could no longer be detected by re-agents, when once mixed with albumen. The salts of lead, mercury, antimony, and deutoxide of tin, afforded only white precipitates, instead of the black, reddish, and yellow ones, which they so easily produce in the aqueous solution.

These experiments are conclusive; they make one readily believe, that if mineral substances, so easily recognised as the oxides of mercury, lead, or copper, and the hydrosulphate of ammonia, can no longer be discovered by tests when dissolved in albumen, or mixed with it, animal substances which, in general, have so few characteristic marks, will be much more difficult to detect, if they are masked by the same principle. Hence, we must not be surprised at the powerlessness of re-agents in leading us to the composition of pus and its varieties. Here, as well as in the study of all animal substances, and particularly of those containing albumen, re-agents are useless until they are brought to act upon immediate principles separated by solvents; in short, they cannot be used before we have had recourse to analysis.

Nevertheless, most investigators of animal chemistry multiply the use of re-agents, and fancy that they find the tints and the precipitates which are characteristic of certain morbid products, such as pus, tubercles, &c.; and even think they can distinguish the varieties of these products. They should abstain from making these attempts by a method whose barren-

ness I wish that I had been earlier acquainted with. If my reflections and experiments are unable to persuade them, I refer them to the tables of reactions published by Tiedemann and Gmelin, in their *Researches on Digestion*, as well as to all the examinations of animal substances made by re-agents alone; they will then be convinced how confused and incomplete are the results obtained by this method, and how necessary it is to adopt more precise ones.

(b.) *On the study of pus by chemical analysis.*—After I have examined the physical characters of the pus, and ascertained whether it is neutral or alkaline, I dilute it with water of the ordinary temperature, and place it upon a filter. The part which is soluble in this water passes but slowly, and only a portion of it goes through the filter; it is put aside after waiting eight or twelve hours. All that remains upon the filter is mixed with triple or quadruple its volume of water; and when all the albumen has been coagulated by continued boiling, I again filter, and thus obtain separately the part which is soluble in boiling water. The insoluble part is dried, powdered, and, after being several times exposed to the action of boiling alcohol, is, in its turn, separated by filtering. Thus the pus is divided into four parts; the first being soluble in water at its ordinary temperature; the second in boiling water; the third in boiling alcohol; and the fourth being insoluble in any of these menstua.

1. *Parts soluble in water of the ordinary temperature.*—This portion, which contains the same elements as the serum of the blood, is boiled until the albumen is coagulated. It is then placed on a filter. The coagulated albumen remains above, and the solution which passes through the filter is now mixed with the one obtained by boiling the pus in water.

2. *Parts soluble in boiling water.*—This portion is evaporated, and its extract is exposed to the action of boiling alcohol, which dissolves the alcoholic extract of meat, as well as the hydrochlorates of soda, potash, and ammonia. After employing the re-agents fitted to detect these salts, I crystallize them on a slip of glass, and examine them in the microscope. The part insoluble in alcohol is again submitted to the action of water, which takes up the aqueous extract, and leaves a little albumen and some earthy salts. These are recognised after incineration. The watery extract is recognised after the evaporation of the water in which it was dissolved.

3. *Parts soluble in boiling alcohol.*—The substances dissolved by boiling alcohol separate as it cools; one is precipitated in

* Vol. vii. p. 71.

the form of a flaky cloud, the other remains in solution; the first is separated by filtration, the second by the evaporation of the alcohol. I shall speak presently of the properties of these substances.

4. *Parts insoluble in cold water, boiling water, and alcohol.*—This portion, which may be composed of fibrin, coagulated albumen, or mucus, is the most difficult to analyse; indeed there is no chemical method of recognizing the substances of which it consists, when they have been once altered by the successive boilings in alcohol and in water.

The analytical method which I have just set forth is copied from the one adopted by Berzelius, in examining the blood; it allows us to ascertain the immediate principles which compose pus, and to obtain an approximative notion of their proportions; but, like the method of re-agents, it fails in characterizing putrefied pus, and in showing the differences of various, syphilitic, tuberculous pus, &c. Hence, after having long adopted it, I was obliged to have recourse to more delicate methods, to resolve the problems that I had proposed for myself.

(c.) *Pus examined by the microscope.*—In using the microscope, I did not examine pus in its natural state; the results obtained by observers who had preceded me were not encouraging, and I thought that this method of experimenting, like the one by re-agents, would neither teach me the composition of pus nor the reason of its varieties. I conducted my investigations as M. Raspail advises—that is to say, by crystallizing the watery and alcoholic decoctions of pus on slips of glass, then studying the forms of their crystals in the microscope, and learning their composition from their form. To judge of the nature of the salts, I followed the delineations given by M. Raspail; but I must say, that several times during the examination of pus, as well as of other morbid products, I have met with crystalline forms which I could not refer to any known delineation, and consequently could not interpret, for want of a standard of comparison. To remove this difficulty it would be necessary to crystallize all the salts which one might imagine could be found in animal substances, to draw them, and then publish the delineations. I began this work, but more urgent occupations prevented my completing it. Until this shall have been accomplished by some chemist, the study of salts by the microscope will often give results incapable of being interpreted, and pathologists who examine morbid products by means of their salts, will see crystalline forms (as I have often done) without being able to determine what these forms represent.

In the actual state of our knowledge, microscopic, like chemical analysis, and like re-agents, can give but an imperfect notion of the composition of pus.

(d.) *Examination of pus by exposing test-papers to its vapour.*—I was led to the employment of this method by finding that it was impossible to produce a black tint by mixing together a solution of lead and fetid pus, while the same pus gave a very decided black tint to compresses dipped in this solution and applied to the abscess. It struck me that the precipitation of the albumen by the salt of lead masked the sulphureous tint, or rather prevented its production; and that if this salt of lead was exposed to the vapour of the pus, the sulphuretted hydrogen, by its volatilization, would produce a very sensible reaction. My supposition was confirmed by experience. To examine this vapour, the simplest and surest method consists in putting the pus into a phial, and applying successively to its orifice papers dipped in a solution of the salts of lead, mercury, antimony, deutoxide of tin, arsenic, &c. In this way all the re-actions produced by sulphuretted hydrogen may be recognized, without heating the pus, if it is very fetid, or by heating it in a water-bath if it is less so.

The examination of the vapour of pus will also enable us to detect ammonia, when this alkali is formed by putrefaction; for this purpose, turmeric paper, or litmus paper, reddened by an acid, must be exposed to the vapour; they should then be kept, to see if the change of colour which they have undergone diminishes by exposure to the atmosphere. Lastly, a rod dipped in muriatic acid is to be brought near the pus, or the sore which secretes it. If ammonia is present, white vapours rise, or show themselves more thickly on the surface of the rod dipped in muriatic acid.

Such are the methods of investigation which I have employed, with the assistance of M. Roux and other house-pupils of the Hôtel-Dieu, at Lyons; and I have applied them, for the last three years, to the study of every variety of pus that I could find. I might have given every experiment, with all its details, had it not seemed to me that this would have added to the dryness of my essay, without giving additional clearness to the facts which I am about to state. My experiments can be easily repeated; I wish this to be done, for by this alone can all doubts be removed as to their correctness.

SECT. II.—On the immediate Principles always found in Pus.

I intend here to speak only of pus which has not been in contact with the atmosphere, before it issues from the abscess.

From the details which I gave when treating of the method to be followed in chemical analysis, it appears that pus contains water, soluble albumen, the aqueous and the alcoholic extracts of meat, earthy salts, soluble salts (such as the hydrochlorates of soda and of ammonia), fatty matter, and fibrin; in short, all the elements of the blood, except the colouring matter.

I tried, with the assistance of all the re-agents which act upon soluble albumen, if that which is contained in pus had any characters differing from those of the albumen of blood or of white of egg; but I could find none.

The alcoholic extract of meat (the *osmazome* of Thénard) also seemed to me to have the same characters in pus that it has in blood and in muscle: I recognized it by its reddish colour, its taste and smell of meat, and its solubility in cold water, hot water, and alcohol. The aqueous extract of meat, likewise, appeared not to vary, whether obtained from pus or blood. In both cases I remarked its solubility in cold and hot water, its insolubility in alcohol, its brittle and semi-transparent appearance, and its insipidity when it was obtained dry, by the evaporation of its aqueous solution. These two extracts have been pointed out by Berzelius, as existing in pus as well as in the majority of animal substances.

As to the animal substances which can be discovered by incineration, I have recognised, as Schwelge did, phosphate of lime and oxide of iron. I have even been able to separate them without incineration, by drying that part of the pus which is insoluble both in water and alcohol, reducing it to powder, and macerating it in dilute nitric acid. This acid dissolves a little oxide of iron and phosphate of lime, substances which also exist in the blood.

The soluble salts have occupied more of my attention. M. Raspail has justly insisted upon the importance of studying them; and it is easy to see how much their proportions and their nature must modify the properties of pus. They are obtained, as separately as possible, by dissolving in alcohol the extract of the aqueous decoction of pus; but as alcohol would precipitate the re-agents employed to test the salts, it must be evaporated, and distilled water used in its stead. The aqueous solution thus obtained, when tested with the nitrate of silver, affords a copious, white, clotty precipitate, insoluble in nitric acid, but soluble in ammonia; it also throws down a precipitate when tested with the nitrate of protoxide of mercury, and with sulphuric acid affords white and acid vapours, which are condensed by ammonia. These re-actions show the pre-

sence of hydrochloric acid; but as the solution is hardly acid, it is evident that this hydrochloric acid is combined with bases. What are they? Potash disengages alkaline vapours which are condensed by hydrochloric acid, and which are, therefore, ammoniacal; the hydrochlorate of platinum gives no clear result, on account of the reddish yellow tint which the extract of meat communicates to the solution of salts, from which it is inseparable. The other re-agents teach us nothing. We must, therefore, have recourse to other methods of investigation, and, placing a few drops on a slip of glass, wait till they crystallize, and then examine them with the microscope.

I must here warn the student that M. Raspail's microscope is insufficient to distinguish clearly the salts which are then formed. From confining myself to its use, I often obtained no precise notion as to the salts of pus, or of other animal fluids. A compound microscope is absolutely necessary. When we attentively examine the soluble salts of pus with this microscope, we find, in the first place, cubic crystals, in which two of the opposite sides are uneven, sinking by a kind of steps formed by the impression of a pyramid with a square base; secondly, squares and parallelograms; thirdly, fern-like arborescence. A very clear idea of all these crystals may be formed by consulting plate 6 of M. Raspail's Organic Chemistry, fig. 12, *a, b, d*; and the description given by the author, at p. 536. With the help of these data I recognised the hydrochlorates of soda, of potash, and of ammonia, in the crystals of the aqueous solution of pus. These salts are always present, which will not surprise the reader, when he recollects that in every analysis the hydrochlorates of soda and of potash have been found in the blood, and that M. Raspail has shown by experiments, of which I have several times verified the accuracy, that the blood contains the hydrochlorate of ammonia. As it is impossible to separate by solvents the hydrochlorates of soda, potash, and ammonia, and as the fear of altering the salts hardly allows us to employ incineration, which would volatilize the hydrochlorate of ammonia, it is obvious that the proportion of the different salts discovered by the microscope can be judged of only by an approximative estimate. The proportions estimated in this manner have seemed to me to differ in the several varieties of pus, as will be seen further on, when I shall treat of serous, mucous, and other kinds of pus. I ought to mention, that the salts of which I have pointed out the crystals, have already been recognised in pus; the hydrochlorate of soda, by Jordan, Pearson, and Gendrin; and the

hydrochlorate of ammonia by M. Raspail, who has pointed out its importance, and shown that it exists in a great number of animal fluids. The examination of the fatty matter of pus, which is soluble in alcohol alone, led me to some experiments showing the relation which exists between the composition of blood and of pus. It is well known that Vauquelin, in his analysis of the brain, and of marrow, discovered two kinds of fatty matter containing phosphorus, to which he gave the names of white cerebral matter, and red cerebral matter. M. Chevreul, when analyzing the blood, ascertained that the fatty matter which can be extracted from it by means of alcohol and ether is identical with the fatty matter of the brain, and offers the same two varieties which Vauquelin had observed in the cerebral pulp. Denys, in his experimental researches into the nature of human blood (p. 109) confirmed the discovery of M. Chevreul, and found these same two substances in the muscles; and as Vauquelin had observed them in the chyle, and Braconnot in the liver, and they contained phosphorus, he gave them the name of phosphorated fatty matter, instead of cerebral fatty matter, dividing them, after the example of Vauquelin, into red phosphorated fatty matter, and white phosphorated fatty matter.

The properties of these fatty matters are easily recognized; if we take one of the substances containing them, for example the fibrin of the brain, it is sufficient, after drying and powdering it, to boil it in alcohol. The liquor, when filtered, remains clear as long as it is hot; but, on cooling, it becomes turbid, and deposits a white substance, soft and greasy to the touch, soluble in ether, and not growing red by heat; this is white phosphorated fatty matter. The alcohol, which has deposited this matter, is now evaporated; and after its evaporation there remains a fatty matter which becomes of an orange-red by heat, softens without melting, and makes paper transparent as oil does: this is phosphorated fatty matter. (For more details, consult Denys' Experimental Researches upon the Blood, p. 105.) These two substances, when suspended in water, make it turbid and milky, and give it the appearance of an emulsion; this emulsion does not coagulate by heat, in which it resembles milk, but differs from it in not coagulating either by pressure or by acids. Now the fatty matter extracted from pus by means of alcohol, have exactly the same appearance, the same re-actions, and are dissolved and precipitated by the same substances, as the fatty substances of the blood, like them, too, they form white emulsions, and when they exist in pus in considerable

quantity, it coagulates but imperfectly by heat, and preserves its milky tint even after boiling. It is this, without doubt, that led M. Dumas to reckon among the constituents of pus*, a substance resembling casein, a fact which cannot be disputed, if the word casein merely means a substance forming a milky emulsion not coagulable by heat.

The property which these fatty matters found in pus possess of forming emulsions, seems to me their most remarkable characteristic, and I shall therefore call them emulsive fatty matters (*graisses émulsives*), not venturing to call them *phosphorated fatty matters*, as Denys did those found in the blood; for when, after incineration, I tried to find the phosphoric acid which would have been produced by the phosphorus had there been any previously, I did not succeed; whether because it was really absent, or from some fault in the experiment.

As I have stated above, the constituents of pus whose existence it is the most difficult to determine, are those which are insoluble in cold water, hot water, and alcohol. When they have been separated by these menstrua from the substances with which they are mixed, the only way to examine them is to dissolve them in acids, or in alkalies, at an elevated temperature. These re-agents, however, alter them, and produce solutions in which the properties of the acid or the alkali mask those of the substance which they dissolve. Thus, whether we examine albumen coagulated by heat, or organizable fibrin, such as is found upon an inflamed peritoneum, or fibrin spontaneously precipitated in blood drawn from a vein, the products obtained from them through the re-action of acids or alkalies are exactly the same. These re-agents, therefore, are unable to distinguish between very different substances, between false membranes capable of organization, and merely dead matter, like coagulated albumen. They are not able to throw any light upon the substances examined with their assistance. Hence we see that those constituents of pus which are insoluble in water, alcohol, and æther, which must be dissolved to be more accurately examined, and which can be dissolved only in acids or alkalies, must in some measure elude chemical examination. These unexamined parts are derived chiefly from the opaque and whitish masses that swim in creamy pus, and from the clots found in cold abscesses, and which are sometimes called coagulated albumen, and sometimes tuberculous matter. These whitish masses, and these clots, coagulate spontaneously during the formation of pus, and are insoluble in cold water, hot water, and al-

* Andral, Anatomie Pathologique.

cohol; and therefore they are formed by fibrin, as Jordan and Gendrin supposed. Nevertheless, they are incapable of organization, while the fibrin of false membranes, and even that of the blood, can be organized. Does this difference prove a difference in chemical composition also? I think not. The fibrin of pus floats in serum; it does not adhere to living tissues, and is therefore unable to receive blood, and become vascular; it is in the condition of a layer of plastic lymph detached from an inflamed serous membrane; like the false membrane detached in this manner, it has lost its capability of becoming organized, but it may have lost it from purely physical causes, and without changing its composition. Nothing proves, therefore, that it is altered, or that it differs chemically from the fibrin of the blood.

A similar conclusion occurred to me from comparing each of the immediate principles of pus with the immediate principles of the blood; and if the authors who have treated of this morbid product have not been able, like me, to point out each article that enters into its composition, and show that its composition is the same as that of the blood, minus the colouring matter, it is because no one of them had completely analyzed it, and more especially because the true nature of its fatty matters was not known; the settling this last question being the really new part of my researches. I was long surprised at the facility with which pus is secreted; for while I considered it as a peculiar substance, without anything resembling it in the animal economy, I could not understand how every tissue and every inflamed organ was capable of producing it, and why every solution of continuity not healed by the first intention necessarily secreted pus. But the moment that its relation to the blood was analytically demonstrated, this facility of production no longer seemed strange.

We learn from physiology that fluids containing immediate principles not found in the blood, such as urea, or the resin of the bile, can be secreted only by organs of a complex nature, such as the kidneys and liver; while the fluids merely formed from the immediate principles of the blood can be separated from it by the simplest tissues. The separation of the serum by the cellular tissue and serous membranes is an example of this. But if it is true that the secretion of every immediate principle which does not exist in the blood supposes a complicated apparatus, how can we imagine that pus can be secreted by cellular and fibrous tissues, or, to speak more correctly, by a false membrane in a vascular state, if it contained any principle peculiar to itself? But as

soon as we know that blood, to be turned into pus, requires merely to be deprived of its colouring matter, we are not surprised that a simple apparatus can produce so slight a change; and if the phenomena is not explained, at least it is shown to be analogous to other ones.

Let us remark in general, that if every tissue, and every inflamed organ, is capable of secreting serum, organizable matter, and pus, it is because every inflamed organ receives blood, and blood contains all the immediate principles which form serum, organizable matter, and pus. Chemical analysis is called upon to establish many relations between morbid products, as well as to specify more clearly the differences between them. But let us continue the examination of facts; the applications to pathological physiology will multiply at every step.

[To be continued.]

A POSTSCRIPT

TO THE

FIRST OF DR. PHILIP'S LATE COMMUNICATIONS.

To the Editor of the Medical Gazette.

SIR,

It is observed in the first of my letters, that the facts which had been ascertained reduce the general laws of our frame to that simplicity which we so constantly find to characterise the works of nature, when we have obtained a correct knowledge of them. It has since occurred to me, that it would have rendered clearer both some parts of that letter, and of those which follow it, had the foregoing observation been illustrated by the following short recapitulation, which I hope you will be so good as insert, as a postscript to my first letter, in the next number of the MEDICAL GAZETTE.—I am, sir,

Your obedient servant,

A. P. W. PHILIP.

Cavendish Square,
Dec. 4, 1837.

It appears from all the facts referred to in the preceding communication, that there are four distinct sources of power in the more perfect living animal, having no direct dependence on each other, namely, those of the sensorial, the nervous and muscular powers, and the powers of the living blood; while the due structure of the organs of each more or less directly depends on the co-

operation of the functions of all those powers; for it appears from what has been said in that communication, and the works referred to in it, that, through the function of respiration, the aid of the sensorial functions themselves is as essentially, though not so directly, necessary to the maintenance of the due structure of all our organs, as those of the muscular and nervous powers, and the functions of the living blood.

From a review of the various facts referred to in my first letter, it is easy to perceive the manner in which these different powers assist each other in the formation and maintenance of our organs.

The living blood supplies the materials from which they are formed, and from which they derive the supply which is rendered necessary in consequence of the materials of which they are composed undergoing a constant state of change, from their being, by the functions of the organs to which they belong, gradually rendered unfit for the purposes of life. This is evident with respect to the living blood, when we compare the arterial and venous portions of it, because there we can better trace the chemical processes on which the changes depend, than in other functions of the same class. In the functions of another class, we find the laws of mechanics in operation.

Observation, without the aid of experiment, at once points out that the mechanical phenomena of our bodies obey the same laws with those of inanimate nature, and that the muscular fibre affords the power on which, in the animal body, these phenomena depend*.

The chemical power, it appears from the experiments referred to in the paper to which the present communication is a postscript, is supplied by the nervous system properly so called; and, as we might from the simplicity of nature in all her works have expected, this power, as appears from the experiments of Dr. Faraday†, compared with those to which I have referred, is here the same which effects the chemical changes of inanimate nature.

The sensorial powers supply sensation and volition, the only sensorial functions employed in the maintenance of the due structure of our organs, and consequently of life‡; for the failure of

any vital function is necessarily followed by that of all the rest.

When we turn to the sensorial functions, we find ourselves in a new world. The striking analogy which necessarily exists between the mechanical and chemical phenomena of the living animal, (which are regulated by the same laws which operate in the external world, essentially modified, however, by the vital principle which pervades all living parts,) and the phenomena of inanimate nature, here wholly disappears; because whereas, while the phenomena of the other functions are the results of the co-operation of their living powers with the powers of the inanimate materials of which they are composed, in consequence of which they are capable of immediate intercourse with the materials, and consequently with the operations of the inanimate world; the functions of the sensorial organs being those of their living powers alone, they admit of no direct intercourse with the materials, and consequently with the operations of that world. And in the paper so often referred to in the Philosophical Transactions for 1836, I have considered at length the beautifully simple means by which they are enabled indirectly to act in concert with those operations, between which and their functions there is nothing in common.

Such are the whole of the functions on which the life of the more perfect animal depends. In the paper just mentioned, their *modus operandi* in their various, and particularly their more complicated results, is detailed, and the observations and experiments on which each position is founded referred to.

REMARKS

ON

THE NATURE AND TREATMENT

OF THE

DIFFERENT FORMS OF PORRIGO.

By WALTER DICK, M.D.

[Continued from p. 332.]

"Dan l'appréciation des phénomènes que présentent les maladies, il faut toujours faire entrer en ligne de compte la structure particulière de l'organe malade."—*Alard, Nat. et Siège des Mal'* tom. ii. p. 257.

THERE is nothing, as a writer justly observes, which affords so fair promise of improvement in the management of

* Philosophical Transactions for 1836.

† Ibid. for 1832, 1833, 1834, and 1835.

‡ Ibid. for 1829 and 1836.

cutaneous diseases, as a due regard to the anatomy and different functions of the skin. Before, therefore, proceeding to the consideration of the different forms of porrigo, we deem it right to premise a short account of the anatomy of the hair and sebaceous follicles, as these, in our opinion, are the parts primarily affected in the disease just alluded to.

Breschet and Vauzeme, who may be said to be the first to have successfully examined the intimate structure of the skin, have not yet bestowed particular attention upon the sebaceous follicles and hair, so that we can derive no assistance from these laborious and precise investigators. Much that is said of the organs in question in books, is neither very satisfactory nor correct we think. In place of making personal observations, the authors of anatomical systems, when treating of the skin, too often blindly borrow from their predecessors, and take for granted, and describe, what they have neither seen, nor taken the trouble to look for. The following short description of the hair and sebaceous follicles is drawn up from notes of some observations, which we have recently made on the human skin and skins of some of the lower animals, chiefly of the cow and sheep. Brief as it is, I trust it will be sufficient for my present purpose. The constituent parts of the skin, we may be allowed to remark, are so minute, so difficult of examination, and therefore so liable to be misrepresented, that much caution and candour are requisite in their examination. With this conviction we have pursued our investigations, and have not advanced any thing, of the accuracy of which we were not perfectly assured. Nevertheless some of our statements may prove to be inaccurate; if so, we shall feel obliged by being corrected, as we write not for novelty, but from a desire of adding our mite to the general stock of knowledge, and of advancing the interesting, but too much neglected, study of dermatology.

Anatomy of the Hair.

That part of the hair enclosed within the skin, is the part whose exact anatomical relations it is most important to know. This part, in the scalp at least, generally presents the shape of the letter J—the dermal extremity of the hair being very slightly bent upon itself. The origin of the hair is in the subcu-

taneous tissue, and generally presents the appearance of a small, blackish body, of a roundish form. This body lies at the bottom of a piliferous cyst, and minute colourless filaments, apparently vessels, may be seen entering it. The dermal extremity of the hair, which is hollow for a little way, and softish, almost pulpy, is implanted on the roundish body just described—the *monticule* of Duvernay, as it has been called—the hollow, corneous part of the hair forming a kind of sheath round the monticule. The connexion between these two parts is not very strong, as the hair may, almost invariably with a little dexterity, be plucked out, without bringing along with it the monticule.

Each hair is enveloped in two sacs. The inner one is very delicate, and closely applied to the hair, especially a little below what is called its neck. The outer sac is thicker and stronger than the former. The two are but slightly connected, and are easily separated from one another. Morbid action in either of them, especially when producing effusion of fluid, would very likely intercept the nutriment of the hair, and cause it to fall out, or separate from its attachments.

In what way hair is produced has not yet been determined. It is not yet ascertained, I believe, whether the inner piliferous cyst is immediately concerned in its production, or whether the monticule secretes both the corneous substance of the hair and its colouring matter. That the monticule is principally concerned in the production of hair, is evident from the fact, that no hair is ever generated after its destruction. But it seems probable to us that two different structures are concerned in the production of an entire hair, as we sometimes see the hairs become blanched, while they remain otherwise natural and continue to grow. This, we think, could scarcely happen if the same organ produced both the corneous part of the hair and its colouring matter.

Some writers have asserted that the hair receives a covering from the cuticle. They suppose that that membrane passes down the piliferous cyst to its bottom, and is there reflected along the hair. Others have fancied that the hair, after rising to the level of the cuticle, pushes it before it, and thus, as it were, mechanically takes a covering from it. The fallacy of these views has been success-

fully exposed by Bichat*. The cuticle does not seem to be reflected at any part along the hair, but is more or less firmly attached to it at its exit from the skin, and passes only a little way down the inner piliferous sac. It is a curious fact that the cuticle, though permeated by myriads of hairs, sebaceous follicles, and sudoriferous canals, does not, when separated from the chorion, present a sieve-like appearance, as might have been expected. Various explanations have been given of this phenomenon: the most satisfactory seems to be Mr. Cruickshank's, who ascribed the disappearance of the pores in the detached cuticle to the elasticity of the membrane†.

Hair, when seen through a powerful microscope, presents the same appearance as the cuticle; and analysis has proved that the chemical components of these two structures are nearly the same. It is highly probable, we think, that hair is formed, like the cuticle, by two different sets of organs; one set secreting the corneous substance; the other, the colouring matter. Some authors, and amongst these Bichat, have supposed that hair possesses a certain degree of vitality; and as confirmatory of this opinion, they adduce the phenomena of *Plica Polonica*. This view, however, seems to be erroneous; and the phenomena of *Plica* can be satisfactorily accounted for, by referring the seat of the disease to the piliferous bulbs and cysts.

Anatomy of the Sebaceous Follicles.

The sebaceous follicles are organs familiarly talked of, but their true structure, I suspect, is but imperfectly known. They are easily seen in the cow's skin; and we may be allowed to state a few particulars respecting their appearance in that animal, before describing those of the human skin. In the cow they bear a striking resemblance to the Meibomian glands, with this difference that they are much smaller, and their central canal or duct is not a shut sac, but is continuous with the piliferous cyst. Each piliferous cyst may be said to be surrounded at its summit by one of these follicles, the follicle being on

the outside of the double pilous sac. Such is the usual appearance of the sebaceous glands in the cow's skin. But there is a peculiarity at the roots of some of the hairs worthy of notice. Interspersed amongst the shorter hairs, there are many longer and stronger ones. The roots of these latter hairs are enveloped by two cysts—an outer one, very thick, and of a fibrous texture; and an inner one, of a delicate diaphanous structure. Between these two cysts an erectile or vascular tissue is situated, which throws out a drop or two of blood when cut into. The sebaceous follicles belonging to these coarser hairs are considerably smaller than those round the finer hairs; and in place of lying outside of the pilous cysts, are situated between the fibrous and inner cysts, being attached principally to the former.

The sebaceous follicles of the human skin are smaller, less complex apparently in their structure, and consequently more difficult of examination than those of the cow; but their relative situation is the same. They are best seen at the roots of the cilia or eyelashes, elsewhere they are smaller, but essentially the same in structure. On the nose, where their orifices are usually pretty perceptible, especially in males, we see delicate or rudimentary hairs passing out from many of the orifices, and frequently by pressure we may force a little sebaceous matter along the hairs, or out by their sides; satisfactorily enough, establishing that the hairs pass through the centres of the follicles, and that the pilous cysts and sebaceous follicles have a common outlet*.

Mr. Chevalier states, in his lectures, delivered some years ago before the College of Surgeons, London, that there are two sets of sebaceous glands in the human skin:—1st, a superficial set, “between the second epidermis and the cuticle, to which they appear very firmly attached;”—2d, “A deeper set embedded in the chorion, and which are commonly known as the sebaceous glands.” This discovery of Mr. Chevalier has not, we believe, been confirmed by any subsequent anatomist.

* Anat. Générale, tom. iv. Système Pileux.

† According to Breschet, the sudoriferous canals, which are of a spiral form, pass through the cuticle obliquely; they are torn, and their fragments attached to the cuticle shrivel up, when that membrane is detached from the corion. In this way, M. B. satisfactorily accounts for the disappearance of the sudoriferous pores.

* It is the sebaceous follicles, we believe, which produce that peculiar, transitory appearance of the skin, denominated *cutis asserina*. Some have thought that it resulted from the bulbs of the hair, but we do not see how these, seated as they are beneath the cutis, could give rise to the appearance in question.

We learn from Alibert*, that Eichhorn, from having observed that "*tannes*," or grubs, escape from the apertures which give exit to the hairs, denies the existence of sebaceous glands, and supposes that the function commonly ascribed to these organs is performed by the piliferous cysts. After what has been said above, we need say nothing in refutation of this opinion.

The sebaceous glands lubricate the hairs, and soften the skin, by their secretion; and they may, perhaps, be subservient to more important purposes in the animal economy than physiologists are at present aware of.

Porrigo Lupinosus†.

This variety of porrigo is well named, from the peculiar form which its characteristic scabs present. Indeed, in the whole catalogue of skin diseases, there is not, perhaps, one more happily named, as its scabs bear a striking resemblance to lupine seeds:—hence the epithet, *lupinosa*. M. Baudeoque believes that these cup-shaped scabs are not the products of pustules, but of a morbid secretion from the piliferous cysts. M. Mahon, whose opportunities of observing the disease have been very numerous, also denies their pustular origin.

* Monographie des Dermatoses, tom. ii. p. 68.

† Achores and Favi are two varieties of pustules, frequently mentioned by writers when describing the eruptions of the scalp. The latter (Favi), as described by Willan, differ in no respect from the psudria: but the achores appear to be a distinct species, and are therefore entitled to special notice.

An English student, who is familiar only with Willan's nomenclature, is apt to be perplexed by finding the terms favi and achores used in a different sense by French writers. The pustules characteristic of *P. lupinosa*, are called by Willan and Bateman, achores; while by the French dermatologists they are styled favi; and hence *P. lupinosa* is called, by the French, *P. favosa*, or *teigne favieuse*—a disease very different from the *P. favosa* of Willan. The French, however, are entitled to the credit of having described the pustules in question more minutely and accurately than either Willan or Bateman, and had they retained Willan's term, they would have prevented the confusion to which they have given rise, by using a different appellation.

The achores of Alibert, Bielt, and others, are nearly synonymous with the favi of Willan.

It is well known that Dr. Willan borrowed the above terms from the ancients, and if ancient usage can go for any thing in such matters, he certainly used them in their proper sense, the achores being anciently reckoned the smaller, the favi the larger, species of pustules.

In the article "Achor," in the Cyclop. of Pract. Med. no mention is made of the discrepancy exhibited in French works in the use of the above terms; and as we ourselves were, for a time, somewhat puzzled by this discrepancy, the above remarks may perhaps prove serviceable to others under similar circumstances.

"Des observations attentives et mille fois répétées ne me permettent pas de regarder la teigne favieuse (*Porrigo lupinosa*) comme une eruption pustuleuse*."

M. Mahon has been led to believe that the disease originates in the sebaceous glands, and that the cup-shaped scabs which it presents result from the indurated secretion thrown out from these organs. We are inclined to take the same view of the disease; and we have a preparation in our possession which we think establishes it. The phenomena of the disease, we will venture to affirm, are more satisfactorily accounted for by this view of its pathology than by any other.

Porrigo lupinosa, when seen at its very commencement, presents small yellowish points, depressed in their centres. These achores, or, as the French call them, *favi*, are almost invariably traversed by hairs. Their contents, moreover, almost from their very commencement, are concrete. They enlarge slowly, seldom exceeding three or four lines in diameter, although they are sometimes seen as large as a sixpence. During their development the cuticle covering them cracks, and partially falls off. They are firmly imbedded in the skin, and when fully formed still retain the circular form, raised at the edges, and cupped. These peculiar scabs are usually of a light yellow colour, and when of old standing a powdery substance is seen lying in their central depressions. They arise usually distinct from one another, but during the progress of the disease they sometimes run together, forming one or more large, elevated, indented scabs. The skin between the scabs seldom appears reddened, but very frequently presents a furfuraceous appearance. The hair very generally becomes lighter coloured, and brittle on the affected parts, and either falls off spontaneously, or may be removed with facility, and without causing pain.

When the scabs are removed by means of poultices, the parts upon which they lay appear raw and excoriated, and depressed; a viscid fluid is speedily thrown out, which concretes into scabs, irregular in their form, and altogether different from the primary ones.

Sometimes, in severe and neglected cases, the scalp becomes deeply ulcerated,

* Recherches sur les Teignes, p. 6.

even so as to expose the cranial bones; and a most abominable smell is exhaled from the scabs in those cases where cleanliness is not observed. Pediculi are likewise generated in great abundance. The cervical glands not unfrequently become enlarged and painful; and in neglected cases the system occasionally suffers so much, that the body is stunted in its growth, the development of the intellectual faculties is retarded, and a state of cachexia induced which sometimes terminates in death.

Porrigo lupinosa is not confined to the scalp, but may appear on any part of the body. Alibert mentions a case where the eruption was diffused over the whole body, the scabs on the trunk and extremities presenting the same cupped appearance as those on the head.

From each of the scabs in *P. lupinosa* being permeated by a hair, Mr. Plumbe and others have been led to believe that the hairs play an important part in the production of the scabs. Duncan, Underwood, and Luxmore, have even asserted that the disease originates in the bulbs of the hairs. This opinion, we think, has been successfully combated by Murray, who found these organs at the commencement, as also sometimes during the progress of the disease, in a healthy state. Murray remarks, "*potiorem sedem mali in folliculis dictis pinguetudinis, vel ipso textu celluloso, quaerendam arbitror.*"

M. Baudelocque, who denies the existence of sebaceous follicles in the scalp, places the origin of the disease in the pilous cysts. "This opinion," says he, "I have deduced from the disease most frequently attacking those parts which are most plentifully supplied with hair; from the non-existence of sebaceous follicles in the scalp, as Bichat, Meckel, and others, affirm; and from one or more hairs almost constantly traversing the *faveous* scabs*."

The opinion respecting the seat of *P. lupinosa* which we have already stated as the one we are inclined to adopt, could not be entertained, were it true that the scalp contains no sebaceous glands. But if we are not much mistaken, we ourselves have seen these organs in the scalp, and are therefore convinced of their existence. They are placed, as we observed above, at the exit

of the hairs from the skin, and surmount as it were the summits of the pilous cysts. Their ducts give exit to the hairs, and communicate inferiorly with the pilous cysts.

The preparation to which we above alluded as confirmatory of the opinion of the disease being seated primarily in the sebaceous follicles, consists of a small portion of the scalp, inclosing an incipient lupine-like scab, taken from a subject who died of fever, and who had, for a considerable period before death, been affected with *P. lupinosa*. The scab is seen to be still covered by the cuticle: inferiorly, it appears to be inclosed in a membrane, and a hair enveloped in its double cyst, apparently in a healthy state, is seen passing through its centre. In short, the scab occupies exactly the situation of a sebaceous gland, and appears to be neither more nor less than a sebaceous gland distended by a concrete secretion. This view of the pathology of the disease seems to be confirmed by the fact that when a lupine-like scab is removed it is not succeeded by a similar one, but by one of an irregular form, without the peculiar cupped appearance. This arises, as we conceive, from the sebaceous follicle having been destroyed during the development of the primary scab.

From the intimate connexion of the pilous cysts with the sebaceous glands, the former very generally become diseased when the latter organs are in a morbid state; and it is from this cause, we think, that the hairs become altered in appearance in *P. lupinosa*. Permanent baldness is not unfrequently occasioned by this disease, and it results, as we remarked before, from the total destruction of sebaceous glands, pilous cysts, and *bulbs*.

There is a variety of *P. lupinosa*, where the *achores* (*favi* of the French) arise closely set together in patches of a circular shape, and ultimately become transformed into large, elevated scabs, irregular on their surface, but without the indentations exhibited by the scabs of the former variety of the disease. It is the pressure which the *achores* mutually exert upon one another, when thus crowded together in clusters, that prevents them from individually presenting the cupped appearance which they present when arising distinct and separate from one another, as in the common *P. lupinosa*. Alibert describes this form

* Recherches sur la Teigne faveuse; Revue Medicale, t. iv. 1831.

of porrigo under the name of *Favus scutiforme*; and correctly observes that it is essentially the same as *P. lupinosa*. It has also been described by some under the term *P. scutulata*, but in my opinion it is quite a distinct disease from the *P. scutulata*, or ringworm of this country. For the sake of distinction, it might be called *P. lupinosa conferta*.

The two forms of *P. lupinosa* above described are those, in our opinion, which are seated primarily in the sebaceous glands. We have next to describe those which primarily affect the pilous cysts, and parts which secrete the hair *

[To be continued.]

SOME ACCOUNT OF MESMERISM.

BY BARON DU POTET DE SENNEVOY.

[Continued from page 379.]

Report of the French Academy, in 1831.

FIVE years had elapsed, and the committee had not yet accomplished its task. Some anxiety was already felt as to the motives of so long a delay, and fears were conceived lest there should be a denial of justice on the part of the commissioners. Several magnetisers had presented facts to their notice, and accounts had been drawn up; and I also had placed myself at the disposal of the members of the commission. They had met at my residence several times to witness experiments, and I had received their promise that the effects produced before them should be acknowledged.

Magnetism continued to spread among those who are usually strangers to scientific researches. Astonishing phenomena were produced, before persons incapable of appreciating the importance of their own works. Somnambulism, a phenomenon of which we shall presently have to speak, was prostituted. The wife of a shoe-maker (*La Fanchouquet*) was spoken of, as having been thrown into somnambulism by her husband, and relating, while in that state, the scenes of a new kind of existence (*les scènes d'une vie nouvelle*.)

Another patient, a blind man (*Lemaire*), having become a somnambulist in consequence of magnetisation, attracted round him all the inhabitants of

his district to witness his ecstasies, and, indeed, this singular phenomenon was being produced in all quarters. Meanwhile, what were scientific men about? They were occupied in disputing the possibility of a phenomenon which was matter of common notoriety; they denied the existence of a force which was essential to life. It was in the very sanctuary of science, and in the deepest meditation, that they ought to have examined into it; but they only laughed at the credulity of those who invited them to do so.

But very soon magnetism emerged from its obscure retreats: for gilded equipages went to seek it there. The princess N—— was seen to reward liberally the services which she believed herself to have received from a somnambulist. The Comte de —— caused the wonders of which he had been witness to be inserted in a journal. No doubt their sole object was to make known these facts, and to recompense some real service. Charlatanism however sprang up. Avarice took possession of somnambulists, and they had recourse to deception. Very soon, serious causes were brought before the tribunals, in which magnetism had been employed as a means of abusing the confidence of respectable persons. Several celebrated magnetisers of that period were called upon by the judges to enlighten them on the subject of their art.

The theatre availed itself of somnambulism; a drama was represented, in which the principal character being magnetised on the stage, sleep and reveals a crime which he had committed, during a species of natural somnambulism. This bold attempt of the author was favourably received by an audience already familiarised with this new art.

The novelists were not behind-hand. Frederick S—— introduced magnetism into his romances; and our celebrated Pigault le Brun warmly undertook the defence of a truth which, on its first appearance, he had attacked; a remarkable circumstance, proving the progress that magnetism had made, French literature, in reality, took a hue of magnetism.

The avowals about to be made by the committee had thus become more easy and less dangerous. The commissioners, therefore, announced to the Academy that they would read their report on the 21st of June, 1831.

* Errata.—In p. 331, col. 2, l. 1, omit "P. scutulata;" and in p. 332, l. 6, omit "profession."

The hall in which the Academy assembled (so empty on ordinary occasions,) was crowded that day. Even the passages were obstructed by the curious. One would have said, that one of those decrees on which the weal or woe of a nation depends, was in agitation. All the members of the Academy, even those enfeebled by age, were at their posts.

At length the meeting was opened, and M. Husson, the reporter of the committee, appeared at the bar with a voluminous roll of paper. This was a good sign. It implied that they had investigated the subject.

He spoke in a grave and measured tone, and began by reviewing the events which had preceded the nomination of the committee; then he invoked the memory of the ancient friendship borne by the elder practitioners present, to the members of the committee. At length he came to the point: he related first all that had appeared doubtful to the commissioners; he then cited the facts which could be explained by causes foreign to magnetism. At length came the turn of the more positive facts. But as the forms of oratory, and the cases already cited, had absorbed much time, the conclusion of the report was deferred till the next meeting.

That was the day of the grand battle;—I say battle, for on that occasion there was a general affray amongst the members of the Academy.

I am now, Mr. Editor, going to quote some facts mentioned in the report. Those who attested them are yet living; they still sit in the Academy. If these were false, the physicians who gave them as being true are unworthy of their seats in that scientific body: if they suffered themselves to be deceived, if they were the dupes of a piece of jugglery, what value is to be set on human penetration?

On the 27th of October, 1827, I was called by the committee to make experiments upon some patients at the academy itself. No patients were there. It was an ingenious mode of inviting me to practise on the commissioners. I complied with pleasure. M. Itard, physician to the deaf and dumb, and a member of the Academy, was the first who submitted himself to magnetisation. The following are the words of the report:—

“A more decided magnetic action

was observed on a member of the committee, M. Itard, magnetised by M. Du Potet; he felt a heaviness without sleeping, a decided *agacement* of the nerves of the face, convulsive motions in the ale of the nose, in the muscles of the face, and of the jaws. A flow of saliva in the mouth, which had a metallic taste, a sensation similar to that which he had experienced from galvanism. The following experiments excited cephalalgia, which lasted several hours, and at the same time the habitual pains greatly diminished.”

The reporter adds,

“It is not upon men of our age, and, like us, always on their guard against the errors of our *esprit* and of our senses, that imagination can have much influence. At our period of life it is enlightened by reason, and freed from those illusions by which youth is so easily seduced: at our age it is ever on the watch, and distrust, rather than confidence, presides over the various operations of our minds. These circumstances are happily united in our colleague, and the Academy knows him too well not to admit that what he professes to have felt he really did experience,” &c.

The committee met on the 10th September, 1827, at seven o'clock in the evening, at the house of M. Itard, to continue the experiments on Cazot. “The latter was in the apartment of this physician, where he was engaged in conversation till half-past seven; at which moment M. Foissac, who had arrived subsequently to him, and had remained in the antechamber separated from him by two closed doors, and at a distance of about twelve feet, commenced magnetising him. Three minutes after, Cazot said, ‘I think that M. Foissac is there, for I feel stupified.’ At the end of eight minutes he was fast asleep.”

This experiment was made to ascertain whether magnetism acted at a distance, which all magnetisers affirm to be the case. The following are the trials made to convince themselves that Cazot was not practising deception. Page 54 of the Report:—“Cazot being asleep, a phial full of ammonia was applied three times to his nose, but he did not awake.”

“M. Fouquier thrust a pin, an inch deep, into the forearm; another was introduced obliquely under the sternum, to the depth

of two lines; a third also obliquely into the epigastrium; a fourth perpendicularly into the sole of his foot. M. Guersent pinched his forearm so as to leave an ecchymosis. M. Itard leant upon his thigh with the whole weight of his body. They endeavoured to excite a tickling sensation by passing a small piece of paper under his nose, on the lips, the eyebrows, the eyelids, the neck, and the sole of the foot; nothing could awake him."

The following operation, inserted in the Report, and which I shall quote at full length, strongly corroborates the real existence of this state of magnetic sleep:—

"You have all heard a fact spoken of, which fixed the attention of the section of surgery, and which was communicated to it at the meeting of the 16th of April, 1829, by M. Jules Cloquet. The committee has thought it necessary to record it here, as one of the least equivocal proofs of the force of the magnetic sleep. The patient was a lady, Mad. P—, 64 years of age, residing at No. 151, Rue St. Denis, who consulted M. Cloquet, on the 8th of April, 1829, for an ulcerated cancer in the right breast, which she had had for several years, and which was complicated with a considerable enlargement of the corresponding axillary glands. M. Chapelain, her ordinary physician, who had been in the practice of magnetising her for some months, with the view, as he said, of dispersing the swelling of the breast, had not been able to obtain any other result than that of producing a very profound sleep; during which sensibility appeared to be annihilated, while her ideas retained all their lucidity. He proposed to M. Cloquet to operate upon her during the state of magnetic sleep, and as the latter considered the operation to be indispensable, he consented, and it was fixed for the following Sunday, the 12th of April. During the two days preceding that of the operation, the lady was magnetised several times by M. Chapelain, who, whilst she was in the state of somnambulism, prepared her to submit without fear to the operation, and had even brought her to converse upon it with confidence, although, in her waking hours, she rejected the idea with horror.

"On the day appointed for the operation, M. Cloquet, on his arrival at half-

past ten in the morning, found the patient dressed and seated in an arm-chair, in the attitude of a person in a tranquil natural sleep. She had returned, nearly an hour previously, from mass, which she was accustomed to attend at that time. M. Chapelain had thrown her into the magnetic sleep after her return, and she spoke with much composure of the operation she was about to undergo. All the arrangements being made, she undressed herself and seated herself in a chair. M. Chapelain supported the right arm: the left was allowed to hang down.

"M. Pailloux, *élève interne* of the Hôpital St. Louis, was employed to present the instruments, and to tie the vessels. The first incision, commencing at the axilla, was carried round the upper part of the tumor as far as the inner border of the breast; the second, beginning at the same point, was carried round the lower part of the tumor, till it met the first. The enlarged glands were dissected with precaution, on account of their vicinity to the axillary artery, and the tumor was extirpated. The operation lasted from ten to twelve minutes.

"During all this time the patient continued conversing tranquilly with the operator, and did not give the slightest indication of sensibility; no motion of the limbs, or of the features—no change in the respiration, or of the voice—no emotion even in the pulse, could be perceived: the patient never ceased to be in that state of automatic *abandon* and impassibility in which she had been for some minutes before the operation. It had not been requisite to hold her; they only supported her. A ligature was applied to the lateral thoracic artery, which was opened during the extraction of the glands, and the wound being closed by adhesive plasters and dressed, the patient was put to bed, still in a state of somnambulism, in which she was allowed to remain forty-eight hours. An hour after the operation a slight hæmorrhage appeared, which, however, proved of no consequence. The first dressings were removed on the following Tuesday (the 14th); the wound was cleansed and again dressed, the patient not testifying any sensibility or pain; the pulse preserved its ordinary character. After this dressing, M. Chapelain awakened the patient, whose somnambulant sleep had lasted

from an hour before the commencement of the operation—that is to say, during two days.

“This lady did not appear to have any idea, any *sentiment*, of what had passed; but on learning that she had been operated upon, and seeing her children around her, she was greatly agitated; which the magnetiser put a stop to by immediately sending her to sleep.”

The assertions of the reporter continued:—He cited the history of a somnambulist who had been observed by the committee. This man was sensible at a distance to the action of his magnetiser, who by the sole power of his will was able, when a written order for the purpose was given him, to produce in a part, also designated, very decided convulsive motions. He was awakened, and again cast into sleep, without being touched. The precautions taken to prevent all trickery were given in full detail to the Academy, by the commissioners, who were anxious to show that they had been good observers. But when they stated that this man, during his sleep, had played at cards with several of the members of the Academy, and that he had won of them all, although several endeavours had been made to cheat him, and when they related many other proofs of great lucidity, a general murmur was excited not very flattering to the commissioners. They were first taxed with exaggeration; then the penetration of men whom the Academy had till then held in distinction was called in question. It was yet much worse when the reporter recounted facts still more strange than those already quoted. Several academicians started from their seats, and apostrophised in coarse terms the men who had come conscientiously to relate to the Academy that which they had seen and attested.

An outcry was raised on all sides against the committee; but without being disconcerted, those members of the Academy who believed in magnetism, because they had themselves examined into it, supported the members of the committee. They replied to their antagonists—You do not believe in magnetism; be it so; but in this very place the circulation of the blood was denied; yet the blood does circulate. In this place the physicians who first employed tartar emetic were put on their trial; they were expelled from the Academy; and now we have for colleagues men

who employ it in enormous doses*. Another exclaimed, with a stentorian voice—the institution ridiculed during a considerable time the men who affirmed that they had seen stones fall from the sky; and yet stones do fall. They recalled also the history of inoculation. A deliberation was cited, held by the Academy of that period, in which inoculators were declared cheats and charlatans, and the inoculated, dupes and fools.

At that moment the sanctuary of science had become an arena in which the passions were let loose. Reason had lost her empire. They ought to have judged coolly, and listened to men who were known to be honourable persons; but they suspected the rectitude of their intentions. The meeting continued in the midst of extreme noise and agitation. It was necessary, however, to terminate the discussion. The first question agitated was, whether the report should be printed. It was decided in the negative; but as each member of the Academy wished to have a copy of so extraordinary a report, they were unanimous in deciding that only a very small number of copies, besides those required by the members, should be lithographed.

For a considerable period this report was the universal theme; but, strange to say, the press this time remained a stranger to the discussions which took place; or rather, it declined interfering with them. It feared, no doubt, that it was not sufficiently enlightened on the subject.

I ought here to speak of the facts which met with so much incredulity, and which the Academy would neither adopt nor reject, for it separated without coming to any conclusion, and no one had the boldness again to bring forward the question. These facts will find a place at the conclusion of the history of magnetic somnambulism, of which I shall give an abridgment. But before this narration, allow me to make some reflections. I am so fearful that the truth will be doubted, that I would anticipate in the minds of your readers the objections that may be excited in them by the new phenomena I am about to present to them.

What would they have replied, before

* At that period Becamier administered tartar emetic in doses of 20, 40, 80, and up to 160 grains.

the discovery of the laws of electric and galvanic action, to any one who had assured them that the friction of resinous substances and glass could, by the contact of two metals suitably disposed, produce the astonishing phenomena now universally known? They certainly would not have failed to treat such a man as a visionary—an enthusiast—and to have considered the result of positive experiments as errors unworthy of refutation. Let them take care not to commit a similar error with respect to magnetic somnambulism. If the contact of two pieces of metal, the friction of glass or resinous substances, bodies of which the properties are at an incommensurable distance from those of a nervous system and of the human brain, are capable of bringing forth such extraordinary phenomena, why should it be inconceivable that two nervous systems—two brains placed in certain relations with each other—should be able to produce a change in the ordinary state of these organs, from which the phenomenon of somnambulism results?

That which I present here as an hypothesis is about to become as positive a truth as that which is now so fully established in the belief of all. As difficult as it would be to demonstrate that electrical phenomena do not exist, so difficult would it be to prove to us that we are in error, for our judgment rests on facts as firmly established.

Magnetic somnambulism, the most curious phenomenon recorded in the history of science, will soon open a vast field to observers. We are hitherto scarcely acquainted with it; yet we are already beginning to rectify errors which have long been adopted as truths; and we already inquire, what will become of the knowledge we have amassed with so much labour and care, if it be true that individuals plunged into somnambulism have a particular mode of existence, senses peculiar to that condition, a distinct memory, and an intelligence more active than in the waking state!

Until Mesmer, the human intellect had not yet thought of carrying further the external operation (*l'extérieur*) of our senses, except by augmenting the condition of the sensations; that is to say, by augmenting the internal effect (*l'intérieur*) of the action which objects exercise upon us. It is this which took place by the invention

of optical instruments (microscopes and telescopes.) By this means the darkness has been penetrated which concealed from us an entire universe of infinitely small and infinitely great objects.

To what an extent has not philosophy been benefited by this ingenious discovery! What absurdities has it not demonstrated in the ancient systems of the nature of bodies, and what new truths has it not rendered perceptible to the attentive eye of the observer!

What would the genius of Descartes, of Galileo, of Newton, of Kepler, of Buffon, have produced, without the extension of the organ of sight? Great things, perhaps; but astronomy would have remained almost stationary, and even natural history would have made but little progress. If the extension of one of the senses has been able to produce a considerable revolution in our knowledge, how much vaster a field is about to open to our observation, if, as it is affirmed, the extension of the intellectual faculties can be carried farther than optical instruments have carried the extension of sight.

[To be concluded in another article.]

NOTE FROM MR. SOLLY.

To the Editor of the Medical Gazette.

SIR,

You have done me justice in assuming that the evidence attributed to me by the *Times* newspaper, in the case of Charles Morris, was an error on the part of the reporter. This I supposed must have been too evident, even to the unprofessional reader, to require correction. I have nothing to do with the merits of the case, in either a legal or political point of view. I was called upon by the clerk of the Board of Guardians to make a post-mortem inspection of the body, and to give evidence before the coroner as to the cause of death. I received no information as to his previous history, so that my mind was left wholly unprejudiced, and my evidence was founded solely on the postmortem appearances, the statements relating to his condition, which were given on oath before the coroner.

The following is a copy of my notes, taken by my pupil, Mr. Alfred Taylor, while I conducted the dissection of the body:—

Nov. 24, 1837, half-past 7, P.M.
Clapham Workhouse.

*Postmortem Appearances found in the
Examination of the Body of Charles
Morris.*

External appearances.—Body plump and healthy; under surface dark-red, from gravitation of blood; an open boil on the inner side of the left thigh, just above the condyle.

Head.—Dura-mater perfectly healthy; vessels of the pia-mater rather fuller than usual, but the arachnoid natural; septum lucidum, and anterior part of the fornix, a little softer than usual.

Chest.—No adhesion of the pleura, or unnatural appearance on the right side; very slight and old-standing adhesions on the left. Congestion of the vessels of the mucous membrane of the trachea and bronchial tubes; fluid in all the smaller tubes; lungs crepitate throughout, but contain a large quantity of watery fluid. Pericardium healthy; both ventricles of the heart full of blood; the wall of the left ventricle thick, but not much more so than is usually found in a strong muscular man. A small firmish deposit, about the size of a split pea, on the posterior flap of the mitral valve.

Abdomen.—Peritoneal surface healthy. Bladder full of urine, but not unnaturally distended. Mucous membrane healthy throughout. Liver full of blood; kidneys the same; spleen soft.

I remain, sir,

Your obedient servant,

SAMUEL SOLLY.

1, St. Helen's Place, Bishopsgate-street,
Dec. 5, 1837.

MEDICAL GAZETTE.

Saturday, December 9, 1837.

“*Licet omnibus, licet etiam mihi, dignitatem
Artis Medicæ tueri; potestas modo veniendi in
publicum sit, dicendi periculum non recuso.*”

CICERO.

THE BRIDGWATER UNION.

THE satellites of the Poor-law Commissioners, by the barbarous zeal with which they obey the edicts of their stern

taskmasters, are most assuredly preparing the way for a mitigation, if not a repeal, of the Malthusian Act. Their conduct is every way revolting to the sense of justice as well as of humanity for which this nation has ever been celebrated, and reminds us of the trite but deep adage which asserts, that Providence deprives of their senses those whom it intends to destroy. We doubt, therefore, whether the triumvirate are always gratified with the reckless ardour of their subalterns; for this constant and open defiance of all the decencies and charities of life has a strong tendency to shorten the existence of some of the snuggest salaries going. Hence, as we stated in our number of October 14, when three little children had been put in the stocks, whipped, and starved, in order to cure them of diarrhœa proceeding from weakness, the potent three actually went so far as to disapprove of it: hence, too, although as Malthusians they are bound to believe that pauperism is the great canker eating into the vitals of the nation, we doubt if they will have the courage to bestow a civic wreath (*ob civem trucidatum*) on the men of Clapham, who milled a sick pauper, as we detailed in our last number, and thus cut out a bit of the national gangrene. Lastly, we more than doubt whether our dictators will venture to bestow any applause on the guardians of the Bridgwater Union, who, though they saved at least fifteen shillings at the expense of not more than two or three lives, yet fell into a money-trap which they had baited for their surgeons; and being indebted to them some two hundred and odd pounds, seem likely to appear in no very enviable position in the Court of Queen's Bench.

The conduct of the Bridgwater people, indeed, has been so curious throughout, that we feel inclined to make our readers acquainted with it, as we find it

detailed in a pamphlet now before us*. When the Bridgwater Union was first formed under the new Act, in 1836, the forty parishes of which it consisted were divided into seven districts; and these were placed under the care of seven surgeons (all legally qualified to practise), at salaries varying from 100*l.* to 25*l.* A fee of ten shillings was allowed for every case of midwifery attended by an order from a competent authority.

At the end of the year the Board of Guardians determined to increase the number of districts from seven to nine, and published a notice giving the divisions of the new districts, and the salaries offered for each, varying from 70*l.* to 30*l.* per annum. One of the 30*l.* districts was that of Polden, containing a population of 2240; another that of Cannington, containing 2394; and a third, the Union Workhouse itself. On this last the authors of the pamphlet observe, that—

“The Union house is calculated to contain 300 persons. The salary offered to the medical officer was 30*l.* a year, or one shilling and sevenpence-halfpenny a day. In the miscellaneous estimates presented to the House of Commons, May 30, 1836, are the following items:—Twenty-one Assistant Commissioners, 700*l.* a year each, 14,700*l.*; their travelling and incidental expenses, including clerks, 800*l.* a year each: 16,800*l.* Each Assistant Commissioner, therefore, is charged to the country 1500*l.* a year, or 4*l.* 2*s.* 2*d.* a day; whilst nineteenpence-halfpenny a day is considered a proper remuneration for an expensively educated medical man, intrusted with the lives of hundreds of his suffering fellow-creatures*.”

This is well put, for it is scarcely possible to conceive a stronger contrast than exists between the reward of the Assistant Commissioner, who can live with great comfort, and yet lay by at

least two-thirds of his salary, and that of the district surgeon, who, if he did his duty efficiently, would find the whole of his time occupied, while his salary would not pay for his medicines. The truth is, that whatever political economists may babble about every thing finding its proper level by open competition in the market of talents as well as of teas or turnips, every man of sense is ready to avow that something like a monopoly practically exists in many cases. When the Reform Bill, and its supplementary laws, were debated, Sir James Scarlett objected to the proposed payment of five guineas a day for revising barristers, as being so small that no man of any standing would accept the place; yet an army physician puts up with one guinea per diem, and has certainly more than 1-5th of the acquisitions of his more fortunate brother. The principal reason of the difference undoubtedly is, that legislators are frequently barristers, but very rarely physicians; and while they would feel a great repugnance to see the one profession ground down by open competition, are careless of such a result in the other. The same explanation will apply in part to the case before us, to which we may add, that the framers of the Act were riding their Malthusian hobby with great spirit, and were determined (if we may use such a contradiction in terms) not to be fettered in their scheme of pinching economy by any regard to expense.

Better days, however, are approaching; the medical profession has been so long at the bottom of the wheel of fortune, that any turn must be in their favour; and if the example of the practitioners of Bridgwater is followed by others, with the same spirit with which it has been set by them, the voice of reason and of humanity must again be heard.

* Facts connected with the Medical Relief of the Poor in the Bridgwater Union. Bridgwater, 1837. 8vo pp. 28.

+ Ibid, p. 7, note.

The notice of the Board of Guardians was issued on the 22d of May, 1837, and they no doubt expected that the medical men of the neighbourhood would jump at such tit-bits, and probably offer to do the jobs for less than was proposed. How unspeakable, then, must have been their astonishment, when, on the 2d of June, they found their offers very courteously but very decidedly declined by the seven surgeons of the previous year (Messrs. Toogood, King, Ruddock, Tilsley, Addison, Caswell, and Poole,) who, at the same time, declared their readiness to resume their duties on fair and equitable terms. Nay more; as it is said that the essence of a lady's letter is in the postscript, so this one had a stinging supplement in the shape of a declaration from nine other surgeons practising in Bridgwater, who not only concurred in what the writers of the letter had said, but added, that the poor could not be properly attended by competent medical officers, at the salaries offered, without subjecting them to considerable loss. The medical officers were also encouraged in their determination by all the established practitioners of the neighbourhood; but what must have been the bitterest pill for these most parsimonious of guardians to swallow was, that the Poor-law Commissioners, in their Second Annual Report, have themselves stated that the surgeons are the most proper persons to form an estimate of what should be considered a remuneration for their services. Their words, which are quoted in the pamphlet before us, are, "The elements upon which that calculation must be founded are, in themselves, obscure, and are utterly unknown to the persons who are selected for the office of guardians. The medical practitioners themselves cannot fail to be possessed individually of the knowledge necessary for making the calculation."

The would-be economists were now in a sad quandary; the surgeons had discovered that union is strength, for doctors as well as guardians; and the workhouse senate was consequently in the following dilemma. If they yielded, they might expect a fine jobation from the Infallible Three; and they would unquestionably be reproached for their profligate diversion of public money into the overloaded pockets of country surgeons. Again, if they refused to yield, they might expect to hear from their masters that they had been skinning flints with too unwearied a zeal, and would be quietly referred to the passage wherein it is declared that medical practitioners know the value of their services better than the guardians can. We are far from wondering, therefore, that the Board was gravelled by this sensible letter, and returned no answer.

As no answer arrived, the medical officers again wrote to the Board at greater length; and while they declared their readiness to acquiesce in any equitable arrangement, they offered to attend the poor gratuitously in the interim, provided the arrangement was made within a reasonable period.

On the 9th of June the Board sent an answer, stating that they could not treat upon any other terms than those mentioned in the circular, until the 16th, the day fixed for the election of medical officers for the ensuing year.

Accordingly, "on the 16th of June, they offered to take the charge of the districts marked 2, 6, 7, 8, at fourpence a head; No. 5 at threepence three farthings; and the Bridgwater district at threepence, on the gross population of 1832; and the Union House, which is intended to contain three hundred people, at fifty pounds a year*."

On turning to the circular of the

* Facts, &c. p. 12.

Board, we find that No. 2 is the Huntspill district, containing four parishes, with a population of 3001; so that at fourpence a head, the medical officer would receive 50*l.* per annum. Supposing, then, his outlay for drugs to be 4*l.* per annum (a computation certainly under the mark), his daily stipend would be half-a-crown. Number 6 is the North Petherton district, containing five parishes, with a population of 4679. Fourpence a head will amount to 77*l.* 19*s.* 8*d.* per annum; which, after deducting 5*l.* for drugs, will afford a daily income of four shillings. A regal salary! especially as the surgeon would seldom have more than forty or fifty patients on his list, except, indeed, during an epidemic, when his practical tact would be sharpened by the addition of a couple of hundreds.

It is obvious that the terms proposed by the medical officers themselves are very low—injuriously low, indeed, to all parties*. What would be thought of a surveyor, or attorney, who should propose to do a good deal of work for four or five parishes, every day of his life, at the rate of half-a-crown a day? And in what style would men of sense expect the business to be done? This is the result of the excessive competition which has been the bane of our profession, and which can be remedied only by practitioners generally following the example set them by the surgeons of Bridgwater.

The terms, however, were summarily rejected by the Board. Their next step was to *appoint unqualified persons to two of the nine districts*; and while advertising for medical practitioners, in the London and provincial papers, and in the weekly periodicals, the Board

authorized the surgeons who had before taken charge of the vacant districts, to attend the poor at the same rate as they did their own patients!

The Board, previously to these their moon-struck days, had divided the Polden Hill district into two, for the plain reason that it was too large for any one practitioner to manage; but now, having become demented, they appointed one of the unqualified persons to the Middlezoy district, in addition to the one he had already; making altogether ten parishes, and including a much larger space than the district which they had formerly divided into two, on account of its immense extent.

During the three weeks that each case attended was to be charged separately, the relieving officers were directed to be sparing in their orders for medical relief, and the poor were neglected accordingly. The following case, which we abridge from the pamphlet, is a striking commentary on the new Poor-Law; it should be appended to all future editions:—

Charlotte Allen, aged 31, who had been suffering for many years from a disease of the liver, was reported by the medical officer to be in a critical state, and in daily expectation of her delivery. Nevertheless no order was allowed, and she was delivered by a woman, after a difficult labour, on Friday, the 30th of June. She became worse and worse, but still remained unattended, because the relieving officer said “no order must be given, unless in cases of absolute necessity, as it was *very expensive*.” The attendance of the surgeon was at last allowed on the following Wednesday. He found that the poor woman had been attacked by puerperal fever, and, from the severity of the labour and the unskillfulness of the midwife, had suffered a dreadful laceration, and “a prolapsus of the womb; which is not

* They are lower than those recommended to the Guardians, as a guide to their calculations, in the Appendix to the Second Report of the Commissioners. It is there stated, that from fourpence to fivepence a head appears, under ordinary circumstances, to be considered reasonable, by medical gentlemen.—(Facts, &c. p. 25.)

only irremediable, but must render the remainder of her existence miserable to herself and intolerably offensive to those around her."

Four months after her delivery she was still unfit for anything, and could stand upright only a few minutes at a time. In fact the woman is ruined for life — but then the Union has saved TEN SHILLINGS!

Other cases, for which we cannot make room, next follow, in some of which death obviously resulted from the tardiness of the relief afforded.

After the blood of the poor had been thus "coined for drachmas," the day came for making the medical appointments. The admirable Board, which at first had divided the Union into seven districts, and then, finding these too large, had made nine of them, now determined to reduce them to six; thus punishing the poor for their own pitiful proceedings. Only three new candidates came forwards; while five of the medical officers previously employed wrote a letter to the Board, inclosing their bill from Midsummer; and offering either to take a district on the terms they had already proposed (in which case they would give up the amount of their bills, the salary being made to begin from the 24th of June); or to take another one on the terms proposed by the Board in an advertisement dated June 24th.

The Board first appointed Mr. Tilsley (one of the five) to the North Petherton District, with a salary of 75*l.*, (being only 2*l.* 19*s.* 8*d.* less than the amount at 4*d.* a head), and promised at the same time to pay his bill.

They next called on Mr. King, to whom they proposed the following question, which was not put to Mr. Tilsley: "Whether, in the event of the appointment of any non-resident practitioners who offered in consequence of the advertisements, he would hold any professional communication with them?"

He replied, that since the gentlemen had been brought forward to oppose him, *against whom not the slightest complaint had been made*, he must decline doing so; on which he was rejected by a small majority, and one of the new candidates was elected instead. The same question was asked of Mr. Caswell, and the same answer obtained. He, however, obtained his district by a majority of one. The only remaining district was the Cannington, with which the Board had determined to unite the Stowey district, making, altogether, fourteen parishes, and covering an extent of country ten miles long and eight broad!

A stranger was elected for this district in place of Mr. Ruddock, but he immediately resigned: on this, the Board elected Mr. Ruddock, who refused to undertake the care of so enormous a district, and, by a new arrangement, was allowed to divide it with Mr. Poole.

The Board of Guardians thus sorely buffeted, and willing to show of what stuff they were made, determined to conclude by an act of insolvency. Accordingly, on Friday, Oct. 27, a motion was made, and carried by a majority of one, that the medical officers should be offered double the amount of three weeks of the present salaries in lieu of their bills; that is to say, 38*l.* 13*s.* 0*d.* instead of 248*l.*!

"Honour to bankruptcy!" says Mr. Carlyle, in his History of the French Revolution; and his reason for honouring the generally unhonoured is, that bankruptcy has a wonderful knack of hurling all sorts of lies, bubbles, and frauds, into their native nothingness; it is their natural and merited end.

The narrow majority, too, is a good symptom; for, when boards of guardians fall out, says a new reading of an old proverb, honest men come by their own.

Meantime, the medical officers intend to appeal to the law for the recovery of the money due to them; we wish them every success, as their cause is not only that of justice violated in their persons, but the cause of oppressed poverty and neglected sickness.

THE UNIVERSITY OF LONDON.

WE understand that a very stormy discussion took place last Saturday, in the Senate of the University of London, as to whether candidates for degrees in arts should or should not be made to undergo any examination on the subject of religion. The question was at length decided in the affirmative, leaving Messrs. Warburton and Lubbock, together with their followers, in the minority. By this decision the candidate is to be examined on the Gospels, and some of the elementary works on Theology. The degree in arts is made a pre-requisite to that in physic; and it is expected that the University will be ready to commence *dubbing* about May.

ST. GEORGE'S HOSPITAL.

Siranguled Hernia—Division of the Stricture on the outside of the Sac(?)—Lithotomy—Necrosis from Scrofula—Necrosis from Accident.

WILLIAM BELL, an ostler, 46 years of age, was admitted, October 22d, under the care of Mr. Babington, having been subject to inguinal hernia on the left side for many years, which has been partly irreducible, and for which he has worn a truss. On the 17th, five days ago, a fresh portion came down under the truss, producing pain and sickness for the first two days, since which, the sickness has not been much. He had nothing done for the strangulation till yesterday, when ineffectual attempts were made for a considerable time to reduce it; and he was admitted about 12 to-day. He has had no motion since the strangulation, except after some injections yesterday and this morning, containing very little faecal matter.

The hernia is of large size, eight inches

long, with the testis at the bottom; the scrotum red, and slightly oedematous and painful. The tumor is very hard in the neck of the sac, and contains a good deal of fluid below; it is not very tender except in the neck; there is very little tenderness in the abdomen, which is not much distended. Tongue dry and furred; pulse 84, rather sharp; not great anxiety. He was seen by Mr. Hawkins, in Mr. Babington's absence, who thought it right to operate immediately, from the length of time that strangulation had existed, and the hardness and tenderness of the neck of the sac, and being an old and large hernia with a wide ring, it was thought a proper case to try and divide the stricture without opening the sac. An incision was first made down to the tendon of the external oblique, which was cut, but without its allowing the hernia to be lessened by pressure; the cremaster and edge of the deeper muscles were then divided, so as to expose the outer sac, but no more room was thus gained within; and there was not any trace of hardness or prominence to show whereabouts the stricture was situated, the outer surface being every where quite smooth where exposed by the incision of the cremaster. An opening was therefore made into the sac; and on introducing the finger, Mr. Hawkins said he felt a firm band dipping perpendicularly from the sac round the lower part of the neck, which being divided, the intestine was reduced, and the omentum examined; and as a good deal of it was condensed and hardened, and adherent at the lower part, it was left in the sac. Some difficulty was experienced in sewing up the wound, from the great mass of omentum, and from intestine being forced down whenever the man struggled or moved, which he did a good deal.

At five o'clock (the operation having been performed at half-past one) Mr. Hawkins found the pulse sharp, and a little more frequent, with some tenderness round the wound.

V. S. ad $\frac{3}{4}$ xij. Blood a little buffed; not cupped.

R Hydrarg. Chloridi, gr. iij.; Opii, gr. $\frac{1}{4}$. M. 3tis horis.

9 P.M.—Pulse 120, more sharp; more tenderness.

V. S. ad $\frac{3}{4}$ xvi. Blood much buffed, and cupped.

23d, 1 P.M.—Passed a quiet night, having had scarcely any pain since the last bleeding. Pulse 100, softer. No motion.

R Calomel, gr. viij.; Extr. Coloc. Comp. gr. x. M. statim.

10 P.M.—No evacuation; abdomen more

distended, with complaint of flatulency. Pulse 100, soft.

Omit Pil Cal. et Opii, (nearly 1 dr. of Calomel taken.)

R Magnes. Sulphat. ʒij.; Aquæ Menth. Piper. ʒiiss. M. 2dis horis donec alvus respond.

24th, 2 P.M.—Bowels plentifully open three times, with much fecal matter. Pulse 92, soft.

Omit Mist. R Extr. Coloc. Comp. gr. viij. h. s. s. A pint of beef-tea.

25th —Pulse 84, fuller, but soft. Bowels open well; no pain; feels and looks better.—Half pint of porter.

From this time every thing went on well, except that a little cellular texture sloughed at one part of the wound, which united well, however, every where else, and is now (Dec. 3d) nearly well.

This history is instructive, as it shows that in a case such as is usually considered the most fitted for the operation of dividing the stricture, wherever it may be situated, on the outside of the sac, the surgeon has not always a choice, from the stricture being really produced by the inner surface of the sac; and we believe it is by no means always an easy matter to divide the stricture, even when its place is evident, and it is formed by the outer part or cellular texture, from the very firm texture of the parts the result of inflammation. The case is also an interesting one, from its showing the immediate occurrence of severe inflammation from the operation itself; the pulse rising forty beats in a few hours, and the different appearance of the blood in the two bleedings, both showing how the inflammation was occasioned.

On Thursday, November 30th, three operations were performed by Mr. Hawkins.

John Wheeler, æt. 9, admitted November 17th, having been sounded in the country, and a stone felt. Apparently a tolerably healthy boy, but thinner lately. First experienced pain and difficulty in voiding his urine two years ago; passes it now very frequently, with much pain and straining, and chiefly involuntarily. It is said to be sometimes tinged with blood, and to deposit a sediment. Pain referred to the bladder and loins. Prepuce elongated. Bowels open only about once in three days.

He was sounded on the 18th, but no stone felt. He was kept quiet, and took saline and alkaline medicines, with an opiate at night; under which treatment he became much relieved from his symptoms, so as to sleep for several hours, and retain a good deal of water. The urine

was sometimes acid and at other times alkaline; it contained once a little pus, and was always albuminous.

On the 24th he was sounded again, and a calculus distinctly felt. He took some castor-oil the evening before the operation, and had an injection in the morning, by which means the bowels were well opened.

The operation was done with a common scalpel only, and a good deal of time was occupied in the dilatation of the neck of the bladder, from the size of the stone, which was an inch long by three-quarters wide, and was composed apparently of lithic acid, with a good deal of the triple phosphate beautifully crystallized. A short gum catheter was passed through the wound after the child was put in bed, which was withdrawn two days afterwards (on Saturday), and the water passed freely by the wound. Since the operation the boy has slept soundly, which he could not do before; he has had no bad symptom whatever, and appears to be going on quite well.

The second operation of to day (November 30th) was performed on an unfortunate man, 28 years of age, with a pasty complexion, who has not had syphilis to account for his disease. Two years and a half ago, without any evident cause, he was attacked nearly at the same time with caries and necrosis of the metacarpal bones and first phalanges of both thumbs, of the middle finger, and one great toe, with suppurating buboes in both groins, and on one side of the neck, and with a serofulous abscess on the cheek. The middle finger was so diseased as to push the other fingers out of their situation, and make the whole hand nearly useless; this was therefore amputated, with an inch of the metacarpal bone. An incision was then made along the thumb, and almost the whole of the metacarpal bone and first phalanx removed, these bones being quite dead and insulated, and broken into numerous pieces, one of which, however, was an inch and a half long. Mr. Hawkins said that as both thumbs were diseased in a similar manner, it would be a great object to save as much as possible; and that, although the thumb would probably be shortened by the loss of the first phalanx, yet it frequently happened that enough of the periosteum of the metacarpal bone was saved to form a good deal of new bone, and afford a basis for the second phalanx to act upon, with a kind of new joint. He had already removed a good deal of dead bone from the other thumb, which seemed firmer, and very likely to be useful, though he had

more doubt about the one he had just operated on.

The other operation was for necrosis of the tibia, the result of a railroad accident some months previously. Mr. Hawkins had removed seven or eight inches of dead bone about a month ago, but some more remained. A shell of new bone was cut off with a pair of bone-nippers, and about three inches of the inner and posterior portions of the old bone removed.

Mr. Hawkins said there was a long piece of the old bone still remaining, which he felt half inclined to remove, as it was much insulated in several parts within the new shell of bone, and he was afraid it might die from the inflammation of the parts in a bad constitution, though it was evidently alive at the time of the operation.

There has been a good deal of inflammation in both cases, but they are now going on well.

ACCIDENTS ADMITTED AT THE LONDON HOSPITAL.

Mr. ANDREWS, Surgeon.

Mr. ADAMS, Assistant Surgeon.

Nov.	Sex.	Age.	Case.
20.	M.	60	Fracture of the left os ilii.
	M.	10	Abscess in the axilla from a blow.
	M.	23	Lacerated face and contused wrist.
21.	F.	69	Fractured tibia, with wound.
	F.	28	Fractured patella.
	F.	3	Scald.
	F.	19	Inflamed bursa on patella.
22.	F.	7	Burn.
	M.	60	Burn.
	M.	27	Compound fracture of the thumb.
	M.	37	Injured back.
23.	M.	19	Lacerated scalp and contused back.
	M.	43	Contused foot.
	M.	29	Wounded toe.
	M.	10	Fractured femur.
	M.	45	Injured groin.
	M.	24	Compound fractured fingers.
	F.	11	Injured thigh.
	F.	46	Contused wrist and pelvis.
24.	M.	5	Lacerated scalp.
	M.	10	Contused knee.
	M.	60	Contused knee.
25.	M.	56	Injured leg.
	F.	3	Burn.
	F.	5	Scald.
27.	F.	2	Burn.
	M.	39	Sprained ankle.
28.	M.	6	Phymosis from ligature (a).

(a.) This case was an instance of an accident (if I may so call it) of by no means unfrequent occurrence. The little boy had, for sport, tied a piece of thread tightly around his penis, and left it. It had been on five days, and he had said nothing about it until his mother observed a considerable swelling of the penis. He was admitted with the prepuce quite oedematous, and an ulcerated line was found about the middle of the organ, extending around it. He denied having played any tricks with the penis; but it was clear that a ligature must begirt it: a careful inspection soon proved the correctness of the opinion, for at the bottom of the wound a small thread could be felt; this was divided and drawn out. The edges of the wound had begun to cicatrise. The case speedily recovered.

These cases are highly important to attend to, as without much care the ligature may be overlooked, and the result will be the production of a troublesome form of stricture. In cases, therefore, of phymosis, without apparent cause, occurring in boys and children, it is requisite to search carefully for the cause of obstruction to the lymphatic circulation.

In more than one instance the wound has actually cicatrised over the ligature; but there is usually one spot where the thread may be remarked, and this corresponds to the knot. A case of this description was admitted last year, where many surgeons had overlooked this fact, and the swelling of the prepuce remained unnoticed for twelve months. A careful examination, however, brought to view the knot of a ligature, which was cut and drawn out, and the case rapidly recovered.

MR. WAKLEY, M.P., AND MR. S. COOPER.

To the Editor of the Medical Gazette.

SIR,

It is justice to my friend Mr. Wakley, whose personal character has been grossly assailed by Mr. Samuel Cooper, in the last number of your work, I have to intreat the insertion of the accompanying correspondence in the next number of the LONDON MEDICAL GAZETTE.—I have the honour to be, sir,

Your most obedient servant,

W. BURT.

Dudley Grove, Padlington,
Dec. 6, 1837.

MR. WAKLEY, M.P., TO CAPTAIN W. BURT.

Bedford Square,
Monday Morning.

My dear sir,—I have to request as a particular favour, that you will call, without a moment's delay, on Mr. Samuel Cooper, of No. 7, Woburn Place, Bloomsbury, and inquire whether he is the author of an article which is printed at page 390 of the accompanying periodical work.

Should Mr. Cooper acknowledge that he wrote the communication in question, a circumstance which it is difficult to anticipate, your experience and judgment will dictate the course which should be pursued by, my dear sir,

Yours faithfully,
(Signed) THOMAS WAKLEY.

CAPTAIN WM. BURT TO MR. WAKLEY, M.P.

My dear sir,—I received your note this morning at half past 11, and called at the residence of Mr. S. Cooper at half-past 12, and again at 1 o'clock, but I did not succeed in seeing him until half an hour afterwards, when I met him at the door of a Dispensary in Great Russell-street.

Having stated the object of my visit, Mr. Cooper acknowledged himself to be the author of the article which contains the discreditable attack on your private character, but shrank at once from all personal responsibility; and then talked, as might be expected from such a person, of law and damages.

I am, my dear sir,
Yours very faithfully,
W. BURT.

Dudley Grove,
Half-past 4, P.M., Monday.

[We last week omitted a part of Mr. Cooper's communication; so now we do not think it necessary to insert the concluding portion of the letter from Mr. Wakley's friend, wherein after the narrative above given, he proceeds to the expression of his own opinion regarding the conduct of Mr. Cooper.—ED. GAZ.]

COLLEGE OF SURGEONS.

LIST OF GENTLEMEN WHO RECEIVED
DIPLOMAS IN NOVEMBER.

J. Rowbottom, Huddersfield.—F. Hyett, Cheltenham.—W. Hughes, Cheltenham.—J. T. Donne, Norfolk.—W. Cutlibert, Mendlesham.—J. B. Francis, Hchester.—J. H. Dowling, Newcastle, Limerick.—T. Lee, Dublin.—H. J. Schooles, Jersey.—E. Young, London.—H. Jewell, Cornwall.—E. Wren, Eastwood, Essex.—W. McCulloch, Belfast.—W. G. Jones, Neath.—H. Hales, Brixton.—W. J. Isbell, Plymouth.—R. Caudle, Brighton.—J. Davies, Tipton, Staffordshire.—

A. B. Rye, Banbury.—W. Roberts, Gosport.—G. H. Smith, Radstone, Northamptonshire.—J. W. Gane, Frome.—J. K. Leipscomb, Winchester.—J. McKnapp, E. I.—R. H. Levinge, Lewes.—T. R. Going, Limerick.—J. T. Darvill.—R. C. Roberts, Roabon.—T. Whately, Bedford.—F. W. Mackenzie, London.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED
CERTIFICATES.

Thursday, Dec. 7, 1837.

Wm. Weston, Sedlescomb, Sussex.—Hetman Charles Harris, Gloucester.—John Colebrook, Petworth.—Edward Campbell, Looe, Cornwall.

LITERARY ANNOUNCEMENT.

In the press, Velpeau's Anatomy of Regions. Translated from the French, with Notes, by Henry Hancock, M.R.C.S., Lecturer on Anatomy and Physiology at the Westminster Hospital School of Medicine.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, Dec. 5, 1837.

Abscess 1	Fever, Typhus . . . 6
Age and Debility . . 25	Heart, diseased . . 1
Apoplexy 3	Hoopings Cough . . 8
Asthma 15	Inflammation . . 16
Cancer 2	Brain 4
Consumption . . . 48	Lungs and Pleura . 7
Convulsions . . . 23	Insanity 1
Croup 1	Jaundice 1
Dentition or Teething 2	Liver, diseased . . 1
Diarrhoea 2	Measles 12
Dropsy 2	Mortification . . . 1
Dropsy in the Brain 6	Paralysis 4
Dropsy in the Chest 2	Small-pox 6
Erysipelas 1	Unknown Causes 115
Fever 7	
Fever, Scarlet . . . 4	Casualties 8

Decrease of Burials, as compared with } 39
the preceding week }

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.

Nor.	Thermometer.	Barometer.
Thursday, 30 Dec.	from 22 to 50	29.65 to 29.58
Friday . . . 1	36 46	29.73 29.90
Saturday . . 2	24 34	30.13 30.22
Sunday . . . 3	25 40	30.27 30.34
Monday . . . 4	19 38	30.37 30.34
Tuesday . . 5	32 34	30.29 30.10
Wednesday 6	33 37	30.06 29.50

Prevailing wind, S.W.
Except the 1st and morning of the 4th, cloudy; rain on the 30th ult. and 5th inst., and snow on the afternoon and morning of the 6th.
Rain fallen, .15 of an inch.

CHARLES HENRY ADAMS.

NOTICE.—"Mr. E." We shall be happy to receive the paper.

WILSON & SON, Printers, 57, Skinner-st., London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL
OF

Medicine and the Collateral Sciences.

SATURDAY, DECEMBER 16, 1837.

LECTURES

ON THE

PHYSIOLOGY AND DISEASES OF THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL
AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of
1836 and 1837,

At the Anatomical School, Kinnerton-Street,
near St George's Hospital,

By CHARLES J. B. WILLIAMS, M.D. F.R.S.

Examination of the Chest through the Vital Properties or Functions of its Organs—Analysis of the general Symptoms of Diseases of the Chest—Dyspnoea; its Nature and Value—Frequent Breathing—Feeling of Oppression—Power of Divers, &c.—Professor Faraday's Mode of enabling a Person to hold his Breath—Table of the Causes of Dyspnoea—Cough; its Nature and Causes—Varieties of Cough, and their Causes:—Catarrhal Cough—Hacking Cough—Irritable and Nervous Cough—Convulsive Cough—Whooping Cough—Hooping Cough—Hollow Cough—Varieties, from the state of the Bronchial Secretion, &c.

WE have been occupied in the preceding lectures in considering the physical properties of the chest and its organs, and the manner in which these properties may become signs of the condition of these parts. We have now to examine them through their vital properties, which, combined with certain physical and chemical powers, constitute function. We have already inquired into the elementary vital properties immediately concerned in the function of respiration—sensibility and contractility, to which may be added, the power of secretion; and we then found how closely

these properties are linked together with the chemistry and mechanism of the organs of respiration, so as to constitute their healthy function. Now any excess, defect, or disorder of any of these properties, will be more or less felt throughout the links of this chain, and hence may arise not only derangement of the function of respiration, or *dyspnoea*, but also new phenomena, proceeding from a loss of due balance of the properties, such as *cough*, *expectoration*, and *pain*; and linked as the vital properties are with those of other organs, there may be added disorders of these, in the form of disturbance of the circulation, and its sign the arterial pulse, general fever, disorder of the secretions of the kidneys, liver, and intestines, and of the digestive, nutritive, and sensorial functions. The phenomena arising from these several disordered properties are what are called the *vital or general symptoms* of disease, which we now have to consider in relation to the organs of respiration.

Now it must at once strike you, and I think it will be more apparent as we proceed, that these general symptoms, dependent as they are on such a linking together of many properties, the laws of which are but imperfectly understood, must be far less simple and intelligible than the physical signs; and the variable measure of the vital properties also renders general symptoms far more uncertain than these signs, in their degree, and even in their presence. We cannot with any certainty, as with the physical signs, from a knowledge of the phenomena, and the laws which regulate those phenomena, deduce the condition of the parts which produce them, nor from knowing the condition of parts and physical laws, deduce what phenomena the parts ought to develop. For example, the solids of the body have sensibility, which varies not only in different parts, but in the same parts at different times, and this for reasons which we cannot discover, therefore we cannot calculate on

it. The contractility of moving parts also varies in a similar manner; and we can by no means gain, from the character of their motions, a criterion of their true condition. Instead, therefore, of pursuing the synthetic as well as the analytic method, which we have done with regard to the physical examination of the chest, we shall shortly analyse the chief general symptoms of diseases of the chest, and by that examination endeavour to determine their nature and varieties, and their value in teaching us to discover, to measure, and to treat these diseases.

Dyspnoea, difficult or disturbed breathing, is the most important general symptom of disease of the chest, inasmuch as it implies some interruption to the due performance of some part of the great function of the chest—respiration. Dyspnoea may be caused by circumstances affecting any one or more of the several elements concerned in the function of respiration; viz. the blood in the lungs, the air, the machinery of respiration by which these are brought together, and the nervous system through which the impression which prompts the respiratory act is conveyed from the lungs to the medulla oblongata, and thence to the muscles which move the machinery; in fact, all the causes which in excess produce asphyxia, in slighter degrees occasion dyspnoea. I will presently shew you a table which exemplifies these causes of dyspnoea; but let us now attend to the character of the symptom itself.

When any thing interferes with the sufficient action of the air on the blood, the impression which prompts the act of breathing not being relieved, causes a quicker and fuller repetition of this act, and if the interference still remain, the breathing will continue to be more or less hurried and forced, until the sensation or impression is reduced to the ordinary standard of almost unconsciousness. Now an individual in whom the breathing is hurried may not be sensible that it is accelerated; whilst in another who feels the oppression there may be little appearance of shortness of breath. Again, the feeling of dyspnoea must greatly depend on the condition of the sensorium; for whilst some patients are conscious of the slightest infringement on their respiration, others, particularly in congestive fevers, are brought to the verge of asphyxia without complaining of any oppression. So, too, we are so at times astonished to find, on opening the bodies of the dead, a whole lung diseased, or one side of the chest full of serum, where the patient had not complained at all of dyspnoea; while, in other cases, a much smaller lesion of the organs has been attended with the most distressing orthopnoea. It is, however, rather to the sensation of breathlessness than to merely

accelerated breathing, that the word dyspnoea is generally attached: for translating it as difficult breathing, this expression can be hardly applied when the difficulty is overcome by accelerated movements, of which the patient may not be conscious. But we will not waste our time on words; but advert to frequency of breathing, as well as the feeling of dyspnoea.

The number of respirations in a healthy adult male at rest generally ranges about twenty in a minute. It is more in children and in females, and it becomes increased in all ages, not merely from affections of the lungs or connected organs, but also from general weakness or depressing causes, which, diminishing the strength of the muscles of respiration, oblige them to make up by the frequency of their contractions what is wanting in their energy. Probably there are some nervous conditions of the system also, in which the breathing becomes accelerated, from what Cullen called *mobility*, a greater readiness to move than power to complete the motions. I have seen the breathing hurried in some cases of hysteria, without the patient being conscious of it, and without either real weakness or pectoral disease to account for it. These cases are of no consequence in themselves, but should be known, that they may be separated from those of true dyspnoea. In many other cases, especially those, I believe, where the nervous system is affected, the breathing is not accelerated, but suspirious, a sigh or deep breath being taken from time to time; yet the patient is often not conscious of any oppression or unusual effort. This may be called irregular breathing, and there are several other varieties, which we have not time to consider in detail. The rhythm in breathing probably depends entirely on the chain of influences which we before described as concerned in the act, and not on any peculiar periodicity, such as that which seems to reside in the heart; and therefore irregular breathing must depend on a change in one or more of the links in that chain.

The feeling of dyspnoea is one of a very peculiar and distressing character. Even when slight in degree, its permanent oppressive influence is very wearing; and when severe, it causes the most horrible suffering, with such a feeling of impending death, that the most courageous are often unmanned by it. The constrained postures of the patient, the anxious or even desperate expression of his countenance, the painful straining of all the muscles that can in any way, however distantly, assist in the respiratory movements, bespeak the intensity of the feeling, which I have heard several people say is far worse than the most acute pain. It is worthy of re-

mark, however, that this feeling is experienced in its severest degrees only by those in whom the dyspnoea comes on rather suddenly, especially when the sensibility is entire, and the lungs are not diseased, as in obstructions in the trachea or large bronchi, spasm or swelling of the glottis, and spasmodic asthma. In these cases the sensibility is not gradually blunted by the circulation of imperfectly oxygenated blood; nor has the activity of the functions, which require arterial blood, been lowered by previous depressing causes. Opium, belladonna, camphor, and other narcotics, will sometimes relieve the symptom of dyspnoea, not only by deadening the sensibility, but also by diminishing the activity of those functions and secretions which require oxygenated blood, and therefore a free supply of air. If we could temporarily produce a state approaching to the torpor of hibernating animals, we might diminish the bad effects, as well as the painful feeling of dyspnoea; and I believe that such a state is actually induced in those who are habitually asthmatic, in whom all the functions are brought to a lower standard, and who thus suffer with impunity such an encroachment on the function of respiration as would be fatal to an individual of a common standard.

The feeling of want of breath has been used as a means of testing the condition of the respiratory organs. A person whose respiration is free and unembarrassed, can hold his breath longer than one whose lungs are diseased. Some one, I think Dr. Lyons, has proposed to measure the condition of a patient's lungs by the time which he can continue without taking breath after a full inspiration, and to insure accuracy, the patient is desired to count numbers during this time. A healthy person with a good chest can continue counting for forty-five seconds without taking breath, whilst those with diseased lungs often cannot keep on for twenty seconds. The same objection may be made to this test that we made to the measuring of the exhaled air proposed by Abernethy; that it is a test as much for the strength of the muscles of respiration as for the condition of the lungs. Besides, both the feeling of want of breath, and the power of augmenting the respiratory movements, vary considerably in different healthy subjects. It is well known that divers acquire the power of remaining under water for two or three minutes (it has been said more) without taking breath. In diving animals there is a structural provision to enable them to continue some time without air. The chief venous trunks are very tortuous, and admit of dilatation, so that the venous blood can accumulate in them, instead of distending and embar-

assing the right cavities of the heart and the lungs. Perhaps some change of this kind may be somewhat produced in divers, by the often repeated practice of holding the breath. Professor Faraday has described another mode by which a person may be enabled to hold his breath for a minute and a half, which is double the time usually practicable. This is by making in succession five or six full and forcible inspirations, which seem to so completely change the air in the lungs, that there is left in them a stock of pure air capable of lasting during that time. The knowledge of this fact may be useful, if ever you should want to hold your breath for a time in going into the suffocating atmosphere of a sewer, a mine, a house on fire, or the like, or in diving.

Dyspnoea is often a symptom demanding great attention in diseases of the lungs; but it must be studied in conjunction with the other general symptoms and the physical signs, for in itself it is most vague and inconclusive. You will perceive this on inspecting this tabular view [see next page] of the causes of dyspnoea, which is founded on the physiology of respiration, as described at the beginning of this course. You will find it worth studying, not only in shewing the varied nature and origin of the symptom, but also in contrast with our tables of the physical signs, the causes of which are much less varied, and far more appreciable. But when through the means of the physical and other general symptoms, it has been made out on what cause the dyspnoea depends, then this symptom often becomes a valuable measure of the increase or diminution of the disease, and a useful guide of our practice.

Let us now attend to another symptom, which is even more common in diseases of the chest than dyspnoea is—I mean, the *cough*. The act of coughing consists in one or more abrupt and forcible expirations, accompanied by a contraction of the glottis, trachea, and upper bronchial tubes. The expirations being more complete than usual, especially when there are several of them, are followed by a deep forcible inspiration, the force of which is shown by the loud respiratory murmur, which, by the ear applied to the chest, may be heard to accompany it. The muscles chiefly concerned in the act of coughing are the abdominal muscles and intercostals, as I explained in speaking of the physiology of respiration.

Let us see what is the essential cause of this act: you know that the common cause of cough is phlegm or some other matter irritating the air-passages, and the object or final cause of the cough is to expel or expectorate this matter. Well,

PROXIMATE CAUSES OF DYSPNOEA, OR DIFFICULT BREATHING.

1. BY IMPEDING THE ACCESS OF PURE AIR TO THE LUNGS.

a. *Mechanical.*

Rigidity of parts of the respiratory machine	e. g.	Ossification of cartilages; induration of the pleura; rickety distortions, Tumors or dropsies of the abdomen.
Pressure on ditto	e. g.	{ Effusions in, swellings of, tumors pressing on, the air-tubes ;
Obstructions of the air-tubes.....	e. g.	{ Spasm of the glottis ; spasm of the bronchi.

Compression of the lungs	e. g.	Effusions or tumors in pleural sac
--------------------------------	-------	--

Engorgement.

Alterations in the tissue of the lungs	e. g.	{ Effusions
		{ { Oedema,
		{ { Hepatisation,
		{ { Tubercle, &c.
		{ { Emphysema,
		{ { Dilated bronchi,
		{ { Vomica, &c.
		{ Altered structure.....

b. <i>Chemical.</i>		
Deficiency of oxygen in the air	e. g.	Mephitic gases; rarified air.

c. *Vital.*

Pain of parts moved in respiration	e. g.	Pleurodyne; pleuritis; peritonitis, &c.
Paralysis of muscles of respiration	e. g.	{ Injuries of the spinal marrow in the neck, &c.
Weakness of ditto	e. g.	{ Paralysis of the bronchi (?)
Spasm of ditto.....	e. g.	{ Excessive prostration, from ataxic fevers, &c.
	e. g.	{ Tetanus; spasmodic asthma, &c.

2. BY THE STATE OF THE BLOOD.

a. *Mechanical.*

Obstruction to the passage of the blood.	e. g.	Diseases of the heart and great vessels; tumors pressing on them.
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b. *Chemical.*

An excessively venous state	e. g.	Violent exertion; idiopathic dyspnoea (?)
Deficiency of red particles.....	e. g.	Anæmia; chlorosis.

3. BY THE NERVOUS RELATIONS OF RESPIRATION.

Excessive sensibility of the par vagum	e. g.	Hysterie dyspnoea; cerebral fevers; neuralgia (?)
Defective ditto.....	e. g.	Coma; narcotism, &c. (breathing slow).

I believe we may fairly say that the proximate cause of cough is always some irritation, either direct or by sympathy, of the sentient parts of the air-tube, or of the nerves which render them sentient. Some parts of the bronchial membrane are much more sensitive than others; that lining the glottis and larynx is excessively so, and the least irritation of it is enough to excite coughing. That of the trachea and large bronchi is less, for foreign bodies have been known to lodge in them for some time without causing any coughing, so that some have supposed that they have nothing to do with the production of this symptom; but when the sensibility of these parts is increased by inflammation or nervous excitement, any thing irritating them will also excite coughing. You can easily see why the sensibility of the air-tubes should be greatest at their entrance; it is the *door-keeper*, placed there to exclude, or, by calling other forces to its aid, to expel any thing improper which may intrude. But the other parts of the tubes have also a preserving sensibility, which may bear a little, but is soon roused into activity by continued irritation. We find the parallel of this in the alimentary canal in the natural state. The sensibility that excites the action of vomiting is peculiar to the fauces at one end of the tube; and that which induces the striving of defæcation resides chiefly in the termination of the rectum at the other end; but uncommon degrees of irritation, or an exalted sensibility, will occasion the same actions to be excited by impressions on other parts that are usually insensible: hence arise the vomiting caused by an over-irritated or inflamed stomach or duodenum, and the tenesmus and purging excited by a similar state of the colon. We shall see this more fully on considering the various causes of cough.

As other irritations, cough may be excited either by an unusual irritant acting on the tubes in their natural state, or by the ordinary circumstances, which, although not usually irritating, yet become so by the exalted irritability of the tubes; or, as is the more common case, by a combination of these causes. You have an example of cough excited simply by an unusual irritant, when a portion of food or of bronchial mucus lodges on the membranes of the glottis; and an irritant may act by sympathy as well as by direct application, as when you excite coughing by introducing a probe pretty far into your ear. The cause by increased irritability is exemplified in the cough of early bronchitis and nervous asthma, which the mere inhalation of air is sufficient to excite. You have both an unusual irritant, and increased irritability in the secreting stages of bronchitis, and other

affections, where an unusual quantity, and sometimes an irritating kind, of mucus is poured out on an over-sensitive membrane. This more complex cause of cough is frequently induced by the continuance of the other causes; thus, the continued application of an irritant will develop an increased sensibility, and increased sensibility and irritation will be followed by inflammatory excitement and the secretion of matter, the quantity and quality of which add to the irritation. Thus you see how the physiological causes of cough become identified with the pathology of bronchitis, or inflammation of the membrane of the air-tubes; and in common parlance, you know, a bronchial inflammation is called a *cough*, this being the most prominent symptom. But although this inflammatory condition is often developed by the continuance of causes which produce cough, yet it is not necessarily so, and there may be irritation or increased sensibility, or both, enough to cause cough, and which may yet be short of the degree or the conditions requisite to produce inflammation.

I have said that the irritation which causes cough may not be applied to the bronchial membrane itself, but may be exerted from a part more or less distant. Thus you may have a cough excited by tubercles in the parenchyma of the lungs, by inflammations or irritations of the pleura, peritoneum, stomach, liver, and so forth; and although we may conjecture that these irritations are conveyed through the nervous branches which connect these several organs and the air tubes with one common sensitive centre, yet we cannot explain why they should be sometimes conveyed, and at others not; for although cough does frequently accompany the pathological conditions to which I have just adverted, yet it is very certain that you may often have irritations and inflammations of the stomach, liver, peritoneum, &c., sometimes even of the pleura and pulmonary parenchyma, without any cough whatever. I have heard some one try to explain these discrepancies, by assuming that there must be bronchitis present to produce cough, and that when these several distant irritations do not excite bronchitis, they are unaccompanied by cough; but this view increases the difficulty instead of diminishing it, for it leaves unexplained the reason why this supposed bronchitis should occur in some cases and not in others; and bronchitis, although including cough, is more than cough, and needs something more to produce it. We may conjecture about local weaknesses, constitutional peculiarities, and irregular sensibilities, as causes of these differences, and this is all that we can do towards explaining them, but this is not what an

explanation ought to be; this is referring phenomena not to known general properties, and the laws which govern them, but to individual peculiarities, and undefined influences, the laws of which are not known. These considerations furnish you with another proof of the uncertainty of general symptoms, as means of diagnosis. Still, when cough does occur, and its cause has been made out by the aid of other signs, it deserves attention, not only as a symptom, but as a morbid action of a distressing and hurtful kind, which sometimes may require remedies expressly to relieve it. This illustrates what I have before told you, that general symptoms, although much less constant and instructive than physical signs, with regard to the diagnosis of organic lesions, yet, when positive, often tell us more of those general conditions of the system, which become our guides in the employment of remedies.

Under this impression, I think that we shall do well to examine some of the varieties of cough which present themselves in different cases, and to trace the connexion between their characters, and the variations in elements that constitute them. Of course the study of a symptom in any individual case must be conjoined with a proper survey of its functional or organic cause; but as we have also (sometimes only) to treat the symptom, it is highly useful to study its varieties, and thus to render it more practically instructive.

The cough may vary according to—

1. The irritant exciting it.
2. The sensibility feeling the irritation.
3. The movements thereby excited, which consist of, *a*, the contraction of the muscles of respiration, and *b*, the contraction of the air tubes.
4. The condition of the bronchial membrane and its secretion.

Under these heads, we shall meet with varieties of cough with which, I dare say, you are all familiar.

1. When other things are equal, the violence of a cough will be in proportion to the degree of irritation that excites it. For example, a healthy person whilst eating or drinking, incautiously suffers some food to enter the glottis; the cough thereby excited will be more severe with wine or any thing peppered, than with water or any bland food. So in the early stages of catarrh, although the sensibility of the membrane is increased, yet the thin saline-tasted secretion also acts as an unusual irritant upon it, and keeps up a short teasing, tickling cough, with continued attempts to clear the throat. When the irritation is more moderate, but irremovable, like that occasioned by incipient tubercles in the pulmonary tissue, the

cough will generally be of that slight *hacking* kind, with little or no expectoration, that is so well known as one of the first symptoms of pulmonary consumption. The irritant here remaining the same, the circumstances which increase this cough are those that augment the sensibility of the lung and air-tubes; such as a quickened state of the circulation from exertion, heated rooms, or during the assimilation of stimulating food.

2. We have already adverted to increased sensibility as being concerned in the cough of recent bronchitis or bronchial irritation. It becomes, however, more developed when the cough has lasted several hours, and instead of being short and tickling only, it comes on in more violent and prolonged fits, which are quite irresistible, and often accompanied by a feeling of soreness. The heightened sensibility of the air-tubes is further manifested by the readiness with which breathing air at all cold, or swallowing any thing at all irritating, will excite cough. We have before remarked how this increased irritability of the inflamed air-tubes is commonly joined with the augmented irritation of their secretion; but we do sometimes meet with cases in which the increased sensibility is purely nervous, unaccompanied by any secretion; and the cough is brought on by the slightest cold or irritating matter in the air. Even strong odours will sometimes cause it. These nervous coughs are to be treated chiefly by various remedies which diminish the sensibility of the nervous system, such as narcotics, or sometimes by those which excite stronger impressions in other parts, such as epispastics, and the application of heat.

3. Besides the sensibility of the bronchial membrane, another property connected with the nerves, muscular mobility, may be the source of some varieties of cough. We have no time now to examine the circumstances under which a change of proportionate relation takes place between the action of the motory nerves in general, and the impressions which excite them. It is sufficient for us that the fact is well known, that in certain conditions or states of the system, an ordinary impression will excite inordinate motions; while in others the motions resulting from similar impressions will be imperfect, and below the natural amount. It is thus also with the motions of muscles concerned in coughing. You may have them excessively mobile, so that the least irritation will set them agoing; and like a clock without its pendulum, they continue their impetuous motions, until their strength has fairly run out. This is the *convulsive cough*, which we meet with in some hysterical and nervous subjects, and its convulsive character is the more evident from the fact that it

sometimes alternates with chorca or convulsive affections of other sets of muscles. The same uncontrollable character is, however, often communicated by a nervous temperament or peculiar nervous affections, to coughs arising from common causes, which thus shake and exhaust the patient in an unusual degree, and require appropriate modifications of treatment to subdue them. *Hooping-cough* in its after-stages is of this kind, and from my experience I should say that the shaking uncontrollable nature of the cough is more characteristic of pertussis than the hooping, which is not always present, especially in adults. This leads me to consider on what hooping depends, and here again you will find the use of our physiological divisions which explain some other varieties of cough that are sometimes met with.

In considering the physiology of respiration, we were led to believe that the act of expiration is aided by the contraction of the circular fibres of the air-tubes. In the forcible expirations which constitute ordinary coughing, there is also a simultaneous contraction of the air-tubes, and especially of the aperture of the glottis, through which the air is driven with the greatest force, in order to expel any irritating matter. Now the contraction of these tubes may be excessive, defective, or irregular, and this will occasion other varieties of cough. When their contraction is excessive, being also generally irregular, they give the *whizzing* character to the cough, so remarkable in asthmatic subjects. I do not mean to say that a whizzing cough always depends on contraction of the circular fibres, for other constrictions of the bronchi will also cause it; but if you listen to the chest of a nervous asthmatic, you may often hear, in the forcible expirations of a fit of coughing, sibilant and sonorous rhonchi, which are too transient to be produced by the thickening or secretion of the tubes. Where the irritability of the bronchi is great, their contraction may not, as usual, cease during the act of inspiration; and it is this spasmodic constriction, affecting particularly the upper part of the air-tube during the forcible inspiration which succeeds to coughing, that causes the *hooping* sound. This state of things happens chiefly in the irritable frames of children when affected with convulsive cough, and the violence and repetition of the expiratory efforts of this cough occasion the back draught to be the more forcible, sonorous, and prolonged. If you apply your ears to the chest of a child during a fit of hooping-cough, you will be surprised to find how little sound you can hear there with all these noisy external efforts: in fact, the continued constriction of the bronchial tubes permits very little

motion of air into and out of the tissue of the lungs. In the convulsive cough of adults, again, in which there is no hooping, the respiratory murmur of the long inspiration, or back draught, is pretty loud, whilst the succession of coughs here also consists more of external than of internal movement. In all these kinds of cough, antispasmodics will often give more relief than any other class of remedies.

But we may have an opposite condition of the circular fibres of the bronchi, a weakness or deficiency of action, a paralysis, so that they do not contract as usual during the expiratory efforts of coughing. This constitutes the *hollow* or *barking* cough which we sometimes hear in chronic bronchitis, and now and then in febrile and nervous affections. This cough is, as we shall presently find, accompanied with a difficulty of expectoration; hence it is sometimes very distressing, and particularly so when, as it occasionally happens, it is combined with a mobility of the external muscles of respiration, rendering the cough convulsive and paroxysmal. The tearing and exhausting fits of this kind of cough are sometimes quite agonizing; and we may judge from the bloated, congested appearance of the lips and face, how much these fits impede the respiration and circulation, and how much they may thus tend to increase and perpetuate the diseased condition of the bronchial tubes. In some such cases, I have seen the terebinthinaeous medicines, with external counter-irritation and occasional emetics, give most relief; but the treatment will depend on various circumstances, which I cannot enter into at present.

4. Besides the sensitive and motory apparatus concerned in the act of coughing, we have the secretion of the air-tubes, which may also by its qualities modify the character of the cough. According to whether this secretion is present or not, the cough may be humid or dry; and according to the relation of the qualities of this secretion to the powers of expectoration, the cough may be loose or tight; and these varieties may be combined with the other species of cough, as those may with each other; and thus are produced the endless host of different kinds of cough that we meet with in practice. Without pretending to affirm that you will always be able to classify these by the division that I have now pointed out, I can assure you that I have often found this analysis useful in drawing attention to the predominant changes of vital property, as manifested by this symptom, and in thus distinguishing cases which require different modes of treatment*.

* *Erratum.*—In Lect. VII. p. 276, line 12, 1st col. for “distinct” read “distant.”

ON THE COMPOSITION AND ABSORPTION OF PUS.

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[Continued from p. 413.]

SECT. III.—*Differences of Composition in the several varieties of Pus.*

THE varieties of pus whose differences I have examined, are the creamy, the serous, the tuberculous, the mucous, and also pus mixed with milk.

Concentrated sulphuric acid poured upon pus in a quantity sufficient to dissolve it, produces a reddish tint, which varies extremely in purity and intensity. This reaction, which Berzelius says is constant, engrossed my attention in my first researches into the nature of pus; I wanted to know the reason of its existence and its varieties. I had then just repeated the experiments of M. Raspail upon albumen, and had observed the fine red colour produced by dissolving sugar in sulphuric acid, and pouring it upon albumen. Hence I conjectured that the reason that sulphuric acid produces the same change of colour in pus is, that the latter contains sugar as well as albumen. I was confirmed in my supposition by the sweetish taste which all authors attribute to pus, and which many patients perceive in the sputa containing it, and by the authority of John Hunter, who, in treating of pus, asks if there is no sugar mixed with it.

This supposition, however, has not been confirmed by analysis. Had it been correct, it would have been sufficient, in order to find the sugar, to boil pus in water, and, after filtering, to evaporate the aqueous solution. The extract of this solution would be syrupy and sweet, and by repose would crystallize like the sugar of diabetes; far from this, it is salt, and smells like the juice of meat, and the crystals formed in it are those of the hydrochlorates of soda, potash, and ammonia. These experiments, which I have repeated again and again, both upon pus and upon purulent sputa, which the patients said were sweet, leave no doubt as to the slender foundation of the supposition by which I explained the reddish colour that sulphuric acid gives to some kinds of pus. I then recollected, still following the experiments of M. Raspail, that sulphuric acid gives a red colour, not only to mixtures of albumen and sugar, but also to those of albumen and fat, which made me necessarily believe that pus owed its property of being reddened by sulphuric acid to a

mixture of this kind. The question was no longer doubtful when I had remarked that the fatty matters of pus dissolved in alcohol, and obtained in a separate state by the evaporation of the solvent, took a very fine purplish tint from sulphuric acid; an effect which inclined me to think, that the greater the proportion of these substances in pus, the more intense would be the colour given by sulphuric acid. I verified this supposition by the following experiment:—

Having dried some pus in a water-bath, I exposed its extract several times to the action of boiling alcohol. After each solution, the extract of pus, which before became of a bright red from the influence of the sulphuric acid, took less and less colour from this re-agent, in proportion as the alcohol deprived it more and more of its fatty matters; and when they had been completely removed, the undissolved part was no longer reddened by the acid. I restored this property by mixing it with the fat which the alcohol had taken up. The more fatty matter there was, the more decided was the red tint.

After these preliminary observations, I return to the differential characters of the several varieties of pus.

(a.) *Creamy and serous pus.*—Creamy pus, obtained from hot abscesses, when mixed with sulphuric acid (especially if we stir it and then wait a little), takes a more intense reddish tint than the serous pus derived from cold abscesses. The tint is finer, and the difference equally great, if, instead of adding the acid to these kinds of pus in their natural state, it is poured upon their powdered extracts—a proof that the difference in the intensity of the colour does not depend only on the greater proportion of water in the serous pus, but on the presence of certain elements which remain in the extract. Now these elements are the fatty matters. They seem to me more abundant in creamy pus than in any other; and it is by the greater proportion of these fatty matters that I explain all the properties which distinguish it.

I have said above, that the fatty matters of pus, when mixed with water, give it a milky appearance, and form a real emulsion, which may be compared to almond mixture. It is well known that emulsions hold insoluble substances in a state of suspension, and do not allow them to be precipitated, and in medicine advantage is daily taken of this property, by giving insoluble substances suspended in lozocks. Let it be admitted, then (as analysis has shewn me), that creamy pus contains more emulsive fatty matter than serous pus, and it will be understood why creamy pus does not separate, by standing,

into serum and clot, a separation which one sometimes sees in serous pus—why, in a word, its undissolved parts remain constantly suspended.

If the reader recollects what I have said of the property which these fatty matters have of forming emulsions not coagulable by heat, he will understand a fact which I have often observed, namely, that if creamy pus is boiled, and then filtered after the coagulation of its albumen, the solution which filters through is turbid and milky, and can neither be cleared by acids nor by boiling again. Now this property of affording a milky fluid, even after boiling, belongs to creamy pus alone. It also depends on the proportion of these emulsive fatty matters being greater in it than in other kinds of pus.

Observe, moreover, that this pus belongs to strong patients, and to acute inflammations, which produce a rapid wasting away; while serous pus is found in debilitated persons, and in inflammations which, having lasted a long time, have already attenuated the patient, and now waste him away but slowly: in short, that the first is found in patients who are losing a good deal of fat; and the second, in those who have but little to lose.

Thus chemical analysis, physical properties, and the phenomena observed during disease, all unite in showing that the difference between creamy and serous pus essentially depends upon the greater proportion of fatty matters in the former. The latter, moreover, is more watery, as every author has observed, and as appears from the most cursory inspection.

(b.) *On mucous pus.*—Pus, of a mucous appearance, may be contained in cold abscesses, or secreted by inflamed mucous membranes.

Whether from mucous pus being but rarely contained in cold abscesses, or whether from my attention not being directed to it, I have met with it but once. I punctured a perfectly circumscribed cold abscess, below the right scapula, from which a scrofulous patient, aged 21, had been suffering four or five months; the pus which flowed out was less white and less opaque than serous pus; and it had that semi-transparent tint, that glutinousness, and that disposition to form threads, which we find in mucus. Analysis shewed that it contained the immediate principles common to all kinds of pus. It contained the fatty matters in a scarcely appreciable quantity, was not reddened by sulphuric acid, and after boiling, afforded a perfectly limpid aqueous solution. In its crystallized salts there was very little hydrochlorate of soda, and much hydrochlorate of ammonia. As the appearance of mucus

can be given to any pus by shaking it with a concentrated solution of hydrochlorate of ammonia, I first thought that mucous pus owed its physical properties to the large quantity of hydrochlorate of ammonia which it contained; but having since observed a similar composition in pus proceeding from tubercles on the vertebral column, and in the pus of large cold abscesses, I began to think that the preponderance of this salt belonged peculiarly to chronic suppuration, and debilitated patients. This, however, is a conjecture which requires to be confirmed by new researches.

The mucous pus secreted by mucous membranes has very much engrossed the attention of physicians. Many of them have thought it an important problem to ascertain the distinction between pus and mucus, and to determine when the former is mixed with the latter. To decide these questions, a multitude of empirical experiments have been made: pus and pure mucus have alternately been mixed with water, with solution of potash, or of hydrochlorate of ammonia, &c., on the supposition that some reaction would at last be found to distinguish one from the other. This was a consequence of the erroneous supposition I have already pointed out, that problems in animal chemistry could be resolved by testing such complex products as pus and mucus. To distinguish them it would be necessary to learn to know them when separated, and therefore it would be necessary to analyze them, and reduce them to their immediate principles*. Examining in this way the sputa of chronic pulmonary catarrh, I found that they contained, as pus and blood do, water, albumen, the aqueous and alcoholic extracts of meat, earthy salts, the hydrochlorates of soda and of ammonia, fatty matters forming an emulsion with water, and, lastly, a product insoluble in cold water, hot water, and alcohol. (Most of these substances have been pointed out by Berzelius in the mucus of the nose: vol. viii., p. 463.)

Thus, finding the same immediate principles in pus and in mucus, I concluded that these products probably differ by some immediate principles being more abundant in one than the other; and I was the more inclined to this supposition when I considered, first, that the pus of an abscess

* The best method of distinguishing pus from mucus is that given by Dr. Young. A minute quantity of the fluid to be examined is to be put between two small pieces of plate glass; the glass is then to be held close to the eye, and we are to look at a distant candle, with a dark object behind it: if the fluid be pus, coloured rings will be seen around it; but not, if it be mucus. (Med. Liter. 2nd Edit., p. 574.)—Translator.

may have all the exterior characters of mucus, as in the case above mentioned, and secondly, that we are able to give any kind of pus that semi-transparency, that disposition to form threads, and that adherence of its parts, which seem peculiar to mucus, by mixing and stirring it with a saturated solution of hydrochlorate of ammonia; that is to say, by increasing the proportion of one of its constituents.

It is well known that, at the end of acute pulmonary catarrhs, and during the course of chronic catarrhs (whether they depend on tubercles or not), the sputa often take on a purulent character; this change has no other origin than the more abundant secretion of the emulsive fatty matter, which, even in health, forms a part of the secretion of all the mucous membranes. As it is this same emulsive fatty matter which gives pus its opacity and its yellowish white colour, it is said in such cases that it is pus mixed with mucus; whereas it is merely, as we have seen, an immediate principle common to both, of which the secretion has been augmented in the catarrhal product.

(c.) *On tubercular pus.*—I have nothing to add to what I have said of this pus in two parts of this memoir. As I have already observed, the clots found in it are insoluble in water, in alcohol, and in æther, and cannot be shewn to be different from fibrine by chemical analysis. Its immediate principles are those which exist in every kind of pus. I have made mention of the small proportion of its fatty matter, and of the predominance of the hydrochlorate of ammonia among the salts which are found in the crystals of its aqueous decoction, when placed under the microscope.

(d.) *On pus mixed with milk.*—I have treated of this pus in a memoir on *Fistula Lactea*, inserted in the *Archives de Médecine* for 1836. I might add several cases to those I have already made known, but I reserve them for a new publication. I hope that I shall be able to call attention to ulceration of the lactiferous ducts, a disease so frequent, and so completely neglected, and which can be rationally treated only by being understood.

Here end my observations on the varieties of pus which have not been in contact with the atmosphere; they all combine to demonstrate, that these varieties depend on a difference in proportion between the immediate principles which are common to every kind of pus, and that no kind of pus contains any peculiar principle not to be found in the others. I was long imbued with the common error on this point, and believed in the existence of a tubercular, a purulent matter, and so on; but chemical analysis undeceived me,

and if I continue to publish the results of my researches into these morbid secretions, it will be seen that all these products, so different in appearance, have this in common, that their immediate principles are found in the blood, and that their differences are in the proportion of these principles, their fitness for organization, and the more or less advanced degree of this organization. It will be shewn that, though in animal analysis one may look for albumen, for hydrochlorate of ammonia, or for emulsive fatty matters, we cannot look for pus or tubercle; and perhaps I shall thus contribute to fix, more accurately than has hitherto been done, the point to which the researches of pathological chemistry should be directed, and also contribute to shew the relations of various lesions, their analogies, and their differences.

SECT. IV.—*On the Changes which Pus undergoes when it is in contact with the atmosphere, and putrefies.*

Before treating of the changes which pus undergoes in putrefaction, it is necessary to demonstrate the existence of sulphur in it, and to dwell upon some of the re-actions produced in it by potash, ammonia, and sulphuric acid.

A very simple experiment shews the presence of sulphur in pus, and, in general, in all albuminous fluids. Boil some pus with potash or soda, and then expose test-papers to the vapour which arises from it. You will find that the salts of lead, mercury, iron, &c. will be slightly blackened by the action of the vapour; the salts of antimony will become of a brick-red; the white oxide of arsenic, yellow; in short, that they will take the several tints that sulphuretted hydrogen communicates to them. These effects will be very perceptible if you pour a few drops of sulphuric acid on the pus heated with potash. By the action of the acid thick vapours will be disengaged, which will almost instantly produce upon the test-papers the tints which are characteristic of sulphuretted hydrogen. (To make the experiment succeed well, the neck of the phial should be stepped up by the test-paper.)

It may be presumed that the sulphuretted hydrogen which is formed in the decomposition of pus by potash, combines with the alkali, and forms a hydrosulphate of potash. It seems to me probable, that the decomposition of the pus also produces ammonia, and that a part of the sulphuretted hydrogen is volatilized with it. However, this is merely an accessory circumstance; the fundamental point is the formation of a great quantity of sulphuretted hydrogen, which shews how

considerable is the proportion of sulphur in pus, and, in general, in all albuminous liquids; for I have obtained the same results by acting upon the albumen of the white of egg, upon the serum of the blood, upon dropsical serum, and (which surprised me considerably) upon milk.

When pus is boiled with ammonia, a hydrosulphate of ammonia is formed, which being volatile, rises with the steam of the water; and without the addition of sulphuric acid, this vapour easily acts like sulphuretted hydrogen upon the solutions of lead, mercury, arsenic, and antimony. This experiment, like the first one, shews the presence of sulphur in pus; it shews, in addition, that this sulphur is capable of combining with hydrogen under the influence of ammonia—a fact important to be known in order to comprehend the phenomena of the putrefaction of pus.

I wished to know whether sulphuric acid, when poured upon pus which has not been in contact with the atmosphere, or upon blood, or white of egg, would disengage sulphuretted hydrogen. My experiments produced only doubtful results. Nevertheless, a faint black tint upon the papers dipped in solutions of lead, and a reddish one on those dipped in tartar emetic, gave me reason to suspect the disengagement of a little hydrosulphuric acid.

These premises being laid down, we see that pus containing sulphur, and, like all animal substances, containing hydrogen and azote, has all the elements necessary for the formation of ammonia and sulphuretted hydrogen; very active poisons, which by their combination produce the hydrosulphate of ammonia, a poison not less dangerous.

Now the presence of hydro-sulphate of ammonia with excess of the alkali in putrefied pus, is precisely the point which distinguishes it from pus drawn from an abscess and not exposed to the atmosphere.

Fetid pus is alkaline; and this alkalinity is found even in its vapour, as may be ascertained by placing it in a phial, heating it slightly, if requisite, and then exposing to its vapour turmeric paper or litmus reddened by an acid: the changes of colour which the papers undergo are those which alkalies produce; and as these changes gradually disappear when the paper is exposed to the atmosphere, it is clear that they are owing to ammonia. We have a new proof of the presence of this gas, if we expose a glass rod, dipped in hydrochloric acid, to the surface of the sore which gives forth the fetid pus; the white vapours which this acid disengages become thicker if they were still rising, or

re-appear if they were no longer visible, as soon as the rod is brought near the sore. As to sulphuretted hydrogen, it is easily recognized in fetid pus by placing the pus in a phial, and exposing to its vapour papers dipped in solutions of lead, mercury, or antimony. If putrefaction is far advanced, if the pus issues from a sore or abscess where the cellular tissue is gangrenous, it will at its ordinary temperature give these solutions the black, reddish, and yellow tints, which indicate a disengagement of sulphuretted hydrogen. If the fetor is not so strong, the temperature must be raised; and, in some cases, I have been obliged to add sulphuric acid; but then, unless the development of vapour is very considerable, we cannot be sure that the sulphuretted hydrogen exists ready formed in the pus, as sulphuric acid produces a little with the blood of the healthiest persons.

There is a result which is often observed in surgical wards, and which shows the formation of sulphuretted hydrogen in fetid pus; this is the blackening of the diachylon plaster and lead wash in contact with the pus. This arises from the formation of the sulphuret of lead at the expense of the sulphuretted hydrogen contained in the pus, and of the lead which enters into the composition of the diachylon and the wash; for whenever this change of colour takes place, the presence of sulphuretted hydrogen may be ascertained by the means which I have indicated above.

Many surgeons consider this discoloration as indicating disease of the bones; this is a mistake, for it may be observed in suppuration of the soft parts: and if it is more frequently produced by suppurations arising from the bones, it is because such suppurations are generally deep, and are seated in spots exposed to the atmosphere, and therefore are in conditions favourable to putrefaction and the formation of sulphuretted hydrogen. The formation of sulphuretted hydrogen, and its action upon the blood, suffice to explain a fact in pathological anatomy which I have often heard interpreted by a supposition of which I think I can prove the falsity. At the post-mortem inspection of large abscesses which have been opened during life, and particularly of those which proceed from caries, from tubercles of the vertebral column, or from the hip-joint, nothing is more common than to find the parietes of a very deep black colour. This colour is generally supposed to proceed from gangrene, while it is owing simply to the hydrosulphate of ammonia, and its action upon the blood. I have convinced myself by various experiments, that this

salt communicates a colour to the blood deeper than that of venous blood. Among these experiments I will quote the following one:—

I injected some blood which had been stirred in the air and deprived of its fibrine, into a knuckle of the small intestine; and when the parietes of the intestine had been strongly reddened by the blood, I macerated them partly in a solution of sulphuretted hydrogen and partly in a solution of hydrosulphate of ammonia: in both cases they became of a very deep black, just like the parietes of the abscesses which are supposed to be gangrenous. We may observe, too, that the discoloration of these abscesses depends so much on the action of sulphuretted hydrogen, that it is only found when it has been possible for sulphuretted hydrogen to be formed; and that it never has been found in abscesses which have not been opened—that is to say, in those which cannot contain putrefied pus; and that this tint does not seem to be formed until after death; for on post-mortem examination, it is often recognized before dissection, without having been perceived during life.

I shall terminate this article upon putrefied pus by the following observation. Even when pus blackens diachylon and the lead wash applied to the parts which furnish it—even when its vapour, without raising the temperature, communicates to re-agents all the colours which are produced by sulphuretted hydrogen—these same re-agents, poured into the pus, produce merely white precipitates; that is to say, the same which would result from their action on pure albumen: a new proof of the difficulty of recognizing the most characteristic substances, when they are mixed with albumen, and a proof, consequently, of the necessity of not examining them until they have been freed from this substance, which masks them so completely.

The force of pus, which I suppose to depend chemically upon the hydrosulphate of ammonia, with an excess of the alkali, may, without question, depend upon the putrefaction of pus, which is subject to all the conditions favourable to putrefaction when it is in a large abscess, in contact with the atmosphere, and surrounded by the natural heat of the human body: it is so certain that its putrefaction is the original cause of its force, that this force is never found in the pus obtained from unopened abscesses, and is peculiar to that which has been in contact with the atmosphere, the indispensable element of putrefaction. But when the hydrosulphate of ammonia comes in contact with the tissues of the body, it is absorbed and

carried into the blood, and exhaled by the urine*; but if the hydrosulphate of ammonia, which alters the state of the blood, tends to escape by the urine, it may do also by all the exhaling surfaces, and particularly by those which secrete pus. This makes me believe, that when the blood has been once affected by the absorption of hydrosulphate of ammonia, the pus found on the surface of solutions of continuity does not owe its force only to the putrefaction which it undergoes, but to its mixture with the putrid principles exhaled by these solutions of continuity, as they are by the kidneys. Perhaps, in such cases, the pus of unopened abscesses would show signs of decomposition; but however this may be, the original cause would be the local injury, the putrefaction of pus, and its contact with the atmosphere; which I hold it important to establish, to show the utility of the point which I aim at, in the practical application of my researches†.

[To be continued.]

ON MALIGNANT REMITTENT FEVER.

To the Editor of the Medical Gazette.

SIR,

THE inclosed remarks on the “malignant remittent fever” of the West Coast of Africa, drawn from personal observation, may perhaps be considered worthy of notice in your journal.

I am, sir,

Your obedient servant,

J. MACDIARMID.

High Row, Kensington Lane,
Nov. 16, 1837.

The colony of Sierra Leone has again been visited by one of those fearful outbreaks of disease which periodically threaten the very existence of the set-

* The second part of this memoir will contain the proofs deduced from chemical analysis.

† This is somewhat obscure, but we believe that the following is our author's meaning. He asserts that pus cannot putrefy unless the abscess is open; with this exception, however, that if, in consequence of the opening of an abscess, the pus has become putrid, its hydrosulphate of ammonia may be carried into the circulation, and then deposited in another and unopened abscess, rendering its pus putrid likewise; but even here, he says, the *point de départ*, or origin of its putrefaction, would be the opening of the first abscess.—*Translator.*

tlement, in as far as the European population is concerned.

The points of similarity between the fever of this and of former periods are great; but as some of their features differ essentially, the few remarks to be offered will bear more upon these as facts, than upon the far more interesting but equally intricate questions to which they naturally give rise. The combined causes (whatever they be) which produce such deleterious effects on the human frame, as have lately been witnessed on this coast, would appear to have been in operation early in the year, a decided case of "black vomit" having occurred on board a merchant vessel in one of the rivers, about the end of January—I believe the only one among several cases of fever from the same vessel in which that characteristic symptom appeared. At intervals from this period several suspicious cases were noticed, but the full development of the disease may perhaps be dated from one which occurred on the 16th April, and terminated fatally on the 19th, being followed by an almost uninterrupted series of cases, and, I regret to say, nearly as many deaths.

During May the cases were more frequent, and (if possible) in a more concentrated form; one member of a family after another became its victim, and neither age, sex, temperament, nor habits, appeared to influence it in the slightest degree. It was now, too, spreading along the coast, accounts reaching us from the southward that the men-of-war in that direction were suffering from its effects, at the same time that the merchant vessels from the northern rivers were sending in numbers of the most malignant cases. While the disease, however, was thus sweeping along the shores and neighbouring rivers, the mountain villages were altogether exempt, and the military station, too, in a remarkable degree, although in the immediate neighbourhood of the town. The barracks are elevated about 400 feet above the town, and enjoy an open and free circulation on all sides. Of the officers, the surgeon of the Royal African Corps was the only one attacked; and among the native soldiers only two cases appeared, both of them, however, terminating fatally.

Under these circumstances it may be supposed how anxiously a change was

looked for, and with what joy the first fall of rain was welcomed by those whom the experience of former years had taught its beneficial effects. I am happy to add, that the rains set in heavily towards the end of the month, and their expectations were in some measure realised by the beginning of June.

The disease which thus carried off, in a short period, upwards of one-third of the European residents of Freetown, independent of merchant seamen and others, was generally ushered in by the premonitory symptoms of common fever, occurring during the night in many cases, and described by the patient next morning, on sending for medical assistance, as "not having felt well on going to bed;" sleep absent, or disturbed by uncomfortable dreams, followed by headache, thirst, and restlessness; pains generally over the body, or (what is more characteristic of this particular fever, though not always present) a burning heat and oppression at the precordia. On examination, skin extremely hot and dry; eyes sometimes, even at this early stage, suffused; headache now intense, particularly over the orbits. Pulse 120 to 130, and sometimes strong, but generally weak; tongue dry, furred, and tremulous; pain in the abdominal region, particularly referable to the epigastrium; the headache and the last symptom varying in severity, and seldom present together in any marked degree, the pain in the abdomen being often absent entirely in some of the very worst cases, and those, too, in which the post-mortems displayed extensive patches of inflamed, ulcerated, and phagedenic intestine. Dyspnoea, more or less, an occasional symptom, with nausea, and vomiting of watery mucus, or diluted yellow bile; stools, when procured, copious, dark, and extremely fetid.

About the third day the disease will appear to yield to the treatment employed; headache either entirely gone, or reduced to a slight vertigo on sitting up; pulse moderate; very slight or no irritability of stomach; evacuations much improved in character, with a distinct bilious tinge, with either a tenacious salivary discharge from the mouth, or a decided tenderness of the gums. Amidst all this seeming success, however, some lurking symptom will betray itself to

the medical attendant, realizing in a short time his very worst fears. The case now retrogrades; the patient does not feel so well; had a wretched night; every symptom of salivation has disappeared; countenance and manner anxious. If not previously, eyes now suffused, and skin either of a peculiar dirty hue, or of a decidedly bilious tinge. Evacuations since last visit frequent, scanty, and harassing; appearance watery, dark, and without a trace of feculence, or suppressed altogether; considerable oppression in the breathing; sense of weight at the præcordia.

This state is speedily followed (in some cases in a few hours) by the closing scene; constant singultus, interrupted only by vomiting of coffee-ground matter; alvine evacuations (if any) of the same character, the peculiar factor of both impregnating the air of the whole room, and every article within it. Pulse scarcely to be felt at the wrist; action of the heart much oppressed; extremities cold and clammy, but no pain now complained of in any part of the body; mind clear, and painfully awake to the hopelessness of the case, even within a very short period of the fatal event.

This outline, as far as it goes, is characteristic of a number of the cases. The disease, however, in some ran on to a fatal termination, with only a very slight attack of fever at first, every irregularity of bowels, circulation, &c. being apparently corrected by the treatment employed, the patient continuing to declare, time after time, that no unpleasant symptom remained. In these cases a dry furred tongue, suffusion of the eyes, or the appearance of the skin already alluded to, gave you not only a mistrust of the case, but an almost certain assurance of danger, as too many melancholy instances tend to prove. A sudden oppression at the chest, followed by the "black vomit," are the first symptoms that succeed this treacherous calm, terminating in a state of collapse, miserably lengthened out to a period of many hours, during which, although all sensibility to external impressions appears gone, the audible and laborious respiration, together with the painful gestures of the body, and drawing up of the limbs towards the close, strongly impress you with the idea of intense suffering.

The exceptions to these general characteristics of the disease were cases proving fatal without the appearance of the "black vomit," three of which occurred under my care. There was also violent delirium in some few instances; and a degree of paralysis in two, with imbecility of mind, passing off in the one case, but terminating fatally in the other: the most remarkable exception being a case of recovery, in which the matter vomited, as well as the evacuations by stool, at one period of the disease resembled, and were in fact, the coffee-ground fluids noticed in fatal cases, the appearance of which has been so generally deemed the messenger of death. The patient was a married woman, young, and delicate in appearance, had been about two years in the colony, had suffered from the climate severely, and was the mother of one child at the time, about a twelvemonth old.

Up to the latter end of June, at which time the tornadoes were becoming frequent and heavy, and the fever consequently nearly extinct, the deaths were, to the whole population of Freetown (not including the military), in the proportion of one to three. The number of ladies resident in Freetown at the time was, I believe, nine; of these five were attacked, and three recovered, shewing a less mortality than in the other sex. Of European children there were not more, I think, than six; two of these had slight attacks, and none died. Of the losses of the several ships of war I am unable to speak; and of the merchant seamen I can only state, that the numbers under my care between the 16th May and 7th June were thirty-seven cases of fever, out of which there were eleven deaths.

The treatment of this formidable disease essentially consisted in procuring the constitutional effect of mercury, as soon as possible, relieving the local symptoms in the meantime, as far as the disease would permit, and keeping up a moderate but constant action on the bowels, so as to prevent any accumulation of the vitiated secretions. A scruple of calomel was generally administered at first, followed by the same preparation combined with opium, in the form of three-grain pills, to be taken every two or three hours; or should the scruple dose not have procured several copious stools, another

was ordered, combined with rhubarb (and ipecac., where no irritability of stomach was present,) divided into four powders, and administered every three hours. Leeches to the head, in large numbers, were resorted to, if necessary; and on the slightest uneasiness being complained of in the abdominal or sternal region, a blister, nearly covering the epigastrium, was applied; this was surrounded by five or six dozen of leeches, and the whole of the abdomen and lower part of the chest fomented with pieces of blanket wrung out of boiling water, and applied the moment the leeches were removed, continuing this latter for half an hour, and repeating it at longer or shorter intervals, according to the necessity of the case. Mercurial inunction was never omitted; and warm enemata, simple or laxative, according to circumstances, proved too useful and too grateful an auxiliary to be overlooked, even in the most promising cases, although, on the whole, no remedy afforded such temporary (and I would almost say) permanent relief, as the repeated fomentations. Their application, in many instances, has been speedily followed by some hours of refreshing sleep; in others, by the appearance of a salutary moisture on the surface, assisting the operation of the laxatives administered, and in no case failing, for the time at least, in alleviating the severe pain and oppression so often complained of in the chest and abdomen. The extract of colocynth, with calomel, in the form of pill, was occasionally found useful; and supposing it admissible, quinine, either alone, or combined with a small quantity of calomel, so as to keep up the mercurial action, was administered during the favourable progress of the case. The scruple of calomel, followed, if necessary, by a blister over the epigastrium, or a mustard cataplasm, generally allayed the irritability of the stomach, at least for the time; and the most agreeable as well as most useful drink, was found to be small and repeated draughts of soda in a state of effervescence. Wine, or other stimulants, except when ordered during the patient's recovery, were only employed to keep the vital spark from expiring, in the hope of nature's eventually rallying.

This hope, however, was only realized in one extraordinary instance, that of

the lady already alluded to, in whom the "black vomit" had appeared, followed by a state of collapse affording little prospect of recovery; the extremities had become cold and clammy, and the pulse was scarcely to be felt at the wrist, when warm fomentations to the bowels were ordered, with hot bricks to the feet, and wine in spoonful every fifteen minutes. Nature did rally, and the patient, four days afterwards, was able to be dressed and removed to another house, although the melancholy cause of her removal was her husband's death, from the same fever. Her recovery was again checked a few days afterwards, by another death in the family; yet, in less than a fortnight from the time, she was perfectly recovered—the only survivor in a little family circle of five.

The last remedy I have to mention is blood-letting; but as it is one which has been most sparingly employed on this occasion, and that little with the worst success, I would only say that I am averse to its use in European cases on the coast of Africa, notwithstanding the powerful arguments in its favour which the post-mortems display. My limited experience, however, does not, perhaps, authorise me to form an opinion, for only in three cases was the lancet used, as far as I am aware; in two by the colonial surgeon, and in one by myself. They promised well at first; but delirium came on in two, and a degree of paralysis in the third, and they all proved fatal.

The case I chose for the purpose was that of a young sailor of a healthy appearance, in the earliest stage of the disease, and with nothing apparently contraindicating its use. I accordingly opened a vein in the arm, and allowed the blood to flow till he expressed himself as greatly relieved of a distressing headache, which formed the principal feature in the case. The arm was then tied up, 16 oz. having been taken, and two buckets of water were suddenly thrown over the patient. He was then well dried and put to bed, thanking me for the great relief he had experienced. The next object was to produce the mercurial action on the system; but ere this could be accomplished, the case began to decline, and the symptoms I have already alluded to preceded his death, which took place on the 13th day—an unusually lengthened period,

the deaths generally occurring on the fourth, fifth, and sixth days.

The few post-mortems which time and opportunity permitted, bore the most marked evidence of inflammatory action, and that, too, of an intense degree, in the several cavities of the body, but more particularly and more generally in that of the abdomen. In one native case which I examined, there were large dark patches on the peritoneal surface of the stomach towards its cardiac extremity, the portions of lining membrane corresponding to these presenting an advanced stage of inflammation; traces of inflammation were found in the small intestines; and the stomach, gall, and urinary bladders, contained the dark coffee-ground fluids; that of the gall bladder presenting more the appearance and consistence of liquid tar; the lungs were in a state of sanguineous engorgement throughout, and deluged the table with blood on being cut into. The next native case presented the same traces of abdominal inflammation, but in a more striking degree; the ileum especially, on exposing its mucous surface, shewed one dark mass of vessels, intersected by gangrenous patches, and large excavated sloughing ulcers. The case was that of Christopher Lynch, native private. In a European case examined, which had proved fatal without any appearance of black vomit, that fluid was found in the stomach to the amount of a pint; and the same traces of active inflammation were apparent in that organ, as well as in other portions of the canal, especially in the ileum, as noticed in the two native cases.

Such, then, being the leading characters of the fever of this year, as derived from the personal observation of a large proportion of the cases which occurred, I would now subjoin extracts from the Medical Reports on those of 1823 and 1829, as given by the medical officers on the station, that by comparing them with the description I have ventured to give of the last epidemic, the points in which they differ may be the more easily contrasted.

Extracts from the Medical Report of the Epidemic of 1823, at Sierra Leone.

The fever made its appearance in the early part of the year.

This disease was generally ushered

in with pain in the loins and limbs, but more particularly in the calves of the legs; sometimes distressing pain in the head, and at others great uneasiness about the præcordia, with occasional vomiting and irritability of stomach; the tongue in general white and tremulous, in some cases red and clean, and in a few an ingrained blackness was perceptible, but perfectly different from that collection of sordes which form a characteristic of the typhus gravior, no symptoms of which were observable during any stage of the disease. There was no great degree of heat on the surface of the body, but on grasping the limbs or body firmly, a stinging heat, producing a very peculiar sensation, was communicated to the hand, which it retained for a considerable time. The pulse hurried, and sometimes feeble, but seldom indicating any considerable degree of inflammatory action. No remarkable appearance of the urine; the eyes were generally suffused, and in most cases there was considerable anxiety during the first stage of the disease. This train of symptoms generally lasted for the first thirty-eight or forty hours, by which time the aperient medicine which had been administered had freely operated; but in vain were healthy bilious evacuations sought for. The disease became now evidently manifest in the thin dark fluid which was passed downwards in considerable quantities, and rendered particularly characteristic by innumerable small floating flocculi, which had very much the appearance of the washed and broken-down fibres of the blood.

The patient felt much relieved about this period, and appeared unconscious of his danger; and this delusive state often gave sanguine hopes to the attendants. This calm was followed by a morbid torpidity, or sometimes by low delirium. This state continued until the fourth day, when the inevitable forerunner of a fatal termination, 'the black vomit,' made its appearance; at first in small quantities, and mixed with the ingesta, but afterwards in amazing volumes, and ejected from the stomach with a most extraordinary spasmodic force.

The fluid had the appearance of diluted and broken-down coagululum of blood, and frequently with portions of coagulated lymph, assuming very much the appearance of the inner coat

of the stomach; the fluid gave a dark stain to linen, not easily removed, and had a raw unpleasant odour so perfectly peculiar, that, on entering the chamber, the state of affairs became immediately manifest. In some cases a troublesome hiccup occurred, and in these the matter vomited was less abundant. The state of the patient's mind was most peculiar in the latter stage of the disease: the poor sufferer seemed unconscious of his hopeless state, and generally expressed himself as being much better, until, the vital flame gradually receding from the surface and extremities, dissolution took place; which in some cases was preceded by violent straining of the eye-lids, incoherent expression, or by some convulsive motions. Sometimes, prior to this, a dingy-yellow appearance took place on the body, particularly on the neck and chest.

The post-mortems showed an inflamed state of the villous coat of the intestinal canal, and patches of inflammation and disorganization of the coats of the small intestines; the whole lined with a glairy mucus, and the dark-coloured fluid which constitutes the black vomit.

Observed to be particularly prevalent on the sea-coast. One strange characteristic, however, presented itself—that females and children were entirely exempted from the disease.

The symptoms were not those of an active or ardent fever. In no case could the salivary glands be subjected to the mercurial influence. The only patients who escaped were those in whom early venesection was had recourse to: those cases, however, were very rare, and were more than equalled by the fatal cases under like circumstances and treatment.

Not a single one recovered after the black vomit made its appearance.

Extracts from the Medical Report of the Epidemic of 1829, at Sierra Leone.

The disease was generally ushered in with pain in the loins and limbs, but more particularly in the calves of the legs; very severe headache; the temperature was, in several cases, much diminished, with torpor of the circulating system. Tongue furred, generally ash-coloured; in several cases it was clean, or prematurely red, generally

moist, when thirst was complained of. Eyes watery and suffused; on the fourth or fifth day a dirty yellow suffusion of the surface made its appearance, attended in most cases with a distressing irritability of stomach. As the disease advanced the vomiting increased, and a large quantity of dark fluid, resembling decomposed venous blood, was ejected from the stomach. At this stage of the disease disposition to coma existed, and generally the patient replied that he had no complaint, and was very well; but this delusive appearance was a certain prelude to death.

In the post-mortems, skin of a high yellow; omentum very vascular, and the external surface of the stomach and intestines were highly inflamed. In some cases the colon was dark, and in one the ileum appeared to be in a state of sphacelus, and contained a dark fluid, which, when washed off, a clear transparent mucus appeared tenaciously adhering to the inner coat of the stomach and intestines. On this mucus being scraped off, the inner coat of the stomach appeared highly vascular and red, and the different portions of the intestines that were examined, all presented the same marks of inflammation. In some cases this mucus resembled false membrane, and was detached among the dark grumous contents of the stomach and intestines.

In several of the cases the colon was remarkably contracted in its whole extent. The liver was large, and of a mottled appearance on the surface, and much distended with blood, which rushed forth when that organ was cut into. Gall bladder generally empty; nothing remarkable in the spleen. Evident marks of great determination of blood to the brain existed; the sinuses were distended with congested blood, and the membranes in a high state of vascularity: about an ounce of serum was generally found in the ventricles. The structure of the brain unusually firm."

"Considering the nature of the disease to be essentially inflammatory," says Dr. Sweeny, "bleeding was practised in the first stage, either to the relief of the headache, or to deliquium. Calomel, mercurial inunction, blisters."

No case recovered unless free pyæmia was produced.

Does it not appear, then, from the

history of these successive periods, that while the locality remained the same, and the constitution of the atmosphere, with its barometrical and other changes, afforded nothing remarkable as differing from a former period, still some circumstances must have been in operation, on one occasion or another, to produce the modifications of the disease which have been observed. The disease certainly appeared, in every instance, early in the year, and gradually became milder as the rains fell; and during the last visitation, I could not but remark that cases rarely happened during the continuance of a strong sea-breeze, while, on the contrary, three and four cases have appeared in the day, during the prevalence of the land wind, or in calm weather; those who were ill at the time appearing sensibly to feel its deleterious influence. On this point, however, I would not speak confidently, although such was my impression and such the effect, whatever might have been the cause.

Between the fevers of 1823, 1829, and 1837, the following points are, I think, strikingly contrasted:—

In 1823, "there was no great degree of heat of body;" in 1837, the heat of the body, especially of the trunk, was intense.

In 1823, "females and children were exempt;" in 1837, the former suffered nearly in the same proportion as males, and children were not exempt.

In 1823, the symptoms were stated as not being those of "an active or ardent fever;" in 1837, I would be inclined to say that they were very much of the character of an acute and ardent fever, although the pulse did not generally indicate the very violent inflammatory action which must have been going on to produce the extensive ravages observed in the post mortems.

In 1823, it appears that "in no case could the salivary glands be subjected to the mercurial influence;" in 1837, decided salivation was procured in several cases.

In 1832, "the only patients who escaped were those in whom early V. S. was had recourse to;" in 1837, the three cases in which that remedy was employed proved fatal.

And lastly, in 1832, not a single case recovered after the appearance of the "black vomit;" in 1837, one case recovered after its appearance.

In 1829, again, "the temperature of the body, in several cases, was much diminished, with torpor of the circulating system." This corresponds somewhat with 1823; but in 1837, as stated already, the heat of the body was intense and the circulating system active.

In 1829, the vomiting does not appear to have been an early symptom, not coming on till the fourth or fifth day, or, in fact, till the approach of death: in 1837, on the contrary, vomiting was a very general, and always in such an early symptom often subdued, but returning towards the close with the appearance of the coffee-grounds.

The appearance of false membrane in the course of the canal was not noticed, I believe, in 1837; the opportunities of observation were, however, rare.

In conclusion it is stated, that in 1829 no case recovered unless free ptyalism was produced; while, in 1837, several cases recovered without the appearance of salivation, especially the case of recovery from the black vomit, in which there was no salivation.

ON THE ANATOMICAL STRUCTURE,

AND ON

SOME OF THE MORBID CONDITIONS,

OF THE

VALVES OF THE AORTA.

To the Editor of the Medical Gazette.

SIR,

THE perusal, in your journal, of Dr. Macleod's lectures on rheumatism, in which reference is made to some opinions of mine, induces me to trouble you with a few additional remarks upon the same subject.

In a lecture, reported in the *Medical Gazette*, (vol. xvi. pp. 56, &c.) it was stated by me that the wart-like appearances, seen upon the semilunar valves of the heart, after death, in rheumatic carditis, often form "a kind of double festoon on each valve, from the sesamoid body in the centre to either extremity of its margin, following a *natural line of division between the thinner and thicker portions of the valve.*"

The natural structure here referred to, which I believe to be constant, has re-

ceived but little notice from anatomists. Its relation, however, to a peculiar morbid condition of these valves, makes it worthy of a more particular description.

The cardiac valves consist of a loose duplicature of the delicate membrane that lines the heart; the *endocardium* of modern pathologists. Between its folds is received a thin prolongation of fibrous tissue from the tendinous rings surrounding or constituting the several orifices which are furnished with a valvular apparatus. In the semilunar valves this fibrous substance does not interpose itself between the entire space of the folded membrane. It reaches the free edge of each valve at three points only, namely, at the centre, where it forms the *corpus aurantii*, and at the two extremities. Between these points it stops short, and has a definite limit and outline—a scalloped edge—and so leaves two crescentic portions of the valve, formed merely by the doubled endocardium. In this manner arises the distinction alluded to in my lecture, between the *thinner* and *thicker* parts of the valve. The crescentic edges are thin and transparent; the remainder of the valve is more or less thick, firm, and opaque. This structure obtains in the semilunar valves on both sides of the heart, but it is much more conspicuous in some hearts than in others; and more so always in the valves of the aorta, than in those of the pulmonary artery.

It is along the line of union between the scalloped edge of the thicker portion, and the inner convex margin of the two thinner crescentic portions, that the minute *vegetations* (as they have been called) most commonly arrange themselves. Sometimes they follow that double festoon very exactly and completely; sometimes their continuity is broken, and they straggle a little from the line; but still the general tendency to adhere to it is obvious.

I regret that I neglected to procure accurate drawings of some of the more perfect specimens of this curious arrangement that have fallen under my notice. The festoon was quite regular and entire, and of a deep red colour, (stained so, apparently, by the blood,) upon the aortic valves of a young woman who died about a month ago in the hospital, of diseased heart, and who had suffered many attacks of acute rheumatism. *Figure 2* is engraved

from a sketch of these valves, made at the time by Mr. De Morgan.

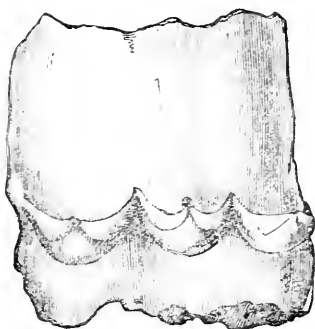


FIG. 1.

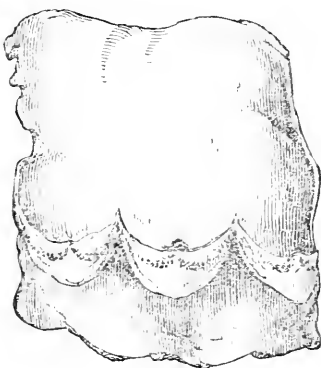


FIG. 2.

Figure 1 represents the *natural* appearance of the valves, as seen in a well-marked example from another subject.

Dr. Budd informs me that he has lately met with a specimen in which the crescentic segments of membrane were wanting. The edge of each valve was formed by a mere string; while the space between this cord and its corresponding arc was open, as if it had been accurately cut out. This appears to have been an extreme degree of that eribriform or perforated condition of the same membranous portion of the valves, which is not very uncommon.

Dr. Hughes has recorded (in the 19th vol. of the *Med. Gaz.* pp. 222, &c.) two very interesting examples of rheumatic pericarditis, characterized by the temporary occurrence of what I have ventured to call the *to-and-fro* sound;

i. e. of a sound resulting from the alternate rubbing of the two surfaces of inflamed membrane against each other in the successive movements of the heart. I was naturally pleased to find that Dr. Hughes had been led by his own experience to the same conclusions with myself, respecting many of the phenomena of rheumatic carditis. On one point, however, he has misunderstood me. He supposes that I refer all the changes which take place in the valves in these cases, to the *smooth surface* of the membrane covering them.

Such, however, is not, nor has ever been, my opinion. I have always believed that this *rheumatic inflammation commences* in the *fibrous* tissues belonging to the heart; and that the "puckering," "stiffening," "increased thickness and opacity*" of the valves, are owing to deposition within their folds; upon or between their *attached* surfaces. In this matter, therefore, I have the satisfaction of agreeing with Dr. Hughes, and, as far as I know, with most other pathologists. But that the free or serous surface also undergoes inflammation, and throws out the products of inflammation, I infer from the fact that the inflamed valves are sometimes folded back, and *adherent*† to themselves or to the walls of the heart or aorta. Of this I have seen several instances.

Dr. Hughes intimates his belief that the granulations or wart-like excrescences are not *morbid growths* from the membrane. Such, also, is, and has long been, my own opinion; and it has been frequently expressed to the pupils in the wards of the Middlesex Hospital. The membrane being inflamed, I conceive that a portion of the fibrin of the blood in contact with it is precipitated upon its surface, and there adheres; just as the retarded blood coagulates and becomes fixed to the lining membrane of an inflamed artery or vein—as a consequence of the inflammation. That this is the true explanation of the origin of these beads of lymph, I was led to suspect by the result of some experiments which Dr. Hope was obliging enough to invite me to witness two or three summers ago in Kimmerton Street. The aortic valves of an ass had been held back by a wire passed into the

vessel, with the view of ascertaining the cause of certain sounds of the heart. The animal was previously rendered insensible by a narcotic poison, and the circulation was kept up—languidly, however, towards the last—by artificial respiration. Soon afterwards, the heart was removed from the body and examined; and little beads of soft lymph, small warty excrescences in fact, were found to have been already deposited on the valves that had been mechanically irritated by the wire.

Does not this fact suggest a practical caution? Is it not probable that bleeding *ad deliquium*, in cases of rheumatic carditis, may favour or determine the formation of these vegetations?

You did me the favour last year, of inserting in your journal the history of a case of carditis, in which I had had, for the first time, the opportunity of verifying, after the death of the patient, the conclusions I had drawn from the occurrence, and the cessation, of the *to-and-fro* sound, during his life. I have since met with a second striking confirmation of the correctness of those conclusions. If you should think it worth accepting, the case shall be transmitted in time for your next number.

I have the honour to remain,

Your obedient servant,
T. WATSON.

Henrietta Street, Cavendish Square,
Dec. 6, 1837.

ON THE
TREATMENT OF SIMPLE FRACTURES OF
THE LOWER EXTREMITIES

BY MEANS OF
PLASTER OF PARIS CASTS.

BY WM. SWEETING, Esq.
Surgeon, Bridport.

THE attention of the profession has of late been a good deal directed to the management of fractures of the extremities, and with the most praiseworthy industry surgeons have laboured to discover means whereby their patients may be saved a modicum of the ordinary pain attendant on fractured limbs, and be rendered as comfortable as is compatible with their situation. With this view Mr. Amesbury recommends his apparatus, which displays considerable ingenuity, and an anxiety for the well-being of the afflicted; this Mr. Radley denounces as cumbersome, and indeed re-

* See MED. GAZ. vol. xvi. p. 58.

† Ibid.

jects all and every kind of splint, and relying on the curative powers of nature, insists that nothing more is necessary than a soft pillow upon which the fractured limb may rest: the generality of practitioners, however, disregarding all extremes, continue to employ the splint and bandage originally recommended by Mr. Pott.

Without meaning to disparage the labours of others, I am of opinion that we are greatly indebted to Messrs. Bond and Gale, of Glastonbury, in Somersetshire, for their valuable communication on the use of plaster of Paris cast, as a substitute for the common splint and bandage in fractures of the lower extremities. Although the employment of this material is confessedly no new discovery, the plan has been certainly lost sight of, or brought into disrepute, in consequence of some badly managed cases. Men are necessarily cautious in adopting any, to them, new course, and wait for the sanction of time, and the testimony of persons of acknowledged reputation, before they venture out of a beaten tract. Knowing Mr. Bond, independently of his professional acquirements, to be a very ingenious man, I felt no difficulty in adopting his suggestion with the regard to the treatment of fractures of the lower extremities by plaster of Paris casts, and within the last two years have had repeated opportunities of employing it, and with a success I could hardly have anticipated.

My object in reporting these cases is to lay before my professional brethren the result of my own experience, and to invite others to make trial of the plan, which I cannot help persuading myself will be, at no distant day, very generally adopted.

The first case in which I was led to employ the plaster cast was that of a man in the neighbourhood, about 40 years of age, named Orchard, over whose legs a loaded cart had been driven, fracturing both very severely: the right suffered a simple, the left a compound fracture. The tibia of the right leg was fractured into three distinct parts; a portion just below the tuberosity, of about three inches in length, being completely detached. My friend, Mr. Southcomb, of this town, was called to the case: the limbs were put up in the usual way, and every attention paid to the man; yet all his contrivances failed, and Mr. Southcomb was daily annoyed at finding the

detached portion of the tibia of the right leg displaced and riding over the others, which its wedge-like form facilitated. In this state of things my assistance was requested, and as it appeared to me to be a case to which the plaster cast would be peculiarly applicable, it was determined to try it: the limb was placed in a double-inclined plane, in a kind of trough, enveloped in the plaster, and thus properly secured: in this apparatus it was retained for five or six weeks, at the end of which the plaster was removed, and the limb found straight and well shaped.

The management of cases wherein *both legs* are fractured is very difficult, and in the country, where proper beds and appliances are wanting, it rarely happens but that some considerable deformity of one leg follows: the difficulty of keeping the leg steady in the necessary movements of the body, especially in evacuating the bowels, is hardly surmountable, and occasions the displacement of the bones. Now this is materially obviated when one limb is so effectively supported as it is by the plaster cast, inasmuch that I was particularly struck with the advantage in Orchard's case, and felt assured that it would be particularly useful in fractures of both legs*.

Since the occurrence of the above case I have attended several others, and treated them in the same way with equal success, and an increasing conviction of the value of the plan. I have now a patient under treatment, a man who for three weeks was treated with the common splint and bandage, but as there was no approach to a union of the fractured ends of the bone, I recommended in consultation the employment of the plaster cast, which was no sooner applied, and the limb at rest, than the man expressed himself delighted with the ease he enjoyed, compared with his former state.

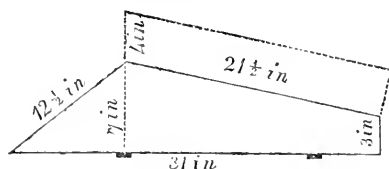
Several advantages accrue from the employment of the plaster cast; such as the uniformity of support given to the

* I had some years since a case of a man whose legs were fractured by a cart passing over them, and all my efforts to keep one leg steady were unavailing: the man declared he could not "mind both legs," nor help moving *one* when the bowels were evacuated. I reasoned with him, and said that his restlessness would bring my fair fame into discredit: he replied quaintly "Never mind, sir, I'll tell every body who may ask me, that *you* cured the right, and *I* the left leg." The former turned out well, but the latter was sadly deformed, although the man was enabled to walk nearly as well as before he received the injury.

whole limb, and its permanently fixed state, without the possibility of motion, and at the same time with freedom from painful constraint or the partial pressure of a bandage, however skilfully applied. Patients continually complain of great pain from pressure on the heel on which the limb, when the patient is placed on his back, rests; this is mainly owing to the imperfect manner in which the hollow space between the heel and calf is filled up: pads become hard, and slip, and we are constantly called upon to make some alteration, to the great risk of displacing the fracture, in order to give the patient ease; and we too often find the patient himself, or some vexatious meddler, interfering and disarranging our plans, and doing considerable damage. This cannot occur when the plaster cast is used; for being employed in a semi-fluid state, it takes the exact shape of the limb, filling up every hollow, and presenting a uniform and perfectly smooth surface for it to rest upon.

Whilst the patient is materially benefited by this method, to the surgeon also it is of considerable advantage, for having once carefully reduced the fracture, and put it in a proper position, no further interference is necessary; hence he is saved a vast deal of trouble, which, in hospital and parochial practice, is highly desirable; but, at the same time, great care and circumspection are necessary, for should the fracture be put up in a hurried or heedless manner, irreparable mischief must ensue, as well to the patient as the reputation of the practitioner.

I am in the habit of placing the limb in a kind of trough of the shape represented in the diagram: it is made of



N.B.—These measurements are taken out and out.

deal; the part on which the leg and thigh rest one inch thick; the sides half inch; the pieces which form the sides of the trough also half inch thick; the first board is moveable, to suit persons of different heights; the width seven inches; and height of the sides four.

The bottom of the trough should be a little hollowed, so as to have a better bed for the leg to rest upon; and two pieces of board projecting from the bottom of each side, about four inches long, to prevent the upsetting of the apparatus. Having poured the plaster into the trough so as to cover the bottom, the limb is put into it whilst in a semi-fluid state: the fracture having been properly adjusted, the trough is filled with the plaster, and the limb retained in its proper position by assistants till the plaster is set, there to remain as long as the surgeon is of opinion that the case requires. It is desirable that a groove should be left on the upper surface of the cast, about a quarter of an inch wide, for the purpose of pouring in an evaporating lotion if required. Mr. Bond recommended a mixture of whiting with the plaster of Paris, but from experience, I decidedly prefer the plaster alone: which after using may be broken up, fresh burnt, and powdered, and employed again and again.

It is hardly necessary to observe that it is desirable that a fractured limb should not in the first place be put up in plaster, but rather suffered to remain at rest supported by a pillow and splint lightly braced upon it, until the swelling and inflammation shall have in a good degree subsided.

I will in conclusion only express my hope, that the plan recommended in this paper may be fairly tried, and that the result of the experience of my professional brethren will be from time to time reported.

Bridport, Nov. 24, 1837.

GRANULATING SURFACE

MEASURING SIX HUNDRED SQUARE INCHES.
RECOVERY.

To the Editor of the Medical Gazette.

SIR,

In presenting the following case to the profession, I am desirous of ascertaining from any authentic source whether I am not warranted in believing it to be the largest sphacelated wound of the integuments recorded in the annals of surgery as having been successfully treated. I have not given it publicity with a view to describe any novel mode of treatment, but as an example of the

amazing power of the system in effecting the restoration of lost structures. Owing to the protracted term of the cure, I am not able to state the daily or weekly condition of the patient; neither would this be interesting to the practitioner; my object, therefore, will be to generalize under its different stages.

I remain, sir,
Your obedient servant,
JOHN GRANTHAM.

Crayford, Kent,
Dec. 5, 1837.

Erith, Nov. 5th, 1835. — Edmund Clark, aged 16 years and 9 months, in consequence of the explosion of some fire-works, was burnt to the following extent:—From the upper and fore part of the neck, extending laterally down the left arm to the insertion of the deltoid muscle; occupying both axillæ; passing backwards to within three inches of the spines of the vertebræ; over the chest, body, and genitals, to the verge of the anus; extending along the upper part of the right thigh, and down the left thigh to the knee, destroying the cuticle, rete mucosum, and corium. The whole measured above six hundred superficial inches, or four feet twenty-four inches, and averaging a quarter of an inch in depth. Also the subcutaneous structure was completely lost, so that the arteries and veins were seen as if neatly dissected, lying on the surface of the muscles and fascia. In recording the treatment it will be better to divide the case into six stages, namely, depression, reaction, exfoliation, ulceration, granulation, and cicatrization. During the whole of the treatment I adhered to three principles: first, a nutritive supply of food; secondly, a proper protection of the wound in reference to the regulation of the animal heat; and, thirdly, the external and internal use of antiseptic agents.

The treatment during the stage of depression, which lasted the first forty-eight hours, consisted in bathing the edges of the destroyed parts with spirit. terebinthinæ, covering the whole of the trunk with flour, and enveloping the patient in a blanket, external to which stone bottles, filled with hot water, were placed at the feet and sides of the patient, and replenished every three hours. He took thirty drops of the tincture of opium, in brandy and

water, every six hours. His diet consisted of beef-tea, thickened with arrow-root. While this depression remained, the sensorium was in a comatose state; the pulse fluttering and rapid. The stage of re-action was, at the onset, attended with great irritability of the brain and violent action of the heart; the pulse ranging between 150 and 200. The stomach so irritable as not to retain any liquid until I administered an enema of beef-tea, highly salted, which acted very beneficially in quieting it. I now gave a drachm of the carbonate of soda, with a drachm of the sp. ammon. arom., according as the stomach could bear them, and applied yeast poultices to the wound where exfoliation had commenced. This stage continued four days. The process of exfoliation, which lasted about twenty five days, was accompanied with a typhoid form of fever, and the effluvia from the sphacelus during this time were most offensive. The application of yeast and linseed-meal poultices was continued, with chalk and cinchona to the exfoliated parts; while the diet was altered to port-wine, beef-tea, &c., and oxymuriatic gas was evolved in the room every two or three hours. Injections of beef-tea, mutton broth, thickened with oatmeal, were also administered every six hours. The stages of ulceration, granulation, and cicatrization, may be said to have commenced from the fourth day, and continued to the end of the case. As the typhoid symptoms abated, I ordered a milk diet; to which article I ascribe, in great measure, the circumstance of the system being able to support such an immense quantity of purulent discharge; the patient taking, on an average, six pints of milk in twenty-four hours. And here I must be allowed to remark on the advantage milk has over all other aliments; though, like every other article of food, it requires discrimination in its use. I believe, in ninety-nine cases out of a hundred, whenever there is fever, the stomach will be found to contain an undue quantity of acid, and in such a state milk would be inadmissible, unless an alkali be taken with it. The wound was now dressed with chalk and cinchona, sprinkled or dredged on it, over which a plaster of spermaceti was laid, and this was covered with a thick layer of wadding or carded wool, and calico

rollers applied with graduated pressure, which was increased as the case progressed.

About three months from the time the accident occurred, the patient suffered from a sphacelated wound in the sacrum; and about four months from this time the unfortunate youth became a sufferer, from an attack of bronchitis, attended with expectoration of blood. When this subsided, I entertained sanguine hopes of his ultimate restoration, as he kept improving more or less, till April 1837, when he was sufficiently recovered to walk a short distance. On the 23d of this month, when apparently in health, he was seized with a fit of shivering, succeeded by erysipelatous inflammation, which increased the dimensions of the wound from 45 inches to upwards of 150 inches, and finally settling in the lowest extremity, destroyed the integuments of the upper part and sides of the foot. The poor fellow's death seemed inevitable, and my expectations, from being raised to the highest pinnacle of hope, became completely depressed. But with a steady adherence to the recumbent posture, the use of small doses of opium, nitric acid, &c., he was again, as it were, recalled to life, since which time the healing has gone on with great rapidity. He is now enabled to ride on horseback a distance of fifteen or sixteen miles in the day, and is free from all contraction, excepting a slight one in the left groin. The wound is reduced to less than twelve inches.

CASES OF

PULMONARY AFFECTION:

WITH REMARKS.

To the Editor of the Medical Gazette.

SIR,

THE following cases of pulmonary affection I have transmitted to you, thinking they would be found worthy of being placed upon record in your journal. I have ventured to add a few remarks upon them.—I am, sir,

Your obedient servant,

T. LAYCOCK,

House Surgeon, &c. to York
County Hospital.

Nov. 22d, 1837.

CASE I.—Convulsive Cough, having first a quotidian, then a tertian type; Sputa very copious, and of a highly faecal odour—Recovery.

Sarah Battley, aged 20 years, wife of Hospital-serjeant Battley, of the 10th Hussars, was attacked in January 1837, with a pain under the right scapula, extending round the side to the anterior lateral region of the chest; it was aggravated when she coughed or breathed, catching her suddenly. She was relieved by bleeding, and suffered from nothing but a slight cough until April 17th. She was then exposed to cold, and experienced some fatigue and anxiety. The pain now recurred, and extended to beneath the right mamma; she had daily pyrexial paroxysms, commencing about noon, and not terminating in sweat. Her cough also became more severe, the expectoration more profuse; and on the 23d she observed the sputa to have a very disagreeable smell, which gradually became more perceptible, and at last highly offensive. The thirst she had suffered from since the commencement of the attack now became excessive. She was admitted into the hospital April 27th, under the care of Dr. Belcombe. The following was her state:—She suckled her first child for twelve months, and weaned it six weeks ago; milk can be squeezed from the nipples. Complaints of violent cough, attacking her in paroxysms, which continue from fifteen minutes to two hours, causing great pain in her head, and accompanied with a profuse expectoration of a muco-purulent fluid, having a distinctly faecal and highly offensive smell. Her bowels have not been moved for two days; she is pale, and has an anxious expression of countenance. Her friends suppose her to be in a *galloping consumption*. There is bronchophony in each upper and anterior region of the chest, most marked on the right; pulse 80, steady and feeble; tongue clean and moist; appetite impaired, and fastidious; thirst excessive, the patient drinking two or three gallons of fluid every day; temper irritable and desponding.

Apply a blister to the upper part of the right side of the chest. To take a dose of Calomel and Colocynth Extract immediately; and every four hours two table spoonful of the following mixture:—

Infusion of Digitalis, Liquor of Acetate of Ammonia, of each 4 ounces; of Camphor Mixture, 8 ounces.

April 27th, evening.—She is obliged to sit up in bed with her body slightly bent forward. Seeing her in this unusual position, and perceiving a strongly faecal smell, I concluded the purgative was

operating, and walked away. I was much surprised, on returning for the purpose of inspecting the evacuations, to be informed that her bowels had not been opened, and that the stench I perceived was caused by her breath and sputa.

28th, evening. — Cough still severe, dyspnœa great; and if she attempt to lie down, is so much increased as to threaten suffocation.

To take with each dose of the mixture a pill, composed of Extract of Henbane and Conium, of each gr. ii. s.; powdered Ipecacuanha and Squill, of each gr. ss.; also, to have dilute Nitric Acid for common drink to any extent.

29th, morning. — Breath and sputa have still the fecal odour; the latter is of a whitish-grey colour and muco-purulent consistence. Cough relieved; pulse 78, full and steady; tongue clean; thirst excessive, — to allay it she has a gallon pitcher at her bed-side full of water acidulated with nitric acid, of which she constantly drinks; bowels constipated. There is an enlarged and painful gland near the right sterno-clavicular articulation.

To have two or three leeches applied to the inflamed gland. Either continue her remedies, take ℞ij. of the Compound Powder of Senna, and have a solution of Chloride of Lime about her bed to destroy the offensive smell.

Evening. — Bowels have been moved once, the fæces of the usual colour and odour; tongue moist, and moderately clean; appetite improved; thirst diminished; pulse 96, round, incompressible; cough much less frequent; sputa in small quantity, and very slightly offensive.

Let her take a Senna draught.

30th, noon. — Slept last night, but coughed incessantly, in paroxysms like whooping-cough, from six to half-past eight o'clock this morning, during which period she expectorated two pints and a half of a muco-purulent dirty grey fluid, having a peculiarly offensive odour, like that of some kinds of discharges from the bowels. Breath not offensive; other symptoms unchanged.

Evening. — Blister irritable, and ordered to be poulticed. Sputa have the odour of decayed apple, and are in small quantity; no fætor of the breath.

May 1st, evening. — Sputa more fecal, but cough and expectoration less.

2d, morning. — Pain in the right anterior and upper portion of the chest; in the same region, puerile respiration and pectoriloquy. On the opposite side, puerile respiration and bronchophony; *no rattle on either side*; sputa and breath of a more fecal odour; pulse 90, full and

steady; tongue clean; appetite good; has slept tolerably well.

Evening. — Has vomited to-day; complaints of being very weak, and sweats; pulse 104, small and soft; tongue moist, and clean; fæces of the natural odour. Has coughed very much to-day, and the sputa is so offensive as to make the ward smell like a foul privy; her breath has a similar smell when she coughs; the sputa are copious, grey, muco-purulent, and swim on water. Cannot lie supine or on her right side, on account of the pain and incessant coughing the attempt to do so produces: is most easy when sitting upright, or leaning forward a little.

3d, evening. — Has been so much better as to walk in the garden; coughed and expectorated less; sputa not so distinctly fecal; can lie on her left, but not on her right side or back.

4th, evening. — Feels great pain beneath the right mamma and scapula. She says the right side feels weak, as if there were no bones in it. Has coughed incessantly during the day; sputa very copious; tongue clean; appetite moderately good; pulse 90, steady.

5th, evening. — Better in every respect. Pulse 114, and soft; sputa much less offensive, and (as she says) of a sweet taste; breath offensive; pectoriloquy on the right side, bronchophony on the left.

6th, evening. — Has had a very severe fit of coughing to-day, with copious and very offensive expectoration; pulse 108; tongue clean. Left the hospital.

A few weeks after this date, I learnt that she soon and rapidly recovered, and joined her husband at Nottingham in good health and spirits. The tertian type which the disease assumed, (a peculiarity I noticed only while copying the case for the press,) is not less remarkable than the quantity and fætor of the expectorated matter.

CASE II. — *Pneumonia; pultaceous, grey, and excessively fetid expectoration — Death in twenty-eight days — Gangrene (?) of the left Lung.*

John Elder, aged 45 years, was admitted on the 27th September last, under the care of Dr. Belcombe, with symptoms of pneumonia. He had severe pain in the left side of the chest (as I was informed), violent cough, and gelatinous adhesive expectoration. On the 2d of October the expectorated matter suddenly increased in quantity, and became decidedly puriform; on the 5th there were pectoriloquy and a distinct blowing, as of air, into a cavity. The breath and sputa exhaled a highly disagreeable fætor, so that I was nauseated while using the stethoscope; the strength was prostrated, and the patient constantly

demanded nourishing diet, which he loathed when offered him.

On the 16th October all the symptoms, which had gradually increased in severity, became much aggravated; the sputa were very copious, of a yellowish grey colour, and excessively offensive. Amphoric resonance could be heard over the whole chest, but very loudly over the second and third ribs. The moment the stethoscope touched the ear, and before it was adjusted, the sound could be felt. His strength now rapidly failed. On the 22d he laid supine, a grey, pulraceous, and horribly stinking fluid, flowing from his mouth. On the 24th he died.

While cutting through the cartilages and intercostal muscles of the ribs mentioned, eighteen or nineteen hours after death, the knife slipped into the cavity in the left lung, and let out a quantity of abominably stinking gas. Raising the sternum, this cavity was found to occupy nearly the whole of the lung. Anteriorly it was bounded by the ribs and softened intercostal muscles; a portion of the third rib near its cartilage was denuded of periosteum. The cavity was half filled by a grey, pulpy, stinking fluid, resembling that expectorated; the lung was of a similar colour, was for the most part softened to the consistence of pap, and when sliced where most solid, the cut surface appeared grey, glairy, and homogeneous. The right lung was dotted with black spots, but in other respects, excepting a few small tubercles, was healthy.

Laennec and Andral give seven cases of gangrene of the lung: in five the left lung was affected, as it was in this case.

REMARKS.—Remarkable factor of the sputa is not a common sign in pulmonary affections. About 1200 in and out-patients of this hospital have come under my notice during the last 20 months; and I have noticed it in none other than the two cases just described. Turning my attention to books, I was surprised to find so little mention made of this symptom. Hippocrates, Arctæus, and the Greek and Latin authors included in *Medica Artis Principes* of Hen. Stephani, A.D. 1567, simply mention it as a bad symptom sometimes observed in phthisis. Galen (*De Febr. Lib. I. cap. 3.*) says, "it is dangerous to be with those affected with *tubæ*, who breathe forth such a putrid stink as to make the room smell strongly." Benedictus (*Tabid. Theat.*), Talpins, and Van Swieten, follow Galen, and considered the disease contagious, in proportion as the breath and sputa were

stinking. Sauvages has a species named *Dyrodia pulmonica*, but merely states that it is observed in some cases of phthisis. Dr. Good, in the first edition of the "Study of Medicine," published in 1822, follows the preceding; nor can I find so much as mention made of gangrene of the lungs. Laennec is the first author I have met with, who refers specially to factor of the sputa. Treating of chronic mucous catarrh, he states the expectoration becomes sometimes more or less fetid, and assumes the smell as well as the other physical qualities of the different kinds of pus, occasionally approaching the gangrenous factor; this may disappear and return in the course of a year*. Van Swieten observed some such cases when he declares, in opposition to the opinions of various illustrious and ancient authors, that he has seen persons who expectorated the most fetid sputa, and yet lived long, and were able to transact their ordinary business. He mentions the case of a youth, as particularly astonishing, who had been ill for some time, when he began to expectorate, early in the morning, sputa so stinking "ut ferre vix potuerim, licet me non adeo delicatam credam, ut a similibus facile afficiat." This expectoration continued for two years without injuring the general health, when it became suddenly increased, the patient emaciated, and death quickly ensued. Andral has given cases in his *Clinique Médicale* resembling this; and Dr. Elliotson, in his excellent and practical lectures†, very clearly draws a distinction between such and gangrene of the lungs. It cannot be doubted that similar cases have been observed in all ages; and included, together with bronchitis, and gangrene, vomica, and tubercular disease of the lungs, under the general term *phthisis*, or *tubæ*. Modern research has taught us to distinguish some of these from each other, but not all, and, when complicated with fetid sputa, they are still confounded.

When fetid sputa are observed in *chronic mucous catarrh*, there is not necessarily any derangement of the general health. Dr. Elliotson has seen persons going about without any par-

* On Diseases of the Chest, &c. by Forbes, 3d edit. p. 75.

† Commentarii apud H. Boerhaave, 2d edit. Leid. tom. iv. p. 72.

‡ MEDICAL GAZETTE, vol. xii. p. 92.

ticular ailment, except that they had a copious expectoration, exceedingly fetid*. Andral mentions a case in which a similar expectoration continued many years†; this patient died of œdema in the lungs; the bronchi were filled with a fluid like that expectorated, and the mucous membranes of the smaller tubes were of a deep red colour. In this case, the expectoration was as copious as if it came from a large excavation; this circumstance is more remarkable in those in which dilated bronchi followed chronic bronchitis or chronic mucous catarrh. Laennec remarks when the dilatation is very extensive the expectoration is sometimes so copious as to simulate the rupture of a vomica‡. This occurred in Case I. related by him. A child, aged three years and a half, brought up by mouthfuls a copious yellow very fetid puriform fluid. After death, bronchi of the left lung were found dilated, and containing a fluid like that expectorated§. Andral gives a case|| in which there was fetid expectoration for four months. Nine or ten weeks before the patient died it was a greyish liquid, which followed in a continued stream, very fetid and in great quantity. There were pectoriloquy, diarrhœa, and other symptoms, from which Lermnier considered the patient laboured under the pulmonary phthisis. After death, a cavity was found in the left lung, formed by a dilated bronchitis, containing a fluid like that expectorated, as also did other dilated bronchi¶. One of the cases given by the same author as gangrene of the lung**, and two by Laennec, as in cases of partial gangrene and of gangrenous eschar††, differ in no essential particular from this, and were really cases of dilated bronchi, with fetid secretion. Indeed, Andral himself seems to have doubt of the correctness of his diagnosis, for, after remarking upon the case, and stating that the grey and fetid sputa are as certainly diagnostic of gangrene, as the red, transparent, and viscid sputa, are of pneumonia, he acknowledges that these very characters induced him to

give an erroneous opinion in a case of fetid tubercular phthisis*. The same remarks are applicable to cases of reported cure of gangrene of the lungs, and this opinion I give the more confidently as it coincides with the sentiments of Laennec himself. He says, "From the result of several cases of recovery, I am tempted to believe that the fœtor and aspect of the expectoration above described, do not necessarily indicate the existence of a gangrenous eschar in the lungs.†" Dr. Elliotson makes a similar remark‡. Confirmatory of this is the opinion of so able and experienced a writer as Forbes, who says, "Gangrene of the lungs is an extremely rare disease. I do not think that I ever met with a case in practice—I do not know where to refer to any case recorded in our medical literature, the nature of which was unequivocal§."

A case of recovery from gangrene of the lungs is related by Dr. Crane, in a recent volume of this journal|. The fetid sputa continued for three months; the general symptoms differed very little from those observed by Andral in the case of chronic bronchitis before quoted. An analogous case is copied into the Medico-Chirurgical Review¶ from the Revue Médicale, in which the diagnosis is derived from the fœtor of the sputa, which continued a long time. For the reasons already given, a diagnosis so made is very likely to be fallacious.

The anatomical characters of this disease differ little from those of chronic bronchitis. Andral and Laennec give six cases. In all, the bronchi contained a fetid fluid, and the bronchial mucous membrane was of a deep red, or a livid colour, and thickened: the bronchi were dilated in five; those of the left lung only in three; and both in two. It seems to be essentially chronic; is characterized by violent cough and profuse expectoration of a fluid of a yellow, green, grey, or other similar colour, and having an abominable stench. The digestive functions and arterial system little disturbed.

The case of Battley seems to be a well-marked instance of the affection

* MED. CAL. GAZETTE, vol. xii. p. 39.

† Clinique Méd. by Spillan, p. 205.

‡ On Diseases of the Chest, &c. by Forbes, 3d edit. p. 112.

§ Ibid. p. 113.

|| Op. cit. p. 295.

¶ Clinique Méd. p. 295.

** Ibid. p. 372.

†† Op. cit. p. 117-235.

* Clinique Méd. p. 274.

† Op. cit. p. 231.

‡ MED. GAZ. vol. xii. p. 134.

§ Trans. of Laennec's Treatise, 3d edit. p. 237, note.

|| MED. GAZ. vol. xii. p. 323.

¶ New Series, vol. ix. p. 165.

existing without any important complication.

Laennec, Andral, and Elliotson, mention five cases of true uncircumscribed gangrene of the lung. In these, the affection was characterized by great disturbance of the arterial system and digestive organs, great prostration, cadaveric paleness of the countenance, emaciation, and death, before or about the thirtieth day. The anatomical characters may be thus stated:—The pulmonary and costal pleuræ of the side affected have contracted strong adhesions; the lung affected is hollowed into a cavity containing a very fetid and pulpy or pap-like matter. The surrounding parts are hepatized at some distance from the cavity; are of a grey, chocolate, or dark green colour, and terminating in a soft, greenish, or grey *pourriture*, which forms the parietes of the cavern.

This description so closely resembles that of the form of phagedæna termed *hospital pulpy*, and *contagious gangrene*, *malignant* or *sloughing sore*, and *gangrenous phagedæna*, that one might be inclined to suspect there was some analogy between the two diseases.

When I commenced my letter, I proposed entering upon an inquiry into the nature and causes of the fætor of the sputa in certain cases of chronic mucous catarrh, but I fear I have already trespassed too much on your pages. I may be permitted to add, however, that this disease, curable in itself, may present all the *physical* signs of tubercular phthisis, and of vomica and gangrene of the lungs; and, in the latter stages, most of the *rational* signs of those diseases. Accurate and practised observers have experienced this difficulty in diagnosis, and it is not improbable that many of the cases reported as cures of tubercular phthisis, have been aggravated forms of chronic mucous catarrh.

REMARKS

ON

A NOTE IN DR. ELLIOTSON'S
PHYSIOLOGY, PART II.

To the Editor of the Medical Gazette.

SIR,

SOME of your readers probably will recollect that soon after the publication

of Professor Elliotson's *Physiology*, Part I., I took the liberty of animadverting, in two letters, to be found in the *MEDICAL GAZETTE* for November 1835, on the views of that gentleman concerning life and mind. My purpose was, in a candid spirit, to refute certain notions, generally current by the name of *materialism*, which he thought it right, in very plain terms, to make public: and this I undertook not *uninvited*, for in his Preface he had solicited of his readers, as a valuable favour, the correction, either "publicly or privately," of any errors they might discover in the work. The points which I took upon me to examine are chiefly contained in such assertions as these:—"The brain thinks, feels, and wills, and has this power, medullary matter though it be, as obviously as that the liver secretes bile; brutes are as really endowed with mind as man; every child is conscious that it thinks with its head; the qualities of mind are corporeal conditions; when the body dies the mind ceases." There were several other points, as also much inaccurate reasoning concerning life and the qualities of matter, but the above particulars were the most important. It was my object to prove, and I trust I perfectly succeeded in proving, that the mind, which by the help of the senses examines, perceives, and reasons, concerning external objects, is, *in its nature*, different from all objects it thus examines, perceives, and reasons about; that, indeed, no rational being *is able* to believe that a thing (suppose the brain within his own skull) which he can think about as an object of sense, is identical with his thinking mind; and, consequently, that if the brain be really the thinking principle, we not only *can never know* the fact, but, by a necessity of nature, are compelled to believe the contrary.

A few months subsequently, I republished my letters somewhat amplified, with the addition of one or two chapters and a few notes, in the form of a small volume, which I entitled "Critical Remarks on certain recently published Opinions concerning Life and Mind." Not having the honour of a personal acquaintance with Dr. Elliotson, I hesitated to send to him a copy of the work as from the author. I, therefore, adopted a different course; as it turned out, an unfortunate one, for I requested a mutual friend to send a copy, and I was sorry to find that it was immediately

returned by Dr. Elliotson, accompanied (if I do not mistake, and I believe I am correct) with an angry letter.

Here the affair rested. A few days ago Part II. of Dr. Elliotson's *Physiology*, "On the Animal Functions," recently published, came into my hands. In glancing over the pages, I discovered that a considerable space, as might be expected, is devoted to a display of phrenology; and somewhat, perhaps, out of place, two lengthy notes to his favourite topic—materialism.

In these notes I find such profound and important announcements as the following:—

"I consider that a soul stands upon the same foundation as a Centaur or a Briarius." "Our own minds, and those of all other animals, are known to us only as powers generated merely by matter." "The brain, matter though it be, is seen, in positive fact, to have these [mental] capacities and endowments." "The properties of simple life, such as vegetables have in common with us, have neither dimensions, nor weight, nor colour, nor form, &c. They, I suppose, are not now ascribed to a soul, though they once were, and ought to be still, by such believers in souls." "The vital properties of a cabbage, I presume, are allowed to result from a well mixed combination of elements; and if such a combination produce such a result, other combinations *may* and *do* produce results still higher." "What property of dimension, weight, &c. is that possessed by mercury, iron, and so many other elements, of variously affecting living natures both corporeally and mentally?" "The vital and mental phenomena are unexampled in the inanimate world, result from no combination nor organization there, *simply because no such combination and organization occur in the inanimate world.*" "No mind exists in nature but as a property or power of matter. We never see mind." "We internally feel our personality in that part of space where our head is." "We possess no such thing as an immaterial soul." "If an animal can live when divided into two or more, its mind can do the same; so that a *planaria's* consciousness may be made into two or ten if we please; each new animal made from sections having its sense of personality, and, therefore, its pretension to an immaterial principle as much as the original, and as much as

the philosopher." "Cases occur in which a man has the consciousness of two persons*."

These passages (there are many others not less curious) which although separated from their context have each an independent meaning, will enable the reader to form a tolerably correct estimate of their author's uncommon profundity, and of his fitness for maintaining and propagating his *new* doctrine, so elevating and *flattering* to human nature. Part II. is throughout a singular and amusing production, covertly intended by its author, perhaps, to exhibit an illustration of his hypothesis, that it is *the brain that thinks*: and to prove how well an author can write who has discovered that he is destitute of a soul.

It is time I should inform the reader what it is that induces me at this time to notice, however cursorily, Dr. Elliotson's opinions. In the pages which have furnished me with the foregoing passages, I find one passage to which I take the liberty of requesting particular attention. "The doctrine of the existence of mind independently of matter [a notion, by the by, never maintained by those thoroughly conversant with the question] indicates a want of modern knowledge, and involves us in endless absurdity. *Its studied display usually proceeds, in our profession, from rank hypocrisy and malice, as though a materialist may not be a devout Christian; and these Pharisees say aloud, 'I thank thee, Lord, I am not as other men are, even as this materialist.'*"

As one of "the profession," also one who, in opposition to Dr. Elliotson, has made a "studied display" in favour, not of "the existence of mind independently of matter," but of the mind's being in its nature distinct from matter, it would be affectation in me to doubt, and in Dr. Elliotson to deny, that there is allusion to myself in the foregoing sentences. I say I cannot but conclude, coupling these with the private transaction before referred to, that such is the case; and this, I may say, is the opinion of more than myself. Lest, however, I should be assuming for myself too much importance—an error I am solicitous to avoid—I will suppose that I am alluded to only in common

* Human Physiology, by Dr. Elliotson, 5th edition, Part II. pages 360—365.

with that part of the profession who are guilty (apparently a most provoking offence) of disclaiming the learned Professor's materialism.

The allusion is of so offensive a nature that it is perplexing to know how it should be dealt with. Against the heavy charges it contains I will, if not for myself alone, as one of the implicated, offer a very brief defence. Having a slight taste for speculative inquiries, I wrote a criticism on Dr. Elliotson's views, partly as a mental exercise, but chiefly to maintain what I regarded as important truth. Dr. Elliotson had publicly challenged the judgment of his readers, and in offering mine I meant no offence. By the charge of "rank hypocrisy" may be understood an *affected* concern for religion. If this be the meaning, I think a reply unnecessary; or, it may mean that the opposition to Dr. Elliotson's sentiments was insincere; flowing not from a regard to the interests of truth, but from sinister and discreditable motives. My answer to this would be, that I now am, and have long been, sincerely persuaded, that materialism is both *illogical* as an hypothesis, and extremely pernicious in its moral tendency. As to the charge of malice, having no personal knowledge of this eminent physician, it is not easy to fancy a ground for such a feeling on my part. The learned professor knows human nature too well to be ignorant that often, when an author is the most loudly complaining of the stings of envy and malice, his sufferings are quite of another kind; that they are inflicted by the darts of contempt.

To say nothing of the bad taste in parodying a portion of Scripture, who has said that a materialist may not be a "devout Christian?" I have not. If a materialist professes to be *devout* it is only reasonable he should be believed. In asserting, on a former occasion, that Dr. Elliotson's views lead by no circuitous route to Spinozism, I was careful to add, that I did not impute to him the holding such a doctrine. But this gentleman, in the warmth of his antipathy, stigmatizes his opponents with the name of Pharisees: leaving it to be inferred that he, for his part, belongs to another and more honourable sect—the Sadducees. Surely he must be aware that the latter were not reckoned "devout."

What, let me seriously ask the reader,

are the doctrines, promulgated by Dr. Elliotson, whose rejection, on the part of his professional brethren, shows a "want of modern knowledge," and which he assumes they ought to accept at his hands with submissive reverence? They are—that the existence of the soul is an idle fable; that when the body dies the mind ceases; that a piece of clay, a pot-herb, and a man, alike matter, differ only in the component elements of each being differently combined and arranged; and that we are acquainted with nothing besides matter, the principle which *apprehends* being identical, as to nature, with *all the things* which it apprehends! If this be modern knowledge, I prefer, for me and mine, *that* that is more ancient.

But, some will be ready to exclaim, such an hypothesis is too absurd to do harm, there being little risk of such *illogical* views obtaining any considerable currency; for that, even should the young student, unhappily, be tempted to angle for knowledge in these turbid and shallow waters, he will speedily discover how unprofitable is the labour, and turn to more wholesome streams. It may be so. But who does not know how very imperfect the preliminary education is in the case of a great proportion of our students of medicine—how poorly prepared they are for thinking correctly on abstruse subjects—and how apt to be caught by specious novelties (which, indeed, are *not novel*) when announced from the professorial chair with boldness, and with strong expressions of contempt for the opinions of the multitude. On this account it is that I see no harm, but much good, in an occasional "studied display" in favour of fundamental, irrefragable truths, even at the hazard of a charge of "hypocrisy" and "malice." At parting, I will remind this eminent physician that though nothing is so bad as hypocrisy, there are other vices of the mind productive to society of more extensive injury: one is the vice of an *insatiable egotism*, particularly when associated with an eager curiosity, a bustling activity, and slender reasoning powers.

Yours respectfully,

JOHN ROBERTON.

Manchester, Nov. 28, 1837.

I beg to observe that I will not be drawn into a controversy respecting the contents of this letter. My maturely-

weighed sentiments on life and mind are to be found in the little work before referred to, "Critical Remarks on certain recently published Opinions on Life and Mind," 12mo. Longman and Co. 1836. When the arguments in that work have been fairly answered, it will then be for me to consider how far a reply may be necessary.

SOME ACCOUNT OF MESMERISM.

BY BARON DU POTET DE SENNEVOY.

[Concluded from page 423.]

On Somnambulism.

It was at Busancy, near Poissons, that somnambulism originally presented itself with all its wonders. The Marquis de Puysegur was the first who thought of interrogating patients thrown by magnetisation into a kind of sleep. They replied without awaking, and the report of this astonishing circumstance soon spread abroad. People hastened from all parts to witness so singular a phenomenon.

The first account of these facts appeared in 1784: it was by a M. Cloquet, receiver of finance. "Attracted, like others, to this spectacle, I went prepared to be a calm and impartial observer, firmly resolved to be on my guard against the illusions of novelty and of wonder; firmly resolved both to look and to listen well."

M. Cloquet, after having described the proceedings employed by M. de Puysegur for acting upon the patients, and having related various scenes of magnetisation, adds—"The consummation of this state (the magnetic state) is an appearance of sleep, during which the physical faculties seem to be suspended, but to the advantage of the intellectual faculties. The eyes of the subject are closed: his sense of hearing is null; he wakes only at the sight of the master (*dumaitre*). No one should touch the patient during a crisis, not even the chair on which he is seated; it would cause him much suffering and convulsions, which the master only can calm.

"These patients, during the crisis, possess an extraordinary (*surnaturel*) power, by which, on touching a patient presented to them, on passing their hand even over the clothes, they feel

which is the affected viscus—the suffering part; they point it out, and indicate pretty nearly the suitable remedies.

"One singularity not less remarkable than all that I have just disclosed is, that these sleepers, who during four hours have touched patients, have reasoned with them, remember nothing, absolutely nothing, after the master has thought proper to disenchant them—to restore them to their natural state. The time that has elapsed between their entering into the crisis and that of their coming out of it, is as it were null. The master has the power, not only, as I have already said, of making himself heard by these somnambulists, but I have frequently seen him, observing him all the time with the utmost vigilance—I have seen him point his finger from a distance to individuals while in the crisis, and in a state of spasmodic sleep, and make them follow him wherever he chose; or send them from him, either to their own homes, or to different places which he designed, without telling them,—the somnambulists, it should be remembered, having their eyes the whole time completely closed. I have forgotten to mention, that the intelligence of these patients is singularly susceptible. If, at distances by no means inconsiderable, conversation is held offensive to propriety, they hear it as it were internally, their minds are disagreeably affected by it, they complain of it, and inform the master of the circumstance; and this has several times occasioned scenes of confusion to ill-witted jesters (*pour les mauvais plaisants*), who indulged, at M. de Puysegur's residence, in inconsiderate and misplaced sarcasms. In order to awaken the somnambulists, the master has only to pass his fingers over their eyes."

M. de Puysegur, whose philanthropy has led him to occupy himself with magnetism, hastened to impart his discovery to persons who, like himself, took interest in this subject. Wherever somnambulism could be produced, the admiration inspired by this strange phenomenon was so great, that those who presented it were consulted as oracles, and the enthusiasm of those who witnessed such scenes knew no limits.

By degrees, however, people became familiarised with this state, and reflection caused it to be better appreciated. Yet after forty years this discovery has lost nothing of its importance; it has

been combated; efforts have been made to prove that it does not exist. Negative facts, it is true, have been brought forward, which have caused its existence to be doubted.

Passing by many historians who have spoken of somnambulism, we reach our own epoch, and shall cite living authors, who may be consulted respecting the merit of their own assertions. The first summary is by the Comte de Redern, a distinguished man of science, who gave great attention to magnetism.

"The body is more erect than in the waking state; there is a marked acceleration of the pulse, and an augmentation of irritability in the nervous system; the touch, taste, and smell, have become more delicate; the sense of hearing is affected only by sounds proceeding from the bodies with which the somnambulist happens to be in direct or indirect relation, that is to say, which are in communication through the vital fluid, from having been touched by him and his magnetiser. His eyes are closed, and have no longer the power of vision; but he has a kind of sight which may be called internal, that of the organisation of his own body, of that of his magnetiser, and of the persons with whom he is placed in relation; he perceives the different parts of them, but in succession only, and according as he directs his attention to them; he distinguishes their structure, form, and colour: he has sometimes the faculty of perceiving external objects by a peculiar kind of sight: they appear to him more luminous, more brilliant, than in the waking state. He experiences a painful reaction of the sufferings of the persons with whom he is in relation; he perceives their diseases, foresees their crises, has a perception of the suitable remedies, and not unfrequently that of the medicinal properties of substances presented to him. His imagination has a tendency to exaltation; he is jealous, full of vanity and self-love, and disposed to make use of little artifices to give himself importance. His will is not inactive, but it is easily influenced by the magnetiser. Very striking contradictions are observable between his ordinary opinions and those in his state of somnambulism; he condemns his own actions, and sometimes speaks of himself as he would of a third person quite a stranger to him. He expresses himself better, has more

esprit, has greater powers of combinations, possesses more reason, more morality, than in his waking condition, all the ideas of which are present to him. When the somnambulist returns to the waking state, he has entirely forgotten all that he had said, done, and heard, during the fit of somnambulism, &c."

Let us now attend to further details on the faculties attributed to somnambulists.

"When magnetism produces somnambulism (says M. Husson,) the individual in that state acquires a prodigious extension of the power of sensation. Several of his exterior organs are, so to speak, put to sleep, especially those of sight and hearing, and all the operations dependent on them are performed internally.

"The somnambulist has his eyes closed; he neither sees with his eyes nor hears with his ears: yet he sees and hears better than a waking person. He sees and hears only those with whom he is in relation. He sees only that at which he looks; and he usually looks at those objects only to which his attention is directed. He is submissive to the will of his magnetiser in all things which cannot injure himself, and in all that does not oppose his own ideas of justice and truth. He feels the will of his magnetiser. He sees, or rather he has a perception of the interior of his own body, and of that of others; but he usually remarks those parts only which are not in the natural state, and which disturb the harmony of it. He recalls to his memory things which he had forgotten in his waking state. He has previsions and *présentations*, which may be erroneous in several circumstances, and which are limited in their extent. He expresses himself with surprising facility. He is not free from vanity; his self-improvement is progressive (*il se perfectionne de lui-même*) for a certain time if guided with discretion, but if ill directed he goes astray. When returned to his natural condition he entirely loses the recollection of all the sensations and ideas he had during his state of somnambulism, so that these two states are as entirely strangers to one another, as if the somnambulist and the waking man were two different persons."

M. Husson adds, "Recent observers affirm, that in this state of somnam-

bulism, of which we have just analytically exposed the principal phenomena, the magnetised persons have a lucidity which gives them positive ideas on the nature of their diseases, on the manner in which persons put in relation with them are affected, and of the mode of treatment to be employed in both cases. If it is assuredly true, as it is asserted to have been observed in 1820, at the Hôtel Dieu, at Paris, that during this singular phenomenon sensibility is so entirely deadened that the somnambulists could be cauterised; if it is equally true, as it is affirmed was seen at La Salpêtrière, in 1821, that the somnambulists possess such a degree of prevision, that women, well known, and treated for a length of time for epilepsy, were able to predict, twenty days previously, the day, the hour, the minute, when the next epileptic attack would come on, and that in fact it did so come on; finally, if it is equally acknowledged that this singular faculty can be employed with advantage in medical practice, there can be no manner of doubt that, in this point of view alone, it deserves the attention and the examination of the Academy."

When this summary was read at the Academy of Medicine, M. Husson was its president. If what he announced to exist had not presented itself to his eyes with all the characters of truth, would he not have shrunk from such language, and so far from soliciting the Academy to make the proposed examination, would he not have designated magnetisers as cheats and impostors? But as we have said, mesmerism was no longer in the hands of a few physicians only, and some scientific men. Many amongst them practised it, and to give even an abridgment of all their labours would require several volumes. To the summary which I have just transcribed I could have added those of M. Deleuze upon the same faculties, of M. Chardel on the same subject, and of Dr. Foissac, the physician, who excited the Academy to the examination, and went to great lengths in his assertions. I could tell that which I have seen developed in more than 300 somniloquists, and quote numerous foreign authors who have written on magnetism. But to what purpose should I prolong the recital, since such phenomena may be produced daily? Once more, magnetisers do not ask that persons should

believe in the reality of their statements, but only desire that they should examine fairly. An examination is, indeed, brought against them, in which all the facts are negative. Can four or five men, who have studied, for no great length of time, two magnetised subjects, prevail against thousands of observations and observers? Prove to us, say they, the wonderful phenomena of lucidity, and we will believe in them; explain to us how the eye and the ear can be supplied by other organs, and we will become your partisans. I desire nothing better than to endeavour to resolve this problem; for however objectionable it may be to speak of seeing without the aid of the eyes, I firmly adopt, in its full extent, the fact it is intended to express. As to the law according to which it is produced, I have not yet arrived at a full and positive conclusion.

I shall enter upon the phenomenon of sight (so called) without the intervention of the eyes. My examples will be numerous, and all authentic. As to the explanation of the facts, if it should appear incomplete, I will endeavour to satisfy, as far as may be in my power, the doubts proposed to me. Before I conclude, allow me to state my regret that, on the first discovery of animal magnetism, words were not created well calculated to express the effects arising from the proceedings employed by Mesmer; for the state of which I have just given a description cannot be rendered intelligible by the word sleep. The vision of somnambulists is not exercised by their eyes, it is another mode of perception, and which cannot be rendered by the word *seeing*. The action also of one individual upon another is no more magnetic than electric, and thence sprang innumerable errors that have furnished a text for many disputes, which would have been avoided, had the singular phenomena arising from the employment of a new agent received more distinctive names. But the mischief is done; it would now be very difficult to make use of other terms than those which the magnetisers have adopted. For the rest, the discovery is made; that is the essential point; it will not be lost; time will mature it, and like the rivulet, which does not become limpid till after it has flowed through the sand, and there deposited its sedi-

ment, so magnetism will make its way through criticism and contradiction; and thus purified, will, in time, appear in all the brilliancy of which so noble a discovery is susceptible.

Philanthropy, the desire of being useful to a suffering fellow-being, have no doubt caused the power of magnetism to be exaggerated; but this power does exist; it is indubitable. It is the part of the physician to study it without prejudice; it is for the physiologist to assign to it its just limits. The direct influence of this new agent upon the nervous system would lead at once to the belief that its action would be efficacious in nervous diseases generally; that hysteria, hypochondriasis, melancholia, catalepsy, epilepsy, might derive, as they have in reality derived, the most salutary influences from it. Spasmodic affections in general, a multitude of pains, some species of deafness, and paralysis, perhaps would experience some kind of modification from magnetism. In these various affections, the nervous system being principally injured, and magnetism especially influencing this system, it is easy to conceive that results might be obtained worthy of attention. Thus it is amongst these diseases that the partisans of magnetism affirm that they have had the most surprising success. It would render this article much too long were we to quote examples, but all works upon magnetism are full of them. No panacea exists, nor do we pretend that magnetism is one. Thus, if it is useful in some circumstances, there is reason to fear that it may be injurious in others.

The nature of its action should be carefully studied, to ascertain whether it is exciting, debilitating, or sedative. If we can rigorously determine this physiological action, we may then proceed philosophically; we may fix the cases in which it may be employed with advantage—we may become useful; at least, we shall cease to do mischief. But will the power of magnetism be limited to the diseases of the nervous system? We know that the brain extends its empire over all our organs, over all the parts of our bodies. This governing organ being by this means profoundly modified, may it not in its turn operate some beneficial change in a suffering part? By suspending pain, will not a first benefit be produced? Pain being

suspended, will not the afflux (*Puppel*) of the fluids, which is its consequence, be also suspended? will not the materials of congestion, irritation, engorgement, which these fluids bring with them, and which augment the local evil because the effect augments the cause, then cease to accumulate (*d'arriver*); and would not the ulterior progress of the disease be in this manner obstructed, and its resolution facilitated? Let us suppose only pain to be suspended, and this effect is incontestable, and we already see that the results are immense: what would be the case if physiological experiments should prove incontrovertibly that magnetism renders absorption more active?

Thus, in acute diseases, magnetism may produce beneficial effects. I ought, however, to call your attention to the circumstance, that if in the healthy state nature alone knows her own weights and measures, and if the harmonious course prescribed by infinite wisdom presents itself to our observation, the case is not the same in some acute states, where nature in a disturbed state seems to abandon the individual to chance. It would then be dangerous for you to rely entirely upon magnetism; you should join with it some active medicine, capable of arresting or of diminishing the existing disturbance; for it is only then that the materials of repair which you employ can be used with advantage by the conservative powers of nature; in the contrary case they may rather increase the evil. What I have just said is not applicable to syncopal diseases, diseases that can only be compared to death. It is on such especially that we may expect beneficial effects from magnetism. These diseases consist in a sudden weakening, in a greater or less degree, of the vital action, and are most commonly caused by some moral affection, such as joy, fear, surprise, grief, the antipathy of the senses, and above all of the imagination, towards certain objects.

In such diseases some unknown organs still retain sensibility; they serve as a last entrenchment for life. It is in this retreat that the magnetic fluid would probably penetrate to re-animate nature, and supply the stimulus required to awaken it, with more certainty than any of the known agents.

Zealous for the good of mankind, we have undertaken the defence of magnetism, because we are convinced that no

discovery could produce such important results; and this not only because it is a means of curing some of the diseases which afflict our race, but because from the knowledge of this agent flow new principles, which will infallibly emancipate us from a host of ancient errors, which have no other foundation than antiquity, and ignorance of some laws of nature.

[We have found it impossible to complete the article in the present number, but shall insert what remains of it next week.—ED. GAZ.]

MEDICAL GAZETTE.

Saturday, December 16, 1837.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."
CICERO.

ON THE PRACTICAL TEACHING OF MEDICINE.

HAVING lately read with great satisfaction Mr. Whewell's work "On the Principles of English University Education," it will not, we hope, be out of place, if we lay before our readers some of the very valuable matter which it contains, using a few of the author's more important announcements in the manner of texts, which we shall illustrate by remarks that may be useful in regard to the subject of medical education. From a gentleman of talent so extraordinary, and of experience so extensive, as Mr. Whewell, any recommendation, indeed, on the subject of education, cannot but be deemed most valuable, not only in regard to those members of our profession who are more immediately concerned in University education, but to the much greater number, to whom, educated elsewhere, the same general principles to which he alludes will apply.

In the first part of his work Mr. W. points out the essential difference be-

tween the two chief modes of teaching at present pursued, in one of which a lecturer imparts to his audience the doctrines of some branch of study, the pupils being merely listeners, and never required to take any active part, or to apply, in the presence of their teacher, the knowledge thus acquired; while in the other, the pupils not merely listen, but do something themselves; they solve problems enunciated to them, while in classics they translate or compose. The former may be called *speculative*, the latter *practical*, teaching. The first is the mode generally adopted in the Colleges; the last, that pursued by the University professors.

Having pointed out the classes of subjects to which each of these systems is particularly applicable, he considers the general effect which each, when widely adopted, has had, and briefly but lucidly shews, that practical teaching (especially as pursued in mathematics) is very far to be preferred, in the discipline which it exercises over the intellect, and the healthful condition of the mind which it induces, by storing it with distinct fundamental ideas. From historical evidence it is proved that it has had the most marked and beneficial influence on the progress of civilization; that so long as practical teaching was adopted, the degree of civilization increased, and the progress of science and art was rapid; but that when speculative teaching was pursued, a result the very reverse followed. The nature of the subject taught of course corresponded with the mode of tuition—subjects of the class of mathematics and inductive philosophy coinciding with the practical, and those of more abstract philosophy with the speculative mode. The practical and mathematical education prevailed in the time of Plato; and at that period the greatest scientific discoveries of the

ancient world were made by Hipparchus and Archimedes. Soon after, the speculative study of philosophy prevailed, and from that time science was almost at a stand-still; it had even fallen below the height it had attained, and the intellectual world grew darker and darker, till the religious orders introduced a system of practical and mathematical instruction among themselves, and then the most rapid progress again commenced; and where the same system has been continued, the most exalted state of civilization, and the highest attainments of science, are, in the present day, reached. In one country, Germany, where the speculative mode of teaching *philosophy* has been resumed, they have been forming system after system, without seeming to be aware that in many sciences their professors are a century behind those of the more advanced parts of Europe.

The "great experiment" of history and accurate observation, have, in short, clearly shown the benefits of practical teaching, on the progress of science and civilization, and on the moral and intellectual condition of the taught: let us see how far this fact can be made useful in that branch of education with which we are most immediately concerned.

Mr. Whewell could nowhere have found a more marked individual instance of the truth of that which he proves generally, than is to be seen in the history of the medical sciences. Look to what period of it we may, we find those leaving the most valuable results of their labours, who cultivated it most completely as a science of accurate observation, and least as one of mere philosophy; and in succeeding years we find the impress which their teaching made on the minds of their pupils commemorated in the increased rapidity and progress of the science. It is but a short time since we made this remark,

when speaking of the state of medicine in England, France, and Germany, and we may here point to the striking difference of repute in which men, probably of nearly equal talent, are now deservedly held, according as they pursued their studies in one or other of these directions. Take, for example, Boerhaave, Stahl, and Van Helmont, on the philosophical side, and Harvey, Sydenham, and Haller on the practical, and it will be seen that while the facts elicited by the latter are now the most familiar, most clear and fundamental, of the science, the systems of the former are never mentioned except by those who now most unnecessarily take the trouble to refute them. Or to come to later times, compare Cullen with John Hunter: the system of the former long since superaded, while the facts of the latter become every day more valuable as they are more completely illustrated. The school of Cullen, even now, is scarcely recognised, and few would choose to acknowledge themselves supporters of his opinions, or followers of his mode of study; while, though hardly any of the actual pupils of Hunter are now left, he is still the model which every student is urged to imitate, and his reputation daily increases. It is a happy thing that at present we can scarcely be said to have a system of medicine in vogue, and seeing how futile have been the attempts hitherto made, and how signally men of very exalted genius have failed, it is to be hoped that none will again lose their time and labour in framing one;—centuries must still elapse, before sufficient distinct and fundamental facts can be accumulated to form any basis stable enough to bear so complex and so lofty an elevation as that must be which will include all the laws of such varied actions as medical science has to deal with.

For the sake of the science, therefore, practical study and teaching should by

all means be pursued, and the same spirit should prevail in the education of all, whether they are destined in after-life to be merely practitioners, or independent and scientific students of their profession. There can be little doubt that if the system of practical instruction could be introduced into medical schools, its tendency would be highly beneficial; though, of course, it would only be applicable to a part of the subjects usually taught there. Medical and surgical lectures must always remain as they are, consisting of enunciations by the teacher of the doctrines of his science according to the most received opinions of his day, and of such illustrations as he may see fit to employ. But such need not be the mode of teaching anatomy, or at least of all parts of it; in it, consisting almost entirely, as it does, of descriptions of visible forms and arrangements, which it is important for the student to remember well, no plan could be more likely to secure accurate and distinct ideas in his mind, than that he should be made to demonstrate his knowledge with the subject before him to his teacher, and to the class around. In some schools this is occasionally resorted to, in weekly examinations; but the demonstrated subjects are very limited, and little more is done than in questions and answers, in imitation of the ultimate examinations for which a competent preparation is at present the main and often the sole end of education. The system is far more generally pursued by the private teachers; but by them the subjects taught are carefully and expressly limited to those *required* by the examiners, and even these are taught in so superficial and hasty a manner, that they obtain not the slightest hold on the student's mind; and he deems that he has received all he paid for, if he has seemed to know on the necessary evening that which he ought to know in every hour of his life.

The course of anatomical demonstrations, which is of comparatively recent introduction, has of late become (whatever its origin might have been) nothing less than a second or supplementary course of anatomical lectures, in which the subjects are not always such as admit of visible demonstration, and are treated of in accidental arrangement. It is altogether a matter of speculation whether the student, who "has attended diligently" both of these courses for a lengthened period, has profited by them in the smallest degree. We know that instances continually occur where pupils who have been present at three or four hundred such lectures, have remained utterly unable to tell one bone from another when they were shewn them together; and it is not long since a candidate, who passed at the College of Surgeons, repeated accurately a demonstration of the scapula, but when asked to point out the superior costa, put his finger on the base. Now, in a study which may and ought to be practical, and which, except for its practical applications, is equally useless and uninteresting, these things ought never to occur, and the idleness of such students as would be content with their ignorance, should be met by more vigilant measures.

An improvement would undoubtedly be effected by making both the studies and the examinations more practical. We are not anxious to advocate any fundamental change in the system of the schools, for we look with satisfaction on the improvements which late years have seen rapidly introduced into them; but we think it well worthy the attention of teachers to consider whether the present subordinate course of lectures might not be made a *bonâ fide* course of practical anatomy. Each student might be called on in turn to dissect and demonstrate some muscle, or other part, while others stood

around; or it might be made the general practice in the dissecting-rooms, for the teachers not merely to direct the dissections, and assist in removing the difficulties of the students, but to call on them to demonstrate the parts in the study of which they were at the time engaged. No one who has for any time observed the degrees of knowledge of practical anatomy which students obtain during their attendance on lectures and dissecting, can be ignorant that the amount which they possess is in general utterly disproportioned to the time expended on it; and we believe that this depends, in great measure, on the want of more practical teaching.

Not long since, we pointed out the comparative inutility of chemistry and botany, as at present taught. The knowledge which students are expected to obtain of them is even more speculative than of anatomy; for they are never called upon to put any part of what they learn into practice, or to take any active part in the study of these branches of education, as they do in that of anatomy by dissecting. Much of the theoretical part of both these subjects might well be dispensed with, if the merest elements of their practice could be taught.

A practical study of the practice of medicine and surgery it would be difficult to enforce in our hospitals, without infringing in some measure upon the comforts and safety of the patients; but it would not be difficult to make the examinations more practical. We do not, indeed, despair of seeing the time when part of the examination shall be conducted at the bed-side of a patient, as it now is in France; where, indeed, it forms the only part in which their educational system is at all superior to our own. But in reference to the subject of examinations, there are some remarks in Mr. Whewell's work which deserve a separate consideration.

We need scarcely add, that a system

of practical teaching would remove the objection, that as students may attend in lecture-rooms without attending to what is said, the attendance might be given up altogether. Not that this objection is of any great force, for it would be difficult to prove that, if excused from lectures, the student would be better employed—as for example, in reading the works of better teachers, which, it is said, are open to him. We suspect the instances are very few where the inattentive at lectures are attentive to private study; and though attention cannot be enforced where attendance is, the student has at least some chance of learning afforded him: but in practical teaching, attendance and attention must be coincident, and we can scarcely see how the most ingeniously idle could evade the acquisition of knowledge.

MR. SAMUEL COOPER AND
MR WAKLEY.

Our readers will have perceived, from the correspondence inserted in our last number, that Mr. Wakley sent a friend to Mr. Samuel Cooper, to demand an explanation in consequence of certain expressions contained in his lecture, published the preceding week. Now the lamentable want of discretion and good taste displayed on this occasion, by one enjoying the reputation, and holding the prominent station of Mr. Cooper, is too mortifying a subject for us to discuss; but we cannot let the affair drop, after having assumed its present aspect, without adverting to the very unreasonable and absurd line of conduct adopted by Mr. Wakley.

It is matter of notoriety, universally felt, acknowledged, and acted upon, in the medical profession, that the Editor of the *Lancet*, by the indiscriminate abusive and often libellous nature of his attacks upon individuals, has earned for his journal such a place in public estima-

ion as to preclude every one, without a single exception, of the many whom he has assailed, from calling him to any account, unless that which is yielded in a court of law. This is the simple and undeniable statement of a well-known fact. But the very circumstances which have obtained for the Editor of the *Lancet* this peculiar species of immunity, have, as a necessary and obvious consequence, deprived him of all right to expect that any of those who, in self-defence, answer his attacks, should consent to afford him "satisfaction," merely because such retaliation may have proved unpalatable to him.

The only difference between the present and most other cases, is, that Mr. Cooper, on being attacked, has chosen to reply, and that, in our humble judgment, not in a manner such as his friends could have desired. Nevertheless, if it be admitted that consistency, and a regard for what he owes to himself and his professional brethren, did not peremptorily require that Mr. Cooper should decline the challenge, then would the Editor of the *Lancet* be placed in an entirely new position, and such as would render it imperative on all those whom he has calumniated to call him to account forthwith; in which case, we apprehend that the life of the honourable member for Finsbury would be held but an indifferent speculation by an insurance company, even in these days of hungry competition.

Since the above was written, Mr. Cooper has sent us another address to his pupils, which we have not, under all the circumstances, felt at liberty to withhold. It appears that he did not regard the message from Mr. Wakley as a demand of satisfaction, and implies that if he were to receive a challenge from him, he would accept it! While we have read these statements with unfeigned astonishment, they have only

served to impress us more strongly with the correctness of the view of the question we had previously taken, and which remains altogether unchanged.

P.S.—As this sheet was passing the press a letter was received, which will be found at p. 478, and to which we refer our readers, without any other observation than that they will find the termination of this affair by no means the least extraordinary, or least amusing, part of the whole.

CLINICAL LECTURE

ON

ENCYSTED TUMORS OF THE EYELIDS—ON PENETRATING WOUNDS OF THE EYE—ON SYPHILITIC IRITIS—AND STRUMOUS OPHTHALMIA.

By W. LAWRENCE, F.R.S.

Encysted Tumors of the Eyelids, containing Hair.

A FEW days ago I removed a small tumor, in the case of a young child, from the neighbourhood of the external canthus. It was about the size of a horse-bean, forming a colourless elevation, with the integuments covering it loosely, so that they could be pinched up into a fold over the swelling, which was more fixed below. It was placed immediately behind, and a little above the junction of the lids; and was said, by the mother, to have been there from the time of birth. She stated that it was increasing, and for this reason she wished that it should be removed.

In performing the operation, it was found that the tumor, which from external examination might have been supposed to be simply subcutaneous, was covered also by the orbicularis palpebrarum, and that it adhered closely to the external angular process of the frontal bone. It consisted of a thin but compact cyst, with a whitish glistening surface, and it contained a fatty matter, resembling soft suet, with an intermixture of short darkish hairs, principally in small bundles on the surface of the fat.

Such tumors are not unfrequent in infants and young children, occupying the situation just described. I believe them to be congenital; at least the statement of the mother generally leads to this inference. Sometimes they are stationary:

in that case, if the swelling is small, there is no necessity for operation, as it is perfectly indolent. I am acquainted with a gentleman who has had through life a rather larger growth of this kind: it causes an unnatural fulness near the extreme angle of the eye, not amounting to deformity, and has never been attended with the slightest uneasiness. The affection, as in the case just described, is a cyst containing fat, which is sometimes of oily consistence, sometimes firmer. I have always found short hairs mixed with it in various proportions. Is this admixture of hairs, which resemble those of the eyebrow in length, to be regarded as an exemplification of the principle so frequently observed in adventitious structures, viz. that they resemble in nature that of the textures in which they grow, or that of parts in their immediate vicinity? It is placed under the orbicularis, and adheres more or less firmly to the bone. You must bear in mind the two latter circumstances in operating; make a larger incision than the size of the swelling would seem to require; and pay especial attention to the complete removal of the cyst from the bone.

If a portion of the cyst is left, the wound will not close, and such an occurrence is very annoying to the patient, and considered discreditable to the operator. I saw a young lady, in whom such a tumor as those I have described had been removed from the root of the nose, at the interval between the two eyebrows. She was a handsome person, and had submitted to the operation for the removal of what she deemed a blemish, though it must have been very slight, as the tumor was inconsiderable. She was much worse off after the operation than before; for the wound did not heal, at least it sometimes scabbed over, and sometimes discharged. A probe introduced into the opening went down apparently to the bone. Having learnt the nature of the swelling, and that the operator had experienced unexpected difficulty in separating it from the bone, I concluded that a bit of the cyst had been left behind, and proposed an incision to ascertain that point, which was readily consented to. I found, closely adhering to the frontal bone, a small strip of the cyst, conspicuous by its white glistening surface, and having a few short hairs on it: this was easily removed, and a firm cicatrix was soon secured.

I once saw a small growth of similar nature, but with an external aperture large enough to admit an ordinary dressing probe, on the bridge of the nose. It was congenital; and the opening sometimes

produced a kind of greasy discharge. A probe passed in about a third of an inch. Various applications had been made to the part ineffectually. I slit it open, and found a smooth shining membrane with small hairs upon it, almost imbedded in the ossa nasi. It was difficult to dissect it out completely; but the removal was accomplished, and the part healed soundly.

Penetrating Wound of the Sclerotica, with Prolapsus (?) of the Iris.

A child, about five years old, was brought to the hospital, as an out-patient, a week ago, having received a severe blow on the right eye with a stick. The sclerotica was ruptured near the margin of the cornea, the wound being about one-eighth of an inch in length. The corresponding portion of the iris had disappeared, so that the pupil, opposite to the wound in the sclerotica, was continued to the circumference of the cornea. The conjunctiva was slightly elevated, as if by a protrusion of the missing part of the iris. The accident had occurred three or four days previously, and nothing had been done for it. There was no inflammation or pain of the eye: the pupil was clear, the iris natural, except in the points already mentioned, and vision seemed unaffected. The case required nothing further than care, with a little medicine and cold lotion. We saw the child since, the eye remaining in the state already described, and ascertained clearly that vision was unimpaired.

Penetrating wounds of the globe seldom terminate so favourably as in this case. They generally produce, more especially if they should be lacerated, as in this patient, serious inflammation, which involves the internal tunics, and is thus likely to destroy or injure sight. To prevent or to lessen this kind of mischief, active antiphlogistic means should be resorted to immediately after the accident, or as soon as the slightest indications of inflammation are observed. Again, a blow on the eye, without producing any wound or visible injury, may impair or destroy sight, by causing concussion of the retina. I use the word concussion here, as in injuries of the head, to denote a species of injury not produced by violence directly applied to the part, and of which indeed we do not know the exact nature, although it is capable of producing the most serious effects. I lately saw a young gentleman, in whom complete amaurosis had ensued from a blow in the eye, not followed by any visible injury.

When the sclerotica is wounded, there is commonly an excessive partial dilatation of the pupil opposite to the injury,

which may proceed, as in this case, to actual disappearance of the pupillary margin. This must be ascribed to injury of the corresponding ciliary nerves. Such wounds are frequently attended with effusion of blood into the anterior chamber, behind the iris, or in both situations. This indicates that the violence has been serious; and wound or rupture of the sclerotica, combined with such effusions, generally terminates in loss of vision, either by concussion of the retina, or by inflammation of the internal tunics coming on slowly after the accident, and terminating in atrophy of the globe.

I remember, however, another case of complete recovery after a penetrating wound of the sclerotica, which I mention to you, because such favourable results, although rare, may prevent us from pronouncing too alarming a prognosis. A boy at school, ten years old, received a blow on the eye, near the external angle, from an arrow, with a pointed nail fixed to its end: it was thrown towards him, and did not strike the globe with great force. He was immediately brought to me in a chaise, from a distance of fifteen miles, where the accident had happened. The master, who accompanied him, informed me that he examined the eye immediately after the accident, that he found the pupil greatly dilated, and that the boy could not see. There was a wound of the sclerotica, a short distance behind the cornea, shaped like a leech-bite, but larger: the pupil was round, clear, and larger than that of the opposite eye. I was pleased to find that the patient could see, though imperfectly; and I was unwilling at that moment to make the trials necessary for ascertaining the exact amount of vision. I directed leeches, cold cloths to the eye, aperient medicines, low diet, and rest in bed, strong light being excluded from the room. I saw the patient again in ten days, when the eye was, as it had been in the interval, free from all inflammatory redness; the iris, the pupil, and the motions of the former, were perfectly natural, and vision was unimpaired. The recovery in this case was permanent; the injury of the sclerotica being still visible. Wounds of this structure do not unite, as we see in the punctures made in couching.

Iritis combined with Scaly Syphilitic Eruption.

I have seen iritis occurring in conjunction with various forms of syphilitic eruption. If asked with which of these it is most frequently conjoined, I should have said the scaly; although Mr. Carmichael, whose experience has been so extensive, has represented it as almost confined to what he denominates the papular form of venereal disease. I mentioned to you in a former lecture a case of acute syphi-

litic iritis, with scaly eruption, and ulceration of the fauces. There was general discoloration of the iris, with a mass of lymph effused near the pupillary margin; contraction and irregularity of the pupil, with vision so much impaired, that the patient could not distinguish even capital letters. The treatment consisted, as I then mentioned to you, in abstraction of blood by cupping on the temple to sixteen ounces, and the administration, every six hours, of two grains of calomel, with one-third of a grain of opium. Subsequently the moistened extract of belladonna has been applied on the brow daily. The mercury has acted considerably on the system, producing pyalism; and the influence on the ophthalmic affection, and the other symptoms, has been proportionably decisive and rapid. In seven days the effused lymph is nearly absorbed, the iris has regained its natural colour, the external redness has nearly disappeared, and the patient can read small print; the adhesions of the iris have not given way, and the organ has not perfectly recovered, although the mercury has been discontinued, and will, probably, not be resumed. The eruption has faded, and the throat is recovered. The antivenereal powers of mercury seem to me to be shown most unequivocally in ulcerations of the throat and mouth; for although the remedy produces, as it did in this case, inflammation and ulceration of the gums, it cures the syphilitic ulceration of the contiguous membrane, thus appearing in the double light of bane and antidote, in reference to two parts of one continuous membranous surface.

W. Pratt was admitted May 25, with gonorrhoea, scaly eruption, superficial ulcerations of the fauces, and pains in the limbs. On account of the latter, which were severe, hyaliodate of potash, in the dose of 5 grains, three times daily, was administered in the compound decoction of sarsaparilla. It acted with its usual good effects; and the patient felt himself so much relieved, that he left the hospital, by his own desire, on June 14. He returned on the 21st with acute iritis of the right eye. There was a zone of pink redness in the sclerotica round the cornea; the iris was discoloured, dull, and dark throughout: the pupillary margin, and the lesser circle, being covered by a fur of dull brownish red lymph. The pupil was irregular, vision greatly impaired, and there was great pain in the frontal region, particularly during the night, completely preventing rest. Twenty ounces of blood were taken from the temples by cupping: two grains of calomel, and $\frac{1}{4}$ grain of opium, were given every six hours. On the 23rd, twelve leeches were applied round the eye: and now (June 26) recovery

is proceeding rapidly, under a decided but not excessive, mercurial influence.

In — Richardson, we have another example of iritis with scaly eruption. There was general discoloration of the iris, without any effusion of lymph in distinct masses—condylomata, as some of the German writers have called them. Although these effusions are seldom seen in the adult; except in syphilitic iritis, they do not always take place; perhaps they are not met with in the majority of cases. The pain in the frontal region was very severe, particularly at night, and we employed for its relief frictions of mercurial ointment and opium on the part, six grains of the former with two of the latter, which seldom fail to relieve this particular symptom. The treatment was the same, in other respects, as in the two preceding cases; and this patient is recovering favourably.

Thus you see that we have in the hospital at the same time three cases of syphilitic iritis accompanying the scaly form of eruption.

Strumous Ophthalmia.

Mr. Lawrence next adverted to two cases of this complaint, which had been some time in the hospital, and were now completely recovered under a mode of treatment which he generally finds successful. The most striking symptom of the complaint, intolerance of light, existed in both to its greatest extent; indeed it might rather be called total inability to bear the light in any shape, for these children lay in bed under the clothes, either with the face resting on the bed, or the hands pressed on the eyes. The face was red and excoriated from the pressure and from the scalding tears, while the physiognomy had the peculiar expression produced by the spasmodic closure of the eyelids, and the combined contraction of all the surrounding muscles, for the purpose of shutting out the light. To get a clear view of the globe was impracticable; but the eyes, as far as they could be examined, presented very little appearance of disorder. In these strumous subjects the occurrence of disease in a new part relieves that which was previously suffering. In imitation of this natural process, the tartar-emetic ointment was rubbed on the nape in both patients, so as to produce and maintain a considerable irritation; the sulphate of quinine was administered internally; the bowels were regulated by small doses of rhubarb; animal diet was allowed, when the appetite indicated the necessity for it; and the suffering organs were occasionally fomented with tepid water. Under this treatment both cases have recovered: the children can now open their eyes and bear the light as well as any body, and we find that no un-

favourable change has occurred in the affected parts. The patients are at the same time improved in general health. In one of them, the pustular eruption caused by the ointment spread over the body, with considerable but temporary constitutional disturbance.

THE BAYONET WOUND CASE.

To the Editor of the Medical Gazette.

Sir,

WHEN a man, who is avowedly a *public* journalist, freely abuses another individual, who happens also to hold a *public* employment, urging that this public employment is fair game, he of all other men should learn to bear with temperance the mortification and exposure to which his own public transactions, and aggression on others in their public capacities, may subject him. With this single remark, I request that you will insert the annexed clinical lecture in your journal. It was delivered this morning in the theatre of University College Hospital, to a great concourse of gentlemen.—I am, sir,

Your obedient servant,

SAMUEL COOPER.

Woburn Place,
Dec. 12, 1837.

GENTLEMEN,—Ever since I have acted as one of the surgeons of this hospital, I have faithfully endeavoured to elicit from the various diseases and accidents brought into it, all the practical instruction which they seemed capable of affording. I have always communicated to you such information in the most sincere and unreserved manner, disdaining to conceal, as some other parties in the profession do, any mistake which may have been committed, or any unfortunate result; and, indeed, rather preferring to give clinical lectures on unsuccessful and difficult cases, than on others. In this respect, perhaps, you may notice a difference between me and others; but I incline to this course on principle, believing that it is at least as important to trace and study the impediments to cure, as it is to dwell upon cases in which there are no remarkable obstacles to success. In this spirit, I began my clinical lectures this session with observations on three cases which ended fatally, perhaps not, however, through any egregious blunder on my part. But let me inform the writer of a well-known disgraceful journal, that, if such mistake had really happened, I should only be upon a par in this respect with some of his *infallible* continental surgeons, who have actually been elected by *concours*.

Had I the disposition, or (to use a term employed by himself) the *instinct* of this dealer in abuse, I might amuse the scandal-loving part of his readers with an exaggerated history of some of the occurrences referred to; but this task I willingly leave for him to perform, whose *instinct* would, indeed, richly qualify him for the business, were it not for the sake of the concourse itself, that little pet, which he loves, I suspect, much more cordially, than he will soon have reason to do some of his greater ones.

One of the unfortunate cases, forming the subject of my first clinical lecture this session, was that of the man Chaplin, who died of a bayonet wound, and who remained under the care of one of the house-surgeons, a regularly qualified resident surgeon, during the night of his admission, for this plain and simple reason, that I received no information that there was any immediate occasion for my interference.

In the lecture on Punctured Wounds, delivered in the surgical course over the way, I took the opportunity, the other night, of exposing some misrepresentations of the particulars of this case, as resorted to by the editor of the above mentioned inglorious publication, and resorted to, not, perhaps, exactly for the gratification of his own *instinct*, but, as is suspected, in the service of a party behind the curtain, who possibly may make his *debut* in due time.

Gentlemen—Amongst other topics in that lecture, I fearlessly declared my views of the base character and objects of the *Lancet*, and commented upon the gross ignorance of pathology and practical surgery displayed by its editor. If any gentlemen now present were absent from that lecture, I earnestly recommend them to read the sketch of it, as given in the MEDICAL GAZETTE of Saturday, the 2nd of December. It constitutes, indeed, an illuminated transparency of the bold and apparently wilful misrepresentations, for which the *Lancet* is more renowned than for any thing else that I know of, save and excepting the profound ignorance always manifested in it of practical surgery.

Notwithstanding the bayonet kind of wound, which such public exposure would be sure to inflict on any defamatory writer furnished with nerves as well as a brain, I did not venture so far as to hope that the editor of the aforesaid unprincipled work, who, in that capacity, has been for years habituated to greater exposures than this recent one, would be sensible of the lesion. In fact, my strictures were intended rather for the benefit of my pupils and others, than offered with the expectation of producing any impression on the editor of the *Lancet*, nice as he is about his *instincts*.

It seems, however, that I was wrong in my calculation; for on Monday, Dec. 4th, a stranger, calling himself a friend of Mr. Wakley's, and mounted on horseback, accosted me in the street. My first observation was, that I did not wish to hold any kind of communication with such a person as Mr. Wakley, direct or indirect. A short conversation, however, took place. The stranger said, that if I would allow him he could explain to me that I had attacked that individual's private character. "Perhaps," he added, "*you have spoken of him only in his public capacity, as editor of the Lancet.*" "Certainly," I replied, "and with respect to Mr. Wakley's private character, as I know nothing about it, I leave the world to form its own judgment of it. If Mr. Wakley fancies that his private character has been attacked, I recommend him to try the point in a court of justice." "Oh, very well!" rejoined my equestrian visitor, and off he galloped.

It is not true, therefore, as insinuated by the latter, in a note in the MEDICAL GAZETTE, that I ever received a challenge from Mr. Wakley, which I had declined to accept. What I should do were such challenge to arrive, remains to be seen; and my class already know what I would do. Indeed, supposing the editor of the notorious journal should venture to propose to me *gunpowder*, instead of *law or pepper*, which he has since been employing so freely, as the means of settling differences, there would be one vast temptation influencing me to accept the invitation, namely, that possibly the experiment which he may be thinking of making with our bodies, might really give the profession an opportunity of ascertaining most unequivocally, whether an extravasation of three or four pints of blood in the abdomen, and a wound of the intestinal canal, in two or three places, would produce pain in the epigastrium, and a disturbance of the stomach. All the doubt, still hanging over this knotty question might thus be removed; and here I speak more especially of the doubt, in relation to this particular individual, my aggressor, whose stomach is supposed to be much less delicate than that of any common mortal. By this time, I flatter myself that he has cooled, and even got rid of most of that harmless kind of ammunition with which he has chosen to pepper me, in return for which, however, I make no apology for treating him with a little mustard and goose.

Gentlemen, I had truly a great desire to invite him to hear this lecture, with a full guarantee of all becoming respect for his person during his attendance amongst us. I was anxious to pay him this compliment, because it is my inten-

tion this morning to explain to you a little more particularly, and on *higher authority than my own*, the grounds which I have for declaring the editor of the disgusting journal, which soils every body that touches it, to be only a *shallow pretender to medical knowledge*.

I pass over the question about the actual quantity of blood effused in the abdominal cavity of James Chaplin, merely remarking again, that *the true quantity was demonstrated to the eyes of at least 200 good and honest men*, some of them learned professors, and that the record of the quantity in the hospital book was made, not by me, an *interested party*, as is hinted with respect to all hospital surgeons, but by others, who had no interest in the statement whatsoever. Ah! Finsbury Thomas, I am sorry for thee; but I cannot pass over the next point so quickly, and depend upon it, that *unless thou canst squeeze thyself out of this part of the scrape*, as ingeniously as thou hast plotted to *squeeze something out of the Whigs*, thou art done for in this affair! Thomas! I have told thee flatly, that *thou art grossly ignorant of practical surgery and pathology*, and I stated in my address some proofs of the fact. *Thou art not, Thomas, even acquainted with the post-mortem appearances, which serve to denote the difference between a fatal inflammation of the bowels, arising from a strangulated hernia, and another inflammation of the same parts, proceeding from a penetrating wound, extracrusation, and other causes!* I rejoice that thou art not my pupil; and believe me when I say, that I would rather meet thee as a coroner, than an instructor in surgery.

This editor still wishes to make his readers suppose that the man did not die of peritonitis, excited by an extravasation of three or four pints of blood in the belly, and lesion of the intestines in two places, but of strangulation of the knuckle of intestine, and not of inflammation at all.

Now, gentlemen, although this empty pretender to surgical knowledge is conceited enough to think, that he will be able to improve me in this branch of science before he has dismissed me, I shall be rather sceptical about it, not only till he has *triumphed in a concours himself!* but obtained better information than he now seems to possess of any part of our noble profession, in which I rejoice to think he is not a real practitioner, but a sheer meddler—a person who, if I remember rightly, solemnly *abjured it several years ago!*

In order that you may perceive the strong light thrown on the case of James Chaplin by the *post-mortem appearances*, I will not repeat my own description of those which result from strangulated intestine, *where this is the cause of death*; but, gentlemen, I will adduce, in confirmation

of the truth of that description, the highest authority on this, as well as on various other important questions relating to surgery.

"The intestine [protruded]," says Sir Astley Cooper, "*is of a chocolate brown, with here and there a black spot, which easily breaks down on being touched with the finger. A coat of coagulable lymph, of the same colour as the intestine, may be peeled from its surface. At the particular part where the intestine is strangulated, it is either ulcerated through, or readily pulled asunder under slight pressure. The inflammation which takes place in strangulated hernia is different from almost every other species. In most other cases it is produced by an unusual quantity of blood sent by the arteries of the part, which become enlarged; but still the blood returns freely to the heart, and the colour of the inflamed part is that of arterial blood.*"

"Whilst, in hernia, the inflammation is caused by a stop being put to the return of blood through the veins, which produces a great accumulation of this fluid, and a change of its colour from the arterial to the venous hue. On dissection three or four convolutions of intestine are found lying across the abdomen, so enormously distended as to exclude the other viscera from view, and agglutinated slightly together by an effusion of adhesive matter. These distended convolutions consist of the portion of intestine immediately above the seat of stricture, and are their state of extreme distension in part to a collection of feculent matter, but chiefly to a secretion of air into the cavity of the intestine. On the contrary, the portion of intestine below the stricture is remarkably contracted in diameter, and free from any appearances of inflammation."

Such are the effects of strangulated intestine, as seen in persons who die of it, according to this description given by the greatest practical surgeon of the age, and by one of the first surgical pathologists of modern times. In James Chaplin, will any spectator of the dissection, *who has a local habitation and a name*, say that any of these characteristic appearances presented themselves? Were the bowels above the protrusion distended with air, or intestinal matter? The distension which may now be seen was produced with the blow-pipe, by Mr. Tayloe, after tying the bowel with the two pieces of string in it. Was the piece of bowel, when disengaged, found altered in the way described? Were the bowels above the protrusion the principal seat of inflammation? Were those below it remarkably contracted? None of these things were exemplified; on the contrary, had not the inflammation the arterial hue, and was it not most violent where the extravasated blood lay?

The coloured casts and preparations on the table, taken from nature, illustrate every point which it is here desirable to explain. But if these were not convincing enough, the animals on the table, a dog and cat, which were purposely destroyed yesterday by strangulating their intestines, would make an impression never to be forgotten. It does not follow, that because a piece of bowel had insinuated itself into the wound at some period after its reduction in the morning, that it had any material influence on the progress of the case. The appearances of the protruded bowel, and of the abdominal contents, tend to prove that the stricture was not of long duration.

Now the practical suggestions which I deduced from a review of this case, and explained in this theatre last month, the Editor of the notorious work had fancied would serve his cause. But remember, Thomas, that one of them, viz. the advice about operating, where the bowel is only suspected, *not known with certainty*, to protrude, is given with the express condition, "*that the case be unaccompanied by other fatal mischief.*" If the patient were dying of other mischief, no surgeon of common sense, always excepting the Editor of the Lancet, would think in a doubtful case, or indeed in any case, of tormenting a dying man by a useless operation. He would no more think of doing this, than of amputating a limb for a diseased joint, when the patient was sinking under disease of the lungs, or other vital organ.

Gentlemen, this miserable editorial judge of surgery, whose censure is rather an honour, and praise at least valueless, compliments me on the "care and precision" with which the treatment and complications of penetrating wounds of the abdomen are explained in my clinical observations; but hints, that the information, which I gave you on these subjects, was "concocted from my Dictionary, just as the Dictionary itself had been concocted from the writings of the *illustrious dead.*" Unfortunately, however, poor Thomas is here out again; for the particular views, to which he alludes, are not in the Dictionary, nor in any other publication. As, after the foregoing explanation, they will no longer suit him, he will now, perhaps, honour them with his reprobation.

As for the *illustrious dead*, I fear, Thomas, that thou hast no chance thyself of getting amongst them. Thy glory, as Editor of the Lancet, will be very transient, and thy only chance of immortality will depend upon thy retaining everlastingly the notoriety which it hath obtained for thee in this sublunary world.—But, gentlemen, to sum up.

1. The reason of my not having seen the patient directly after his admission, is, that *I was never asked to do so.*

2. That the protrusion, or swelling, whatever it might be, was reduced by me in the morning, as soon as I was informed of the existence of a swelling; and this was so much the case, that we were thinking of applying a compress.

3. That the statement about the man not having been visited by any surgeon of the hospital, considering that *I visited him thrice in the course of the twenty four hours* during which he was in the hospital, must be pronounced to be very bold editorial mendacity.

4. That the statement that *only a little blood* was effused in the abdominal cavity, when the quantity estimated, not by me, but many others, was from *three to four pints*, must be referred to the same head.

5. That the assertion that the man died from the effects of the strangulated state of the knuckle of intestine, and not of inflammation, is a proof that the party making it does not possess any acquaintance with surgical pathology.

6. That as the protruded bowel had suffered none of the changes constantly exemplified in cases of death from strangulated hernia, the inference is, that it had not been long strangulated.

7. That an operation for reducing the bowel, when the swelling was found to have returned in the evening, would have been unjustifiable, first, *because the patient was rapidly dying from other fatal mischief*, and *because the nature of the swelling was, in some degree, doubtful.*

8. That the occasional vomiting was no proof of hernia, as *the extravasation of three or four pints of blood, a wound of the bowels in two places, and peritonitis from these causes, would inevitably be attended with this symptom.*

9. That, for reasons explained in this lecture, since no purgatives were given, and the man had one feculent motion in the short time while he lived, no constipation can well be said to have been manifested.

10. After all the abuse lavished upon me, be it observed, that *my assailant has not been able to show, that there was the slightest chance of the man being saved by any thing which was not done.*

11. That from the accidental condition of the protruded piece of bowel, the wounded state of it would not have been visible, if the skin had been divided, and therefore it must certainly have been pushed back into the abdomen, so as to have occasioned the risk of intestinal effusion, in addition to that of the copious extravasation of blood. Would the man have been then in an improved state?

SECOND MESSAGE TO MR. COOPER.

To the Editor of the Medical Gazette.

SIR,

ON finding that two letters had been inserted in your journal of Saturday the 9th, intimating that I had received a challenge from Mr. Wakley, and declined to meet him, I took the earliest opportunity of publicly contradicting this representation in the hearing of about three hundred gentlemen, who attended my surgical lecture on Monday evening, the 11th instant. On Tuesday last, I found the same insinuation repeated in the *Morning Herald*, to the editor of which I transmitted a statement of facts.

In consequence of this, Captain W. Burt, and another gentleman, both on behalf of Mr. Wakley, called upon me yesterday morning, the 14th, when the Captain acknowledged my account, as published in the *Morning Herald* of Thursday last, and repeated, I believe, in nearly the same terms, in the clinical lecture sent to you on Tuesday the 12th instant, to be substantially correct. I then said, that I should adhere to that statement, and that if Mr. Wakley were dissatisfied with it, I was ready to meet him that afternoon. Captain Burt replied, that Mr. Wakley only required me to say, that I had not directed my observations against Mr. W.'s private character. As this had been already done in our first interview, and also in the letter published by me in the *Herald*, I had no difficulty in agreeing to this proposal. Captain Burt, and the other gentleman, then expressed themselves satisfied.

I wish it, then, to be understood, that whatever expressions may have been used by me in defending myself in this affair, are intended to relate solely to Mr. Wakley in his public capacity, as the responsible, but perhaps not the acting, editor of the *Lancet*.

Your obedient servant,
SAMUEL COOPER.

Woburn Place, Russell Square,
Dec. 15, 1837.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Dec. 12, 1837.

THE PRESIDENT IN THE CHAIR.

Abstract of a Report of Twenty-one Cases of Cholera that occurred in the Seamen's Hospital Ship Dreadnought, between the 8th and 28th of October, 1837. By GEO. BUDD, Physician, and GEO. BUSK, Surgeon of the Dreadnought.

The paper commences with a narrative of

the details of six of the cases in question, which prove in the clearest manner the identity of the disease with spasmodic cholera, as it appeared in 1832. To these two cases are subjoined, in consequence of some remarkable circumstances attending them. In one of them the patient was, at the time of attack of cholera, the subject of typhoid fever, and in him the symptoms, though unequivocal, were less violent than in most of the others, affording, perhaps, an example of the simultaneous influence of two poisons. The subject of the sixteenth case had been previously twice attacked with cholera, at Calcutta, in 1831 and 1833, on each of which occasions he had been treated by frictions, brandy, and opium. Of the twenty-one cases that occurred, twelve terminated fatally, the rate of mortality being greatest in the persons first attacked.

The organs of digestion, examined after death, presented appearances which varied as the patients died at a period more or less remote from the attack. This difference was no less observable in the external characters of the intestines, (which, in the rapidly fatal cases, presented a rosy hue, that disappeared in the protracted examples of disease,) than in the morbid appearances within. In the duodenum the solitary glands were very conspicuous, in the greatest abundance near the pylorus, and never extending into the jejunum. The glands of Payer, and those of Brunner, were much developed in all cases, but chiefly so in those cases that proved rapidly fatal. Generally there was increased vascularity of the mucous membrane of the intestines, giving rise in some cases to a purple colour near the termination of the ileum. In three cases, one of which proved fatal in less than seven hours, there were small black specks at the extremity of the villi. In those who survived the attack more than thirty six hours, the contents of the intestines were usually tinged with bile. The mucous membrane of the large intestines was in most cases pale throughout, being observed to be vascular in some of the protracted cases only. The lungs presented no appearances of disease in those cases which proved fatal within thirty-six hours; while of six cases in which the patients lived forty-five hours, four presented appearances of pneumonia: in two of these, who lived ninety-six, and one hundred and thirty-eight hours, parts of both lungs were found hepatized. In all the cases in which evidence of pneumonia was found on dissection, the symptoms had been latent, confirmatory of an observation made by Mr. Jackson, in his Report of Cholera in Paris in 1832. The gall-bladder in every case was distended with bile. The bladder

was empty in those who died in the stage of collapse, but contained a small quantity of urine in some of the others.

That the disease could not be attributed to any general atmospheric condition, the authors contend, 1st, because all the cases occurred in persons previously in the ship; 2dly, because no cases occurred on board the *Iphigenia*, which is moored under the stern of the *Dreadnought*; and 3dly, because no cases occurred in Greenwich Hospital, which is subjected to nearly the same atmospheric influence. The supposition that it was imported by infection from foreign parts, the authors consider to be highly improbable, from the very long period of incubation which, on that supposition, must be admitted; in some instances as long as thirty days. The authors also adduce the most satisfactory reasons for believing that the disease was not propagated by infection from one patient to another. The influence on which it depended seemed to act with an intensity which was variable, but generally greater in the lower decks; and in almost all cases its effects were first manifested during the night. The subsequent observations of the authors tend to show that the origin of the disease could neither be attributed to defective ventilation, to a crowded state of the ship, to the condition of the hold, nor to the food of the patients; the scale of diet being more liberal than those of the London hospitals, in consideration of the previous habits of the seamen who are its inmates. The disease did not attack the debilitated principally, but the most rapidly fatal cases happened in the medical ward, where cholera generally occurred as a complication of some visceral disease, and in patients previously reduced in strength.

PHYSICAL SOCIETY, GUY'S HOSPITAL.

Fifth Meeting, December 2, 1837.

DR. GUY IN THE CHAIR.

A PAPER was read by Mr. HIFF—

On the Difficulty of Diagnosis in the commencement of Malignant Diseases in the Abdomen; illustrated by a Case.

The principal feature of disease observed in the patient was severe pain, extending from the hip-joint half way down the leg; this was at first thought to be simple neuralgia. The abdomen, however, subsequently enlarged, and a tumor appeared on the left inguinal region.

Sir A. COOPER expressed it as his opinion that it was an extravasation of blood

beneath the iliac fascia. The patient gradually sank, and post-mortem examination shewed a tumor as large as an uterus in the sixth month of utero gestation, occupying the inguinal, iliac, and hypochondriac regions of the left side. The tumor contained a grumous fluid inclosed in a firm unyielding capsule. The greater part of the swelling consisted of brain-like matter, and was easily broken down by pressure. The left iliacus and psoas muscles were partly absorbed and destroyed, and the venter of the ilium was ulcerated.

Great difference of opinion existed between the members present as to the possibility of malignant disease commencing from an effused clot of blood. This point was argued by Dr. Hughes, and Messrs. Bently, Bird, Molloy, &c.

Mr. HILTON said that he thought it highly probable that the disease had commenced from the periosteum of the ilium. This opinion was in close accordance with that of Sir Benjamin Brodie, as explained by Mr. Hiff.

Dr. HUGHES and Mr. BIRD related cases of malignant disease, in which there had been great difficulty in diagnosis. In both cases the kidney was affected with fungoid disease, and the urine was stated to be natural. Dr. Hughes said that fungoid disease in young persons was not essentially combined with a peculiar cachectic appearance. He was of opinion that malignant disease, when near the surface, was generally the result of violence. This latter opinion was opposed by Mr. Roderick.

On the motion of Dr. Hughes, seconded by Mr. Bird, a vote of thanks to Mr. Hiff was unanimously carried, and the meeting adjourned.

At the next meeting, C. Aston Key, Esq. in the Chair, Mr. Molloy will read *Some Observations on certain Cerebral Affections.*

ACCIDENTS ADMITTED AT THE LONDON HOSPITAL.

Mr. SCOTT, Surgeon.

Mr. HAMILTON, Assistant-Surgeon.

Nov.	Sex.	Age.	Case.
28.	M.	21	Lacerated hand.
	M.	47	Lacerated arm (a).
	M.	22	Injured foot.
29.	M.	23	Contused chest.
	M.	35	Injured back.
	M.	20	Compound fracture of the leg (b.)
	F.	4	Burn (died Nov. 30.)
	M.	5	Burn.
	F.	10	Burn (died Dec. 3.)
	F.	3	Burn (died Dec. 5.)

Nov.	Sex.	Age.	Case.
30.	M.	30	Fractured radius.
	M.	45	Contused head and chest.
	M.	18	Injured head.
Dec.			
1.	M.	14	Contused hip.
	M.	25	Injured pelvis.
	F.	67	Fractured humerus.
	F.	20	Erysipelas of the head and face.
	F.	56	Injured head and mortified toe.
2.	M.	21	Lacerated scalp.
3.	M.	47	Fractured patella.
	M.	25	Fractured fibula.
4.	M.	19	Injured side.
	F.	65	Fractured radius, and contused head.
	M.	39	Fractured fibula.
	M.	15	Injured head, erysipelas.

In-patients..... 25
Out-patients..... 38

Total during the week .. 63

(a.) This patient was admitted with an extensive lacerated wound of the left forearm, extending only through the integuments down to the fascia, accompanied with great swelling and contusion of the whole extremity. On the second day after admission mortification commenced around the wound, and has passed for some distance up and down the arm. The case has been treated with ammonia, opium, and wine, and a line of separation between the dead and living parts is now becoming well defined. The accident was caused by a heavy cask rolling on the limb.

(b.) In this case the man's leg was caught between the two sides of a draw-bridge just as it was closing, the tibia being crushed just above the inner ankle, and the soft parts extensively lacerated; a large spicula of bone, about an inch and a half in length, was removed; there was considerable arterial hemorrhage for some time, but it was arrested by compresses of lint. The case is doing well.

Omitted last week—

In-patients..... 28
Out-patients..... 31
Total..... 59

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, Dec. 14, 1837.

Charles Wellington Kent, Lincoln. — John Henry Bunnister, Havant. — Arthur Young, — Frederic Nesbitt, Honiton. — John Henry Lamb-
brick, Wiltchurh.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, Dec. 12, 1837.

Abcess	21	Hernia	4
Age and Debility	304	Hooping Cough	82
Apoplexy	37	Hydrophobia	1
Asthma	79	Indigestion	14
Cancer	9	Inflammation	195
Childbirth	26	Bowels & Stomach	42
Cholera	12	Brain	11
Consumption	286	Lungs and Pleura	24
Constipation of the Bowels	3	Influenza	24
Convulsions	145	Insanity	13
Croup	28	Liver, diseased	11
Denitition or Teething	41	Menses	46
Diabetes	4	Miscarriage	10
Diarrhoea	9	Mortification	24
Dropsy	72	Paralysis	12
Dropsy in the Brain	21	Rheumatism	4
Dropsy in the Chest	10	Small-pox	26
Dys-utery	7	Sore Throat and	
Epilepsy	7	Quinsey	1
Erysipelas	2	Spasms	5
Fever	151	Stone & Gravel	1
Fever, Scarlet	32	Stricture	2
Fever, Typhus	17	Thrush	2
Fistula	5	Tumor	5
Gout	15	Unknown Causes	1385
Heart, diseased	31	Casualties	19

Increase of Burials, as compared with the preceding week .. 3002

In consequence of this being the last Weekly Bill which will be included in the General Bill of the present year, many of the parish clerks, who had neglected to make their returns in due order, have now reported their deficiencies, from which cause may be attributed the great increase which appears in the Christenings and Burials.

Since the new Registration Act came into operation, the office of Searcher has, in several parishes been discontinued; it has not, therefore, in these parishes, been possible to ascertain the diseases of which many of the persons have died; and, in such cases, the numbers reported are inserted under the head of Unknown Causes.

NOTICES TO CORRESPONDENTS.

We cannot insert the letter of Mr. Edward Heran.

The case communicated by Mr. S—ll does not appear to us of sufficient interest for publication.

It is not stated at what hospital the case of "Fungoid Degeneration of the Right Kidney" was treated. The case ought to be abbreviated. If our correspondent will attach his name to the paper, it shall be inserted.

The "Observations on the Mucous Membrane," by Mr. S., of Barnsley, are not sufficiently practical for our pages.

"The Homoeopathist from actual experience" must adduce stronger facts than any contained in his letter of Dec. 7th.

As Mr. Coles affixed his name to his paper, we do not think it would be quite fair to publish an anonymous answer to it: "M.," of Dec. 8th, will therefore excuse us.

Numerous other correspondents, the receipt of whose papers have not been acknowledged, will find them in our next number.

WILSON & SONS, Printers, 57, Skinner-st., London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, DECEMBER 23, 1837.

LECTURES

ON

FORENSIC MEDICINE;

Delivered at the Aldersgate School of Medicine.

By SOUTHWOOD SMITH, M.D.

Physician to the London Fever Hospital.

LECTURE IV.

Confirmative and Infirmative Facts. — Fallaciousness of Evidence. — Fallaciousness of Direct Evidence; of Circumstantial Evidence. — Cases in illustration. — Misdecision sometimes the inevitable consequence of Ignorance; sometimes subsequently removed by Science. — Induction as to Punishment. — Doctrine of Livingstone, of Bentham, and of Paley, on the Punishment of Death.

Confirmative and infirmative facts.—Every fact, the accumulation of which tends to render the existence of the principal fact probable, is termed a confirmative fact. Every fact which tends to weaken, or to render infirm, the probative force of another fact, is termed an infirmative fact. There is no confirmative fact which may not be attended with its opposing infirmative supposition. In the case of Donellan, one of the confirmative facts was his secret practice of distillation; the corresponding infirmative supposition was, that the distillation might have been carried on for the purpose of obtaining rose-water, or of procuring some other product of distillation; and great efforts were made on the trial to give to the circumstance this very colour.

If either a confirmative or an infirmative fact be overlooked, the consequence, it is obvious, may be fatal to one or other of the direct ends of justice. If the circumstance overlooked be a confirmative

fact, the wrong-doer may escape punishment; if an infirmative fact, an individual who is not a wrong-doer may be subjected to all the penalties of guilt.

Fallaciousness of evidence.—I have already stated the properties which give to a mass of evidence its highest degree of probative force, which constitute its perfect trustworthiness, correctness, and completeness. How difficult it is to secure these two properties, how completely the judgment may be deceived even in a case in which these properties appear to be obtained in the highest degree of perfection, numerous instances on record, in which individuals really innocent have sunk under the load of imputation heaped upon them by fallacious evidence, but too fully testify.

Fallaciousness of direct evidence.—Sense itself may be deceived—that is, at least, possible; and being possible, it follows that direct evidence itself may be fallacious. Deception of this kind is certainly rare. Many things called deceptions of the senses are really false inferences drawn from a true testimony, delivered by the senses. Sense seldom deceives, but reason often errs; the impression made upon the eye and the ear is in general correct, but the induction drawn from the impression is often rash and false.

The story of the traveller and the innkeeper (Bradford), to which I have already adverted, may be considered as affording a case the very next remove from one of direct evidence. The innkeeper (Bradford) had come to the knowledge that his guest was well provided with money; he had determined, at least, on robbery; he had gone to the traveller's chamber for the purpose of accomplishing his purpose; but he had been anticipated by another traveller, with whom Bradford had had no communication, and who had contrived to perpetrate the murder, and to escape unseen with the booty. Bradford, discovered in the very chamber of the murdered man,

with the very weapon in his hand with which it was manifest that the mortal wound had been inflicted, what inference more natural, more ready to occur to every mind, than that the innkeeper was the murderer? He was universally believed to be so; the evidence that he had perpetrated the crime appeared irresistible; he was convicted and executed; yet he was really guilty only of the crime of intending to commit a robbery; there was no evidence whatever that he even intended to commit murder. The actual murderer escaped unsuspected, and on his death-bed confessed the fact.

Fallaciousness of circumstantial evidence.—It has been said that circumstantial evidence is more trustworthy than direct; that circumstances cannot lie. There are circumstances which cannot lie—namely, circumstances which connect the inference with the fact so closely as to render the induction absolutely necessary and unavoidable: as when a witness deposes that he actually saw a man run his sword through the body of his victim. Any circumstance short of this—any circumstance which leaves the conclusion in any the slightest degree contingent, may lie, and has lied. If a witness swear that he saw the prisoner draw a reeking sword from the side of a dead man, even in that case the conclusion that the prisoner was the murderer of the deceased is not absolutely necessary and unavoidable, and therefore may be false. This act of the prisoner might have been the friendly act of an innocent man, who had accidentally passed that way after the murder was committed. Even if it were the prisoner's own sword, it might have been snatched from his side and plunged into the body of the deceased by some one who had escaped; or the deceased might have borrowed the sword from the prisoner, and plunged it into his own body.

Cases in illustration.—Many appalling accounts are on record of the fallaciousness of circumstantial evidence—of the false decision which has been the result, and of the consequent infliction of horrible injustice. One case, having a fortunate issue, is curious. September 11, 1772, came on, at the sessions in the Old Bailey, the trial of one Mall, a barber's apprentice, for robbing Mrs. Ryan, of Portland street, on the high-way, on the 17th June last. The witnesses swore positively to the identity of the lad, and the whole court imagined him guilty. He said nothing in his defence but that he was innocent, and that his evidences would prove it. His evidences were the books of the court, to which reference being made, it appeared that on the day and hour when the rob-

bery was sworn to be committed, the lad was on his trial at the bar where he then stood for another robbery, in which he was likewise unfortunate enough to be mistaken for the person who committed it; on which he was honourably acquitted.

A remarkable instance of the deceptive circumstances by which it is possible for an innocent person to be surrounded and overwhelmed, is afforded by the melancholy case of Le Brun, the domestic servant of the widow Mazel, whose story I adverted to in the first lecture, and respecting which I stated that five sorts of professional persons were consulted. All the suspicious circumstances in which this poor lad was involved, had been preconcerted—all of them had been arranged by a companion of his, the real murderer, who had contrived to obtain a portion of his hair, to steal from him his knife, to purloin a cravat, and to get into his possession for a short time the key of the shop, from an impression of which, in wax, he procured a false key.

With the story of the aged Calas all Europe resounded at the time the event took place. This old man was a merchant of Toulouse; he was 70 years of age. He was greatly respected for his probity; but in the bigoted age and country in which it was his misfortune to live, he was bitterly hated by many as a protestant. He had a son, 28 years of age, robust, of a melancholy turn of mind, a student of the law. On account of his being a protestant, difficulties were thrown in the way of this young man in obtaining his license to practise his profession. Imagining these difficulties to be insuperable, his mind became extremely irritable and desponding, and he formed the determination to put an end to his existence. This resolution he effected by hanging himself, having fastened the cord to a billet of wood placed on the folding doors which led from his father's shop to the store-room. Two hours afterwards he was found lifeless. The father removed the cord from the body, and, as far as the confusion of mind into which he was plunged permitted, he obliterated or changed some of the appearances about the body, and other circumjacent objects. Here was forgery of real evidence. On his examination he denied some of the facts evidentiary that the death was not natural; he concealed some of the facts evidentiary that death was produced by strangulation. To what end these aberrations from the strict line of truth? The concealment of crime? No; for on the part of the accused there was none. The object was to save the reputation of his departed child, and thereby the reputation of the family, from the igno-

miny which, had the direct truth been known, would, in that age of ignorance and bigotry, have been stamped upon it.

The people, stimulated by religious prejudice, carried the dead body to the town-house, where it was the next day examined by two medical men, who, without viewing the cord, or examining the place where the death had been effected, declared that the young man had been strangled. The father was condemned to be broken on the wheel; the sentence was carried into execution; and the victim expired with the most ferrent and solemn protestations of his innocence.

Reflection, when too late, returned. It was now recollected that the son had been of a melancholy turn of mind; that no noise had been heard in the house when the deed must have been done; that his clothes were not in the least ruffled; that only a single mark was produced by the cord; that this mark was in the situation which indicated suspension; and, in addition to all these circumstances, it was remembered that the dress proper for the dead was found lying on the counter, close to the spot where the suspension took place. Voltaire espoused the cause of the injured family. The case was carried up to the Council of State, who reversed the judgment of the inferior tribunal, and vindicated the memory of John Calas; but alas! the tribunal had pronounced and executed an irreversible decree, and no earthly power could render compensation to the injured.

Misdecision sometimes the inevitable consequence of ignorance—ignorance which science subsequently removes.—Sometimes misconception, and consequent misdecision, is the inevitable result, not so much of the fallaciousness of evidence, as of the want of knowledge necessary to the true understanding of a particular article of evidence. There may be placed fully and directly before the eye, a circumstance demonstrative of an essential error: nevertheless, into that error the mind must inevitably fall, because it is destitute of the knowledge requisite to enable it to perceive the true character of that circumstance. In such a case, the desire to avoid error, however earnest—the endeavour to avoid it, however strenuously put forth, in the most minute and laborious investigation of all the ascertained circumstances, together with the exercise of the most cautious and strictly logical reasoning concerning them, can be of no avail. Additional knowledge is the only thing that can be of avail. At length science affords that additional knowledge, and then the error becoming manifest, the avoidance in all time to come of misconception and conse-

quent misdecision, in relation to that particular case, is easy; but the avoidance of misdecision may still not be possible in relation to every other subject whatever, of which experience and science have yet to render the knowledge of the human mind perfect.

That wrong conviction, followed by judicial murder, the consequence of misdecision, rendered inevitable by this want of knowledge, has taken place, is beyond all question. There is reason to believe that an instance of it occurred recently.

“A man of the name of Gilchrist, some time ago, was condemned and executed at Glasgow. He and his wife lived in an irregular rambling sort of way, getting drunk sometimes for days together. On one occasion, after their return home in the evening, the people who lived on the floor above them heard a noise like that of two persons struggling, and soon afterwards a rattling or gurgling, and moaning, as of one choking or bleeding to death. They so strongly suspected that all was not right, that they called down to Gilchrist, through the floor, that they were afraid he was killing his wife. In no long time they were alarmed by the smell of fire, and the filling of the house with smoke; upon which they went down to Gilchrist's apartment and demanded admission. After some delay he admitted them, and in doing so appeared to them to have come out of an inner room, where he said he had been asleep in bed. On letting them in he stumbled over the body of his wife, who lay in the outer apartment quite dead, kneeling before a chair, and very much burnt. The prisoner was accused of having murdered her, and burnt the body to conceal the manner of her death: he, on the other hand, alleged that he had gone to bed tired, and knew nothing of what had befallen her until he was awakened; and that he presmied her clothes had caught fire while she was intoxicated, and that she was thus burnt to death.”

What remained of the body was examined by medical men; but their report was merely, that the body was so much burnt that nothing could be learnt from it as to the cause of death. The man was hanged, to the last vehemently and solemnly denying that he was guilty.

There is the strongest ground for supposing that the medical report, in affirming “that the body was so much burnt that nothing could be learnt from it as to the cause of death” was incorrect. Nothing could be learnt from it with the knowledge which these medical examiners possessed; nor perhaps with the knowledge which any medical man possessed at that time.

But that this very body exhibited appearances capable of demonstrating the true cause of death, had the real character of these appearances been understood, is rendered in the highest degree probable by the result of an inquiry to which a subsequent case gave rise.

The wife of a man was apparently burnt to death under circumstances exactly similar to the preceding. "The prisoner and his wife lived together on bad terms. On the night of her death the woman had returned home at a late hour, after having lighted a candle and got some whisky at a neighbour's house. At this time certainly the husband was in bed; but, some time afterwards, there was heard a considerable noise, like that of struggling, and of chairs pushed up and down the room. Not very long after, the neighbours were alarmed by a strong smell of fire proceeding from the prisoner's apartments. They therefore knocked at his door for admission, but in vain; all the noise they could make did not bring him to the door. At last a man forced his way in, by breaking the window of the outer room, and, on entering, found the room full of smoke, and observed something burning red in a corner, over which he instantly threw a pitcher of water:—it was the body of the woman burning on the hearth. Several persons now entered the inner room; they found the prisoner either asleep or feigning to be so. On being roused and told of his wife's death, he expressed neither surprise nor sorrow, but coolly demanded by what authority the people had broken into his house." The presumptions were strong against him; they were completely removed by the result of a scientific investigation suggested by certain appearances on the dead body. The burns on those parts of the body which were not reduced to a cinder exhibited peculiar characters which appeared to the medical examiners to indicate that the burning had taken place during life. This idea led to the institution of a series of experiments with a view to ascertain whether a part of the body burnt during life, and a part burnt after the extinction of life, present the same or different appearances; and if different, whether the differences are definite and constant. The result of this inquiry was the discovery of the means of determining with indubitable precision, in every case in which the whole of the body is not reduced to a cinder, whether the body were alive or dead when burnt.

In the present case, on examining the face and extremities (the parts of the body which were not burnt to a cinder) "we found (says the late Dr. Duncan) what we

were unanimous in considering to be incontestible proof that the woman had been burnt to death—that she had been set fire to while alive, and had died in consequence of the burning. There was every mark of vital reaction; some spots merely red and inflamed, others scorched to a hard transparent crust, but surrounded with distinct redness, and a great many blisters filled with lymph, perfectly different from those produced on the dead body, which are not filled with a fluid, but with air or vapour. In short, we found appearances exactly similar to those of fire on a living body; and therefore we reported, as our unanimous opinion, that the deceased was burnt to death."

But for the light shed upon the true nature of this case by science, coming at the very moment it was needed to the aid of justice, there cannot be a doubt that the fate of this man would have been the same as that of Gilchrist; that he would have been put to death on evidence in his case now certainly known to be fallacious, as Gilchrist had been put to death on evidence, in all probability, equally fallacious.

Induction as to punishment.—To infer from these and similar cases which demonstrate the fallaciousness of circumstantial evidence, that such evidence is not to be trusted, would be a false induction. Neither in a court of justice nor in the ordinary affairs of life could we proceed a single step without placing trust in it. That it may mislead is an infirmity inherent in its nature which cannot be cured. One consequence, however, inevitably follows from this infirmity in the nature of evidence. *The punishment annexed to a crime, the proof of the commission of which rests on circumstantial evidence, ought in no case to be of a nature which is irremediable, which admits not of compensation, should proof arise that he who has been pronounced guilty, is really innocent.* The injustice when it happens is so dreadful, that the risk of it ought on no consideration to be incurred. This is an argument against the punishment of death which may supersede all others. It is alone sufficient to outweigh every advantage which any one has ever supposed to result from the employment of death as an instrument of punishment, and is absolutely unanswerable and irresistible.

Doctrine of Livingston on the punishment of Death.—How solemn and profound was the conviction of this truth on the mind of the great American legislator, the illustrious Livingston, who on being empowered by the House of Representatives of the State of Louisiana to prepare for that State a Penal Code, in his first Report to

the Legislative Assembly thus speaks of the punishment of death: "I approached the inquiry into the nature and effect of this punishment with the awe becoming a man who felt, most deeply, his liability to err, and the necessity of forming a correct opinion on a point so interesting to the justice of the country, the life of its citizens, and the character of its laws. I strove to clear my understanding from all prejudices which education, or early impressions, might have created, and to produce a frame of mind fitted for the investigation of truth, and the impartial examination of the arguments on this great question. After the best use that my faculties would enable me to make of all the sources of knowledge on this subject within my reach, after long reflection, and not until I had canvassed every argument that could suggest itself to my mind, I came to the conclusion, that the punishment of death should find no place in the code which you have directed me to present.

"In coming to a resolution on this solemn subject, we must not forget a principle, established on the soundest reason, that other things being equal, that punishment should be preferred which gives us the means of correcting any false judgment to which passion, indifference, false testimony, or deceiving appearances, may have given rise. Error, from these or other causes, is sometimes inevitable, its operation is instantaneous, and its fatal effects in the punishment of death follow without delay; but time is required for its correction: we retrace our steps with difficulty; it is mortifying to acknowledge that we have been unjust, and during the time requisite for the discovery of the truth, for its operation on our unwilling minds, for the interposition of that power which alone can stop the execution of the law, its stroke falls, and the innocent victim dies. What would not then the jurors who convicted; the judges who condemned; the mistaken witness who testified to his guilt; what would not the whole community who saw his dying agonies, who heard at that solemn moment his fruitless asseverations of innocence; what would they not all give, to have yet within their reach the means of repairing the wrongs they had witnessed or inflicted?

"Instances of this kind are not unfrequent, many of them are on record; several have taken place in our own day, and a very remarkable example which was given but a few years since, in one of the northern states, shows, in a striking manner, the danger of those punishments which cannot be recalled or compensated, even though the innocence of the sufferer is rendered clear to demonstration. A few such instances in a century are suffi-

cient to counteract the best effects that could be derived from example. There is no spectacle that takes such hold on the feelings, as that of an innocent man suffering by an unjust sentence; one such example is remembered, when twenty of merited punishment are forgotten: the best passions take part against the laws, and arraign their operation as iniquitous and inhuman.

"To see a human being in the full enjoyment of all the faculties of his mind, and all the energies of his body; his vital powers attacked by no disease; injured by no accident; the pulse beating high with youth and health; to see him doomed by the cool calculation of his fellow-men, to certain destruction, which no courage can repel, no art or persuasion avert; to see a mortal distribute the most awful dispensations of the Deity, usurp his attributes, and fix, by his own decree, an inevitable limit to that existence which Almighty power alone can give, and which its sentence alone should destroy, must give rise to solemn reflections, which the imposing spectacle of a human sacrifice naturally produces, until its frequent recurrence renders the mind insensible to the impression."

Doctrine of Bentham.—While such sound and humane views determine and guide the sanctions of the law in other countries, it is painful and humiliating to think, that still, in the impressive language of Bentham—"Death is the English judge's universal remedy: higher he cannot screw up the exertions of blind barbarity. To this point the labour of every session adds; at this a stop is made, because there is nothing beyond it.

"It is the part of the same man, the same natural and implacable enemy of justice, on the one hand to keep watch and ward in favour of the murderer, charging him not to let drop any the least hint from which justice may receive assistance, not to say any thing by which his guilt may be brought to light—and on the other hand to be no less active in his exertions to extend the demesnes of death. To the profit of cold barbarity he thus adds the praise of tenderness. The manly dictates of public utility are sacrificed to the cant of hypocritical or childish sentimentalism. The excess of the punishment becomes a sufficient warrant for not executing it. Extending the demesnes of death, he thus extends the range of his own despotism; of that preposterous state of things by which, every year, the lives of men, by dozens and by scores, are laid at the feet of every English judge."

Doctrine of Paley.—It is this preposterous state of things which is advocated as the best possible, or at least as the best

which is likely to be practicable, by Paley, whose work is the text-book of moral and political philosophy, taught in our universities, where most of our legislators and judges are educated. "By the number of statutes creating capital offences," says this writer, "it (the English law) sweeps into the net every crime which, under any possible circumstance, may merit the punishment of death. But when the execution of this sentence comes to be deliberated upon, a small proportion of each class are singled out, the general character, or the peculiar aggravations of whose crimes render them fit examples of public justice. By this expedient few actually suffer death, whilst the dread and danger of it hang over the crimes of many." "The wisdom and humanity of this design furnish a just excuse for the multiplicity of capital offences, which the laws of England are accused of creating beyond those of other countries. The charge of cruelty is answered by observing, that these laws were never meant to be carried into indiscriminate execution; the legislature, when it establishes its last and highest sanctions, trusts to the benignity of the crown to relax their severity."

But the direct effect of the system thus advocated is, to take away the *certainty* of punishment which all jurists admit, Paley among the rest, to be the most effectual means of preventing the commission of offences. "The *certainty* of punishment," says Paley, in the very chapter from which the above passage is taken, "is of more consequence than the severity. Criminals do not so much flatter themselves with the lenity of the sentence as with the hope of escaping. They are not so apt to compare what they gain by the crime with what they may suffer from the punishment, as to encourage themselves with the chance of concealment or flight. For which reason, a vigilant magistracy, an accurate police, a proper distribution of force and intelligence, together with due rewards for the discovery and apprehension of malefactors, and an undeviating impartiality in carrying the laws into execution, contribute more to the restraint and suppression of crimes than any violent exacerbations of punishment."

Bentham assigns as the reason why the English law and the English judges stop at the infliction of death, that there is nothing beyond it. If there had been any thing beyond it, and if the law and the judges had eagerly availed themselves, as they would certainly have done, of that something more beyond death, it is plain that Paley would have justified the principle and the practice. "If," he says, "a mode

of execution could be devised, which would augment the horror of the punishment, without offending or impairing the public sensibility by cruel or unseemly exhibitions of death, it might add something to the efficacy of the example, and, by being reserved for a few atrocious crimes, might also enlarge the scale of punishment, AN ADDITION TO WHICH SEEMS WANTING; for, as the matter remains at present, you hang a malefactor for a simple robbery, and can do no more to the villain who has poisoned his father. *Somewhat of the sort we have been describing, was the proposal, not long since suggested, of casting murderers into a den of wild beasts, where they would perish in a manner dreadful to the imagination, yet concealed from the view!*"

The frequency of capital punishments in this country, according to Paley, owes its necessity to three causes—much liberty, great cities, and the want of a punishment short of death. The evils arising from all these sources, according to him, can only be counteracted by adding to the number of capital punishments. "The uncertainty of punishment must be compensated by the severity. The ease with which crimes are committed or concealed, must be counteracted by additional penalties and increased terrors. Thus the statute of James I., relative to the murder of bastard children, which ordains that the concealment of the birth should be deemed incontestible proof of the charge, though a harsh law, was well calculated to put a stop to the crime."

The grand argument in favour of the infliction of capital punishment is the terror of the example. "If," continues Paley, "there be any thing that shakes the soul of a confirmed villain, it is the expectation of approaching death." The dreadful suffering endured by a criminal about to undergo execution, which suffering must be known to and is commonly witnessed by his confederates in crime, and which is necessarily brought home, in a greater or less degree, to the imagination of all addicted to the same guilty courses, must, it is presumed, if there be any thing that can do so, shake the soul of the most confirmed villains. The answer to such representations, and to the argument in justification of the punishment of death founded thereon, is the evidence given before a Committee of the House of Commons appointed to inquire into the effects of Capital Punishment. One of the witnesses examined before this Committee, a solicitor, who had practised for more than twenty years in the criminal courts, makes the following statement:—

"In the course of my practice, I have found that the punishment of death has no

terror upon a common thief; indeed, it is much more the subject of ridicule among them, than of serious deliberation. The certain approach of an ignominious death does not seem to operate upon them; for, after the warrant has come down, I have seen them treat it with levity. I once saw a man, for whom I had been concerned, the day before his execution, and on offering him condolence, and expressing my concern at his situation, he replied, with an air of indifference, 'players at bowls must expect rubbers;' and this man I heard say, 'that it was only a few minutes, a kick and a struggle, and all was over.' The fate of one set of culprits, in some instances, had no effect even on those who were next to be reported for execution; they play at ball and pass their jokes as if nothing was the matter. I have seen the last separation of persons about to be executed; there was nothing of solemnity about it, and it was more like the parting for a country journey than taking their last farewell. I mention these things, to show what little fear common thieves entertain of capital punishment, and that so far from being arrested in their wicked courses by the distant possibility of its infliction, they are not even intimidated by its certainty."

Another witness, a magistrate of the capital, being asked, whether he thought that capital punishment had much tendency to deter criminals from the commission of offences, answered, "I do not. I believe it is well known to those who are conversant with criminal associations in this town, that criminals live and act in gangs and confederacies, and that the execution of one or more of their body seldom has a tendency to dissolve the confederacy, or to deter the remaining associates from the continuance of their former pursuits. Instances have occurred within my own jurisdiction to confirm me in this opinion. During one sitting, as a magistrate, three persons were brought before me for uttering forged notes. During the investigation, I discovered that these notes were obtained from a room in which the body of a person named Wheller (executed on the preceding day for the same offence) then lay, and that the notes in question were delivered for circulation by a woman with whom he had been living. This is (he adds) a strong case; but I have no doubt that it is but one of very many others."

The Ordinary of Newgate, a witness perhaps better qualified than any other to give information on this subject, being asked, "Have you made any observations as to the effect of the sentence of death upon the prisoners?" answers, "It seems scarcely to have any effect upon them; the

generality of people under sentence of death are thinking, or doing rather, anything than preparing for their latter end." Being interrogated as to the effect produced by capital executions on the minds of the people, he answers, "I think, shock and horror at the moment, upon the inexperienced and the young; but immediately after the scene is closed, forgetfulness altogether of it, leaving no impression on the young and inexperienced. The old and experienced thief says, the chances have gone against the man who has suffered; that it is of no consequence; that it is what was to be expected; making no serious impression on the mind. I have had occasion to go into the press-yard within an hour and a half after an execution, and I have there found them amusing themselves, playing at ball or marbles, and appearing precisely as if nothing had happened."

Some of the investigations in which Paley engaged led him to pay considerable attention to the subject of evidence. He was well acquainted with that infirmity inherent in its nature, against the consequences of which, false judgment and wrongful punishment, no degree of human sagacity and caution can obtain security. Yet fully aware of this, he was capable of writing as follows:—"I apprehend much harm to have been done to the community by the overstrained scrupulousness, or weak timidity, of juries, which demands often such proof of a prisoner's guilt as the nature and secrecy of his crime scarce possibly admit of; and which holds it the part of a *safe* conscience not to condemn any man whilst there exists the minutest possibility of his innocence. Any story they may happen to have heard or read, whether real or feigned, in which courts of justice have been misled by presumptions of guilt, is enough in their minds to found an acquittal upon, where positive proof is wanting. I do not mean that juries should indulge conjectures, should magnify suspicions into proofs, or even that they should weigh probabilities in *gold scales*; but when the preponderation of evidence is so manifest as to persuade every private understanding of the prisoner's guilt; when it furnishes the degree of credibility upon which men decide and act in all other doubts, and which experience hath shewn that they may decide and act upon with sufficient safety; to reject such proof, from an insinuation of uncertainty that belongs to all human affairs, and from a general dread lest the charge of innocent blood should lie at their doors, is a conduct which, however natural to a mind studious of its own quiet, is authorised by no considerations of rectitude or utility. It counteracts the

care, and damps the activity of government: it holds out public encouragement to villany, by confessing the impossibility of bringing villains to justice; and that species of encouragement which, as hath been just now observed, the minds of such men are most apt to entertain and dwell upon.

"There are two popular maxims, which seem to have a considerable influence in producing the injudicious acquittals of which we complain. One is:—'That circumstantial evidence falls short of positive proof.' This assertion, in the unqualified sense in which it is applied, is not true. A concurrence of well-authenticated circumstances composes a stronger ground of assurance than positive testimony, unconfirmed by circumstances, usually affords. Circumstances cannot lie. The conclusion also which results from them, though deduced by only probable inference, is commonly more to be relied upon than the veracity of an unsupported solitary witness. The danger of being deceived is less; the actual instances of deception are fewer in the one case than the other. What is called positive proof in criminal matters, as where a man swears to the person of the prisoner, and that he actually saw him commit the crime with which he is charged, may be founded in the mistake or perjury of a single witness. Such mistakes, and such perjuries, are not without many examples; whereas, to impose upon a court of justice, a chain of circumstantial evidence in support of a fabricated accusation requires such a number of false witnesses as seldom meet together; an union, also, of skill and wickedness which is still more rare; and after all, this species of proof lies much more open to discussion, and is more likely, if false, to be contradicted, or to betray itself by some unforeseen inconsistency, than that direct proof, which being confined within the knowledge of a single person, which appealing to, or standing connected with, no external or collateral circumstances, is incapable by its very simplicity of being confronted with opposite probabilities.

"The other maxim which deserves a similar examination is this:—'That it is better that ten guilty persons escape than that one innocent man should suffer.' If by saying it is *better*, he meant that it is more for the public advantage, the proposition, I think, cannot be maintained. The security of civil life, which is essential to the value and the enjoyment of every blessing it contains, and the interruption of which is followed by universal misery and confusion, is protected chiefly by the dread of punishment. The misfortune of an individual (*for such may the sufferers, or*

EVEN THE DEATH, of an innocent person be called, when they are occasioned by no evil intention) cannot be placed in competition with this object. I do not contend that the life or safety of the meanest subject ought in any case to be knowingly sacrificed; no principle of judicature, no end of punishment, can ever require that. But when certain rules of adjudication must be pursued, when certain degrees of credibility must be accepted, in order to reach the crimes with which the public are infested, courts of justice should not be deterred from the application of these rules by every suspicion of danger, or by the mere possibility of confounding the innocent with the guilty. They ought rather to reflect, that he who falls by a mistaken sentence may be considered as falling for his country, whilst he suffers under the operation of those rules by the general effect and tendency of which the welfare of the community is maintained and upholden."

According, then, to the express statement of this authority, direct evidence itself may be fallacious. "What is called positive proof in criminal matters, as where a man swears to the person of the prisoner, and that he actually saw him commit the crime with which he is charged, may be founded on the mistake or perjury of a single witness. Such mistakes, and such perjuries, are not without many examples."

In regard to circumstantial evidence, according to this same authority, proof sufficient to warrant conviction is obtained, when the preponderance of evidence is so manifest as to persuade every private understanding of the prisoner's guilt; when it furnishes the degree of credibility upon which men decide and act in all other doubts, and which experience hath shewn that they may decide and act upon with sufficient safety. But universal experience testifies, that with this degree of evidence misdecision does take place, and that not unfrequently: this is not denied by Paley! Nevertheless, he argues that in all these cases the punishment of death—a punishment in its own nature irreversible and irremediable—may be justly inflicted, on the ground that those who fall by a mistaken sentence are unfortunate individuals, who may be considered as having fallen for their country.

Without doubt "certain rules of adjudication must be pursued, certain degrees of credibility must be accepted, in order to reach the crimes with which the public are infested; and therefore the courts of justice should not be deterred from the application of these rules by every suspicion of danger, or by the mere possibility of confounding the innocent with the guilty;"

but it is equally true, that no punishment ought to be inflicted on any criminal, however apparently complete the evidence of his guilt, which punishment, when once carried into execution, is absolutely irrevocable and irreversible, and which consequently admits not of the possibility of compensation for unmerited suffering, when the proof of innocence is established.

But, indeed, in the whole argument of Paley on this subject, there is involved every possible vice which can attach to legislation, and to the mode of the administration of the law. While there is an acknowledged chance of misdecision, a sanction is imposed which, in the case of misdecision, inflicts the most dreadful injustice that can have place between man and man, without the possibility of reversal or compensation.

Moreover, this dreadful and irrevocable sanction is attached to numerous offences; not, it is admitted, because those offences are in their own nature more atrocious than others to which it is not attached, but merely because they are more easy to be committed, and more difficult to be prevented.

Nay, this dreadful and irrevocable sanction is attached to many offences chiefly as a threat, there being no intention actually to carry it into indiscriminate execution. But in order to render the exercise of this discrimination possible, there must necessarily be lodged somewhere a power either to suspend the law, or to enforce its execution. This power is actually lodged with the administrators of the law. It follows that, in regard to this whole class of cases, the real law is not the statute, but the will of the administrator of the law. But the will of the administrator of the law is changeable; the sanction of the law is peremptory and unchangeable. The encouragement necessarily given to crime, by removing the punishment of it from the unchangeable to the changeable, it is attempted to counteract, partly by the reiteration and extension of the menace, and partly by the occasional seizure of an individual, not more criminal than the hundreds who escape, who is put to death.

In this manner the law, rendered at once hard, partial, and uncertain, generates in the criminal the hardness that leads to recklessness and violence, while at the very same time it encourages in him the hope of escaping with impunity.

Gentlemen, I make no apology for bringing before your view, on the present occasion, such considerations as these. You cannot perform the momentous and difficult duties which will devolve upon you, in a manner conducive to the public

welfare, or satisfactory to your own conscience, if you possess in any degree an enlightened conscience, unless you keep constantly in view the consequences which must result from the evidence you give in a court of justice.

And I take another ground. You will go forth into the world with minds enlightened and humanized by science, and having especially (for you must have grievously failed to derive from your *peculiar* studies their noblest profit, if you have not) a purer and more correct knowledge of human nature, than is common in the cultivators of other professions. The result cannot but be a deep conviction in your own minds—a conviction which I trust you will contribute largely to diffuse among other minds—a conviction of two truths which mankind have been slow to learn, but which are as certain as any mental, and therefore as any physical law, truths which ought to form the basis of all penal enactments, and to guide the imposition of all penal sanctions: first, that justice is not vindictive, but corrective; and, secondly, that an incomparably greater influence is to be exercised over human dispositions and actions, the evil no less than the good, by mildness, firmness, and benevolence, than by hardness, harshness, and implacability.

ON THE COMPOSITION AND ABSORPTION OF PUS.

BY M. BONNET,

Surgeon to the Hôtel-Dieu, at Lyons.

[Concluded from p. 454.]

SECT. V.—On the Absorption of Pus.

I DISTINGUISH the absorption of pus which has not been in contact with the atmosphere, from that which has. I have shown above, that the former contains no principles but those which are present in healthy blood; the latter may hold in solution an active poison—the hydrosulphate of ammonia.

(a.) *On the absorption of pus which has not been in contact with the atmosphere.*—With reference to its absorption, I distinguish in pus, first, the parts which dissolve in water; secondly, those which form an emulsion with albumen; thirdly, those which are undissolved and merely suspended.

The first are re-absorbed the most easily; their *ensemble* constitutes the serous part of pus, and, like all serous fluids, they can easily, under given conditions, re-enter the circulation: thus, when col-

jections supposed to be purulent are partly re-absorbed, they always contain a large proportion of serum—such as those of the pleura, the peritoneum, the articular synoviae, and cold abscesses. In the last, as every one knows, the serous parts predominate; and in the pus of the pleuræ, or of the articular synoviae, they are in a still greater proportion. If, for example, we place upon a filter the pus produced by an acute or chronic pleurisy, we see more than two thirds pass through in the state of a serum as limpid as that which swims upon the clot in the blood of a healthy man. This is by no means surprising, for in serous and synovial membranes, the product of purulent secretion is mingled in the same cavity with the serous exhalation.

When the serous part of the pus has been re-absorbed, the abscess, after having diminished for some time, remains stationary; and if it is then opened, we find upon its parietes clots, or filamentous masses, whitish and insoluble, which I cannot compare to anything so well as to those which remain upon the filter when pus has been placed upon it for some time, and a part of the serum has filtered through. These are the parts which, in consequence of their insolubility, have not been capable of being taken up by the absorbent vessels, or rather of being imbibed by the tissues.

The fatty matters which form an emulsion with the albumen of pus, seem to me to be absorbed with difficulty, and even to hinder the absorption of the serum. When they are abundant, they almost entirely prevent filtration, as may be seen in the creamy pus, which, when placed upon the filter, allows but a few drops to pass through. Now I do not believe that I am advancing a paradox, when I maintain that every substance which is difficult to filter is also absorbed with difficulty: this general proposition is derived naturally from M. Magendie's experiments on the imbibition of tissues; and hence it appears why creamy pus, that is, pus containing a large quantity of emulsive fatty matter, which is difficult to be filtered, is so rarely absorbed, that I cannot venture to affirm the possibility of the case.

As to the parts of pus which we have considered as consisting of fibrin, they do not dissolve in water, cannot be imbibed, and are evidently the most difficult to be re-absorbed; it is not surprising, therefore, that they are found on the surface of cold abscesses, when a part of the pus of those abscesses has re-entered the circulation.

We can now understand that if the immediate principles of pus which has not been in contact with the atmosphere are carried into the blood, either in whole or in part, they need not disturb its functions.

This absorption may seem frightful as long as we do not know the composition of pus, and consider it as a peculiar and hurtful substance; but our fears must cease as soon as we comprehend the relations of its composition to that of the blood, and see that its absorption is merely the restoring to the blood the serum and fatty matters which had been separated from it. Hence we see why the diminution of an effusion, supposed to be purulent, causes no more bad symptoms than if it were a dropsy; and we shall attach less importance to finding pus in the blood, a problem which it has often been attempted to solve by chemical analysis, without perceiving that it could not be done.

If pus, indeed, contained some immediate principles peculiar to itself, as urea and the yellow colouring matter are to urine and the bile, it might be possible to recognise it in the blood; but it consists of those immediate principles only which belong to the fluid which forms it, in its normal state; and hence the mixture of pus with the blood can only alter it by changing the proportion of its elements. When we find this change, how can we be sure that it arises from the absorption of pus? To arrive at this certainty it would be requisite to know all the varieties that can be produced in the proportions of the immediate principles of the blood, by other causes besides absorption—a knowledge which we have not attained, and which perhaps is unattainable.

(b.) *On the absorption of pus which has been in contact with the atmosphere.*—Pus which has been exposed to the atmosphere contains, when it is fetid, hydrosulphate of ammonia, with an excess of the alkali; and this salt necessarily accompanies the pus when it is absorbed, or even if its serum is absorbed alone; and a septic poison, which can be recognised by accurate methods, is thus introduced into the blood.

For ages writers have spoken of the absorption of putrid matters, and of the putrefaction of pus; but as they did not specify in what this putrefaction consists, and what are the new elements which it develops, it was impossible to trace these substances in the blood, where, moreover, the attempt to examine them by any known method, such as reagents, chemical analysis, and the microscope, would have been useless. But now that I demonstrate that the fætor and the alteration of putrefied pus depend upon the hydrosulphate of ammonia, and that I point out a certain method of discovering its presence, namely, the exposing to its vapours the test-papers which indicate the presence of sulphuretted hydrogen and ammonia, the solution of the problem becomes easy, and it is seen that if pus

altered by putrefaction gets into the blood, it can be discovered there. This I have done; but as I have not long thought of applying to the blood the method of discovery which I have employed for two years in the study of pus, I am in possession of only one well-established fact on the absorption of the hydrosulphate of ammonia, its presence in the blood, and its excretion by the urine. But this fact is sufficient to establish the possibility of the phenomenon; and the method being once shewn to be useful, will be capable of many applications.

CASE.—A man, aged 43, had the upper part of his foot and the lower part of his right leg severely contused by the wheel of a heavy carriage; all the subcutaneous cellular tissue of these parts became gangrenous, and the skin itself mortified over the malleolus externus, and the superior part of the tarsus, in irregular patches of about two inches square. On the sixteenth day the gangrene was well defined, and the cutaneous eschars were detached, but those of the cellular tissue were still partly adhering: the pus which ran from this vast abscess was filled with gas of an insupportable fetor, and gave a black colour to diachylon plaster, and to compresses dipped in the lead wash. Its alkalinity was well marked; it immediately restored the blue colour of litmus reddened by an acid, and when a rod, dipped in hydrochloric acid, was brought near the surface of the sore, white vapours were disengaged, copious and very thick. I put some in a phial, and on placing litmus paper reddened by an acid, at its mouth, its blue colour gradually returned, and then grew weaker on exposure to the atmosphere. These tests demonstrated the presence of free ammonia.

Continuing my experiments, I put two ounces of the pus in a phial, and then placed upon its mouth the test-papers for sulphuretted hydrogen. The solutions of lead, copper, and mercury, became of a very deep black; those of antimony, reddish; those of oxide of arsenic, yellow; and a salt of the tritoxide of iron, of a rusty colour; but, except this last change, which unquestionably depended upon free ammonia, they all demonstrated the presence of sulphuretted hydrogen. Moreover, the disengagement of this gas was quicker and more copious when I heated the pus in a water bath, or added a little sulphuric acid.

Meanwhile the patient, who, during the first days of his illness, had merely had an inflammatory fever, now laboured under the most serious symptoms, as soon as the sores were thus overflowing with fetid pus; on the tenth day he was seized with

rigors, which, after having lasted an hour, were followed by fevers, with extreme acceleration of the pulse, and abundant sweats. These febrile attacks were repeated until the fourteenth day, sometimes in the morning and sometimes in the evening. At a later period they were no longer preceded by a sensation of cold, but were merely characterized by fever and sweating. These fits were accompanied by a very fetid diarrhœa; the loss of strength became extreme, and the face of a paleness slightly tinged with yellow.

I had examined the pus on the sixteenth day, when it contained a large proportion of hydrosulphate of ammonia; and as the symptoms of the attack exactly resembled those of the absorption of pus, it was natural to suppose that the hydrosulphate of ammonia had entered the blood. I then ordered a small bleeding on the seventeenth day. The house-surgeon reported that the blood, on issuing from the vein, was more fluid than usual, and of a colour resembling the lees of wine. When I examined it, two hours afterwards, it had separated into serum and clot; the surface of the latter had become red by exposure to the atmosphere, and its physical appearances were quite regular. I repeated the same experiments on the blood as on the pus, and with the same results. The serum quickly restored the blue colour of litmus reddened by an acid; and its vapour, when disengaged by a slight increase of temperature, produced the same effect on the test-paper. Lastly, when the blood was not heated, and a rod dipped in hydrochloric acid was brought near its surface, white and thick vapours were disengaged in abundance. This blood, therefore, like the pus, contained free ammonia.

I first tried to find sulphuretted hydrogen by placing the serum in a phial, heating it in a water-bath, and exposing to its vapour papers dipped in solutions of lead, tartar emetic, and white oxide of arsenic. No change resulted; but when I made my experiments upon the clot mixed with the serum, that is to say, upon the whole blood, its vapour acted upon the test-papers in the manner characteristic of sulphuretted hydrogen, just like the vapour of the pus. As it was thus shown that the hydrosulphate of ammonia was present in the blood of this patient, I was naturally led to suppose that this salt might be found in all the secretions, and particularly in the urine. I accordingly collected some on the day following that on which I examined the blood. It was perfectly clear, of the natural colour, but a little paler, and deposited no sediment after being suffered to stand for two hours. It was very alkaline, which was

obvious either by dipping litmus paper reddened by an acid into it, or by exposing this paper to its vapour. This vapour, disengaged by a slight elevation of temperature, indicated, when tested in the usual way, the presence of sulphuretted hydrogen; so that it was evident that the hydrosulphate of ammonia absorbed from the surface of the gangrenous sore became mixed with the blood, and ran off in part by the urine. It is to be remarked, that although the urine was not albuminous, the tests for hydrosulphuric acid did not show that it was present.

The existence of hydrosulphate of ammonia in the urine, made me imagine that this salt might also be found in the perspiration. Unfortunately, I did not attempt to find it until the twenty-second day of the disease: at this period the sweats had ceased, and I had no other way of examining the perspiration but by applying turmeric and litmus paper to the skin; but the slips of paper became displaced during the night, so that I could not find them; and on the following days, whether from want of alkalinity, or from the quantity of perspiration being insufficient, no change of colour took place.

It was about this period (on the twenty-third day) that the eschars of the cellular tissue were completely detached; the sore began to be covered with healthy granulations; the pus gradually became less foetid and less alkaline; the urine recovered its acidity, and the febrile paroxysms ceased. The patient suffered from nothing but constant quickness of the pulse, extreme weakness, and diarrhoea, which diminished in foetus as the suppuration grew less. Six weeks passed away in this progressive improvement; the patient's appetite returned, and he took nourishment, but his strength could not be renovated; he continued dreadfully pale, and always lay upon his back. Eschars formed upon the sacrum; the diarrhoea returned, and the patient sank, without any particular symptom, rather less than three months after his accident. On examination, we found no structural disease in any of the thoracic or abdominal viscera; there was very little blood in the veins, and no fibrous clots in the aorta. The heart and the pulmonary arteries were completely empty. The large abscesses which had occupied all the subcutaneous cellular tissue of the foot, and of the inferior part of the injured leg, had entirely closed; the cicatrization of the sores was almost completed, and without doubt the cure would have been perfect, if the exhaustion and prostration of his strength had not carried off the patient by a truly exsanguine death.

The treatment which I adopted was the usual one. To get rid of the foetid pus, I

opened the abscesses freely; I made a considerable number of counter-openings, and used frequent injections with the chloride of lime; I removed, as soon as possible, the cellular tissue when it became gangrenous within; I gave *negus* (*limonade vineuse*) when I found that there was hydrochlorate of ammonia in the blood; and I administered the preparations of cinchona when the symptoms of exhaustion succeeded those attending the absorption of putrid matter.

If the surgical art is able to do much in this kind of absorption, it is chiefly in the way of prevention, by diminishing the secretion of pus, and preventing its contact with the atmosphere. Immediate reunion assists in attaining this object in operations; and it is to be accomplished, when large abscesses are opened, by making the incision under the skin to the affected parts. This method, to which I have been led by my scientific researches, is destined, as I hope, to exercise as beneficial an influence upon the treatment of large abscesses, as immediate reunion upon that of recent sores.

However this may be, when we have not been able to prevent the putrefaction of pus, and the injurious principles resulting from its putrefaction have been absorbed, can the demonstration of their presence in the blood lead to any useful application? I doubt it. We might imagine that it would be well to excite the urinary secretion, as it might serve as an outlet to the hydrosulphate of ammonia; and that it would be right to try to neutralize the excess of ammonia in the blood by acids, and to decompose the sulphuretted hydrogen by the chlorides, or neutralize it by the salts of lead, which might form an insoluble sulphuret; but unquestionably it would be a strange mistake to attribute any real efficacy to any of these means. Even chemically no one of them decomposes, or completely neutralizes, the hydrosulphate of ammonia when it is dissolved in serum; and even if this decomposition or neutralization took place, there is no proof that the blood would thereby regain its natural state. The following experiments, moreover, may serve to clear up this question.

At a time when I supposed that, in the diseases called putrid, the blood contained hydrosulphate of ammonia, without my having yet been able to detect it, I thought that if the blood found in the bodies of those who die of typhus fever, or of absorption of pus, owed its colour (which remains black after exposure to the atmosphere), its fluidity, and its want of coagulum, to its mixture with hydrosulphate of ammonia, I could give the same characteristics to ordinary blood by mixing it, as

soon as it was drawn from a vein, with hydrosulphate of ammonia. In order to verify this supposition, in bleedings performed on persons suffering under slight indispositions, I let the blood partly into dry vessels, and partly into vessels containing hydrosulphate of ammonia, or solutions of sulphuretted hydrogen. The blood mixed with either of these solutions (which were in the proportion of one-fourth or one-fifth of its volume), preserved its venous colour whatever was the length of time that I left it exposed to the air, and also remained liquid; while the blood of the same patients, received into dry vessels, became red and separated into serum and clot. Now if the blood mixed with hydrosulphate of ammonia had all the physical characters of that which is found in the bodies of those who have died of typhus fever, or absorption of pus, it was in vain that I aided acetic acid, or subcarbonate of lead, or chloride of lead; it neither recovered its power of becoming red, nor that of separating into serum and clot—a result which tends to shew, if it does not strictly prove, that the depravation of the blood is too great to be removed by the addition of substances which decompose, or partly neutralise, the hydrosulphate of ammonia; and hence we learn, that in the treatment of diseases capable of forming these putrid principles, it is in the *prevention* of their formation that art must be employed; and it is from such an attempt that really useful results are to be hoped for.

From the moment that I detected hydrosulphate of ammonia in the blood and urine of the patient whose history I have given, I have thought of searching for it in the same fluids, and by the same methods, in patients suffering from malignant typhus, or from purulent absorption following either an operation or the opening of a large abscess.

Hitherto, however, I have not had any favourable opportunity for prosecuting my inquiries. I wanted cases of typhus fever which had arrived at the stage in which there is fœtid suppuration from ulcers in the bowels, or cases of absorption of pus, indicated by paroxysms of fever, preceded by rigors, and followed by sweating; but though I have not had any opportunity of seeing these serious diseases for some time, they are frequent enough to allow any industrious man, taking advantage of the method of which I have laid down the rules and shewn the application, to ascertain what part is played by the hydrosulphate of ammonia in the changes which take place in the blood, the urine, and the sweat, of the patients who are affected by these diseases, including in general all those patients who labour under the symp-

toms which are called putrid. As it is always easy to examine the urine, and since, to judge by the case I have narrated, if the hydrosulphate of ammonia is in the blood, it is probable that it will also be in the urine, we ought always to begin by the examination of the latter fluid. We must first see whether it has lost its acidity and become alkaline; and if it has, we must then employ the tests which detect free ammonia; and then, after heating the urine, we must observe whether its vapour contains sulphuretted hydrogen. If the vapour causes no change in slips of paper dipped in solutions of lead, tartar emetic, and arsenic, we must then try if we cannot disengage sulphuretted hydrogen by the addition of sulphuric acid. I have examined in this manner the urine of several patients whose suppuration was fœtid, ammoniacal, &c. without any signs of absorption being present. As upon examination I neither found free ammonia nor sulphuretted hydrogen, I proceeded no farther; but had I found these substances in the urine, I should have extended my researches into the sweat and the blood; into the sweat, by applying turmeric, litmus, and acetate of lead test-papers to the axilla and the front of the chest; and to the blood, by examining its vapour by the method of which I have now repeatedly given the details.

My researches into the composition of pus, and the chemical reason of its varieties, have led me to some pathological facts, which are merely useful as they contribute to our comprehension of morbid phenomena: but the proof which I obtained of the existence of the hydrosulphate of ammonia in pus which has undergone putrefaction, brought me to more practical and useful applications. Knowing that the decomposition of pus cannot take place without the contact of the atmosphere, that indispensable agent in every kind of putrefaction, I thought right to study, as so many others have done, the best means of preventing this contact. My first plan was to make an incision into the parietes of abscesses under water,* and I did so in two cases where they were in the elbow and the

* Five years ago, M. Messiat, well-known for his investigations into empyema, when talking with me about the dangerous sequelæ of the extraction of foreign bodies from the knee-joint, which are generally attributed to the action of the air, proposed to extract them under water. At the time that Monro insisted so much upon the dangerous action of the atmosphere after operations, Aitken proposed to perform the Cæsarean operation in a bath. The air, however, is not of itself an irritant, but it is indispensable to putrefaction; its entrance into large abscesses is dangerous, not that it directly inflames their parietes, but because it allows of the putrefaction of the pus contained within them.

knee. In the former case I left the joint for several days in the water contained in the small vessel in which the operation had been performed; in the latter I followed a different plan.

The patient being placed in a bath, and his knee kept under the water, but near its surface, I made an incision an inch in length into the spot where the fluctuation was most perceptible; when the pus had escaped, I pierced the middle of the lips of the wound under water with a pin, and joined them by the twisted suture. This opening of the abscesses in the two patients was not followed by any bad symptom; but the pus soon returned, and rendered fresh incisions necessary. I then reflected that as the exit of the pus contained in an abscess diminished the pressure upon its parietes, and this diminution of pressure favoured the afflux of blood, it was necessary, when the pus had been discharged, to make a permanent compression, in order to prevent or retard its reproduction. I therefore covered the knees of the first two patients whom I next operated on, and afterwards those of three others whose abscesses I also opened under water, with strips of diachylon plaster so disposed as to form Scultet's bandage. Two of these patients, who were much relieved by these means, having unfortunately got up, I saw that I had given them too much liberty, and being convinced that the destruction of the cartilages which always follows abscess of the joints would not allow motion to be re-established, and that anchylosis was the most favourable result that could be expected, I resolved to treat knee joints, which had been the seat of open abscesses, like fractures, and accordingly to place them in the usual apparatus; but since the patient found it difficult to bear the constrained position, and I was afraid that lying on the back too long might cause sloughing of the sacrum, I suspended the apparatus, by fastening cords to the two extremities of the lateral splints. Thus taking my researches into the dangers arising from the putrefaction of pus as a starting point, I devised and put into execution, in cases of chronic abscess of the knee-joint, a method of treatment which consists,

First, in letting out the pus, the diseased joint being placed under water, and uniting the wound thus made, by the first intention, and also under water.

Secondly, in compressing the parts immediately by means of strips of diachylon plaster.

Thirdly, in keeping the bones of the thigh and the leg extended and fixed by means of a fracture apparatus, until anchylosis is produced.

Fourthly, in suspending this apparatus so as to allow the patient to move the trunk without allowing any motion of the bones which we wish to have anchylosed.

It is impossible for me at present to give the result of this complex method, because the original plan having been put into execution not more than three months ago, I do not as yet possess any complete case, and the additional means which have advanced this method to the point which I have just explained, being never still, cases which are merely begun would not allow my readers to judge of the plan with all its advantages. Moreover, in order to explain the method of treating abscesses of the joints, it would be necessary to set forth the pathological anatomy of white swellings, with the diagnosis of their several varieties, and the interpretation of the phenomena which they offer. I reserve for another period the publication of my labours in this field; but my reason for detailing the method of treatment which I employ in abscesses of the joints, was to show that the scientific investigations which form the basis of this essay have not been without a result; I wished also to fix the date of a method which I believe will improve the treatment of a very serious disease which most obstinately resists our ordinary means. I now do what I did in the preceding years for the solution of urinary calculi, and the radical cure of herniæ. A few days of fortunate meditation are sufficient to invent a method: whole years are necessary to develop it practically, and to pass upon it a judgment founded on experience*.

REMARKS

ON

THE NATURE AND TREATMENT

OF THE

DIFFERENT FORMS OF PORRIGO.

By WALTER DICK, M.D.

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[Continued from p. 419.]

Of Porrigo primarily affecting the Pilius Cysts, and Structure secreting the Hair—comprehending Porrigo Furfurans, and Porrigo Scutulata.

WE agree with Mr. Plumbe in believing that porrigo furfurans, and porrigo scutulata, are not two distinct diseases, but varieties merely of one and the same disease. In this country, they

* From the *Gazette Médicale*.

both receive the popular name of "Ringworm." We shall, therefore, describe them together; and we cannot do this better than in the words of Mr. Plumbe, who has described ringworm of the scalp, as occurring in this country, more faithfully than, perhaps, any other author*.

"The attention," says Mr. P., "is first attracted to the disease by the *falling off* of the hair of the part; there is little attendant itching, and no apparent fluid secretion on the spot. Sometimes, but not always, the patches are of a pretty regularly circular form, the margin being clearly defined, and exhibiting a line of scurf considerably thicker than that in the centre. In the centre of the spots the skin is scurfy, and the air thinned and easily extracted by the finger and thumb. What remains of it is unhealthy in appearance, some hairs being thin and delicate, others being the remains or stumps of those which have been broken or dropped off. There is a *downy or towy-looking substance* just rising above, and mixing with the scurf, evidently formed by feeble attempts at the production of new hair. Two, three, or more of these spots, varying in dimensions, are usually discovered on examining the head more particularly; and when the hair has been removed by shaving, they exhibit a red and slightly inflamed appearance. Several others, in an incipient state, will be discovered in different parts. The latter may be known before the hair begins to fall off, when they exhibit nothing beyond the appearance of a small discolouration about the size of a spangle; the hue is of a yellowish red, somewhat resembling the bran of the darker coloured wheat; others, a little larger, have decidedly assumed the ringed form.

"At the commencement of the disease, and for some time after, spots, evidently of the same nature as the affection of the scalp, may be seen on different

parts of the body; but the former being usually protracted for a considerable time, these spots generally disappear before much improvement is effected on the scalp. To describe them more minutely would be to copy very nearly the account of the first form of *Herpes circinatus* of Bateman*."

Although Mr. Plumbe despises (perhaps too much) minuteness of distinction in cutaneous diseases, his descriptions are generally very accurate—evidently the result of sound observation and extensive experience. The above quotation contains an admirable delineation of the most striking features of "ringworm," as it usually occurs in this country; and its faithfulness will be at once recognised by those even the least conversant with the disease. *Porrigo furfurans*, as described above by Mr. Plumbe, does not, most frequently occur in adults, as is said to be the case with the form of the disease so called by Bateman; it is met with, almost invariably, in young subjects, from infancy to puberty.

Ringworm of the scalp is described by some of the French writers under the terms *teigne tondante*, and *porrigo tonsoria*; but they are in error in supposing that their *porrigo tonsoria* is the species described by Willan under the epithet *decalsans*. Any one who attentively reads their descriptions will, I think, agree with me in this opinion.

Willan and Bateman assert that *P. furfurans* and *P. scutulata* begin with *achores*; but, with Mr. Plumbe, we are inclined to think that these elementary lesions, when they do occur, are accidental, rather than primary or essential symptoms, being generally produced subsequently by irritating applications, or by the disease extending upwards, and affecting the sebaceous glands. In this latter way, *achores* (or, as the French call them, *favi*) are frequently produced, we believe, during the progress of the disease.

As the *furfuraceous* patches of ringworm enlarge, they not unfrequently exhibit a somewhat granular appearance, consisting, when examined through a lens, of minute vesicles, which either subside spontaneously, or burst, and their contents concrete into delicate incrustations or scales.

* *Furfuraceous porrigo* is one of the group of heterogeneous diseases described under the term, *Alopecia*, in the *Cyclopædia of Pract. Medicine*. But it is scarcely right to make (as the writer of the article alluded to has done) a separate disease of *alopecia*, which is but a symptom common to several eruptions which attack the scalp. Such an article as the one alluded to, we may be allowed to observe, shews the urgent necessity of investigating the pathology, and zealously striving to establish the *exact seats* of the different diseases of the skin. Till this is effected, it must be pretty obvious, we should think, that dermatology will continue in a very unsatisfactory state.

* Plumbe on Diseases of the Skin, 4th edit. p. 141.

In long-standing cases of ringworm, nearly the whole scalp is rendered bald, exhibiting only here and there a little towy-like hair; and the affected parts appear *mealy*, from being covered with scurf. This scurf consists partly of epidemic *debris*, and partly of squamous incrustations, formed by a secretion thrown out from the pilous cysts. In some cases the scurf seems to consist almost entirely of these latter thin incrustations, as any one may be convinced by attentive examination. They are seen surrounding the hairs at their exit from the skin; they resemble scales of bran, and are quite different in appearance from epidemic scurf.

The thick circular scabs, by many thought to be characteristic of *P. scutulata*, are never met with except as the effect of improper treatment, and inattention to cleanliness. Those who look upon such scabs as characteristic of ringworm, have been misled by confounding it with the variety of *P. lupinosa* described by Alibert under the term *faeus scutiformis*, and which we have called *porrigo lupinosa conferta*.

From *P. furfurans*, and *P. scutulata*, so generally commencing with furfureous patches, from which the hair falls off and becomes altered in texture, we are inclined to think that it would be proper to refer them both to the common term, *porrigo furfurans*, and expunge the term *P. scutulata* from the cutaneous nomenclature. Alibert also seems inclined to consider *P. scutulata*, or, as he calls it, *P. tonsoria*, closely allied to *P. furfurans*. After alluding to M. Mahon's opinion of the affinity of *P. scutulata* with *P. lupinosa*, he remarks:—"Je ne saurais adopter cette opinion; je pense, au contraire, qu'elle se rapproche davantage de la porrigne furfurace*."

The separation and altered appearance of the hair in ringworm (under which term we always include *P. furfurans* and *P. scutulata*) are sufficient, in our humble opinion, to lead to the belief that the parts nourishing and secreting the hair are those primarily affected in this disease. Mr. Plümbe and others, however, take a different view of the matter. "Much light," says Mr. P., "may be thrown on the pathology of this affection by reference

to the known effects of the severe forms of *scarlatina*, as well as other diseases attended by determination to the skin. In this disease (*scarlatina*), where excessive excitement of the part prevails through its course, its termination is attended by extensive exfoliations of the cuticle, and occasionally by the separation of the hair of the scalp; which two circumstances appear to depend on the same cause—the excessive action of the vessels of the cutes. The action of which those vessels partake which secrete the cuticle, produces a morbid increase of this structure; while the privation of blood which (during the continuation of the inflammatory state of the cutes) the structure secreting the hair sustains, cutting off its nourishment, it is observed to drop off, apparently by the roots, though, if examined, never having its rounded healthy bulbs. The same privation of nourishment which the structure secreting the hair suffers in the extended inflammation of *scarlatina*, is experienced in an aggravated degree in the smaller space which ringworm commonly makes its appearance on; and hence it is that the hair separates, in this disease, without the formation of pustules." He adds, "Ringworm may be defined to be inflammation of a specific character, affecting the solid structure of the cutis." The hairs, in his opinion, exert a very mischievous influence on the disease; and to their presence he ascribes the formation of pustules.

The above explanation of the nature and phenomena of ringworm is too hypothetical, and at the same time *mechanical*, I apprehend. The supposition that a hyperemic state of the skin can be so circumscribed and permanent, and operate in such a way as by derivation to deprive the hair bulbs of their due *quantum* of blood, is not, when properly considered, supported by any recognized law of pathology. We are also of opinion that the phenomena of *scarlatina* are altogether inapplicable to the elucidation of the disease in question. Indeed, it is very problematical whether the desquamation of the cuticle, after *scarlatina*, be the effect of the previously injected state of the cutis; for it occasionally succeeds *scarlatina anginosa* unattended by cutaneous efflorescence. Those who wish to find cases establishing this fact, may refer to a clinical lecture of Dr. Graves',

* Vide Monographie des Dermatoses, tom. i. p. 470.

published in the MEDICAL GAZETTE for March last.

That ringworm consists primarily of subacute inflammation of a specific character, affecting the pilous cysts and structure secreting the hair, we infer from the following circumstances:—

1st. The thin bran-like incrustations so frequently seen surrounding the roots of the hairs, and consisting evidently of a morbid secretion from the pilous cysts.

2d. The falling off of the hair from affected parts, and the towy, unhealthy appearance of what remain.

3d. In pulling out the hair from affected parts, we have sometimes brought the bulbs along with them; and these never presented their usual or normal appearance*.

We proceed to make a remark or two upon

Porrigo Decalvans.

The form of porrigo, called by Willan and Bateman decalvans, presents patches usually of a circular shape, completely deprived of hair—the scalp, at affected parts, appearing of natural colour, smooth and shining. There is no visible cutaneous lesion: the hairs appear to drop off from their roots. These bald patches sometimes attain a considerable size, but they commonly appear of about the size of a shilling or a half-crown-piece. Sometimes there is but a single patch; in other cases there are three, four, or more, on the same subject, as we ourselves have occasionally observed.

Mr. Plumbe thinks that *P. decalvans* cannot be deemed, strictly speaking, a disease. However, from its appearing in schools and families, where the other forms of porrigo prevail, we are warranted in viewing it as allied to them.

From the phenomena of the affection, one would be led to suppose that the principle which produced it acted upon the organs secreting the hair in such a manner as to produce a temporary atrophied or inactive state of them. But its true nature, it must be confessed, is very much in the dark. We see it often continue for a very long time, and afterwards, either spontaneously, or during the application of stimulant liniments,

see, get well—the hair sprouting up, strong, healthy-like, over the previously bald patches.

Causes of Porrigo.—Porrigo is a disease to which, in some form or other, persons of all ages are subject; but it attacks chiefly the young, and from the frequency of its occurrence it is justly deemed the bane of youth. The forms of it most frequently met with in this country are porrigo furfurans and porrigo scutulata, which, as we stated above, are essentially one and the same disease, and designated by the common people, ringworm. In France, judging from the Hospital Reports, *P. lupinosa* seems to be the most common form. *P. decalvans* is the least frequent form, and indeed in this country is comparatively seldom seen.

Although all the forms of porrigo may arise spontaneously in those who are badly fed, and inattentive to cleanliness, yet it may be safely affirmed that it is by contagion they are most commonly produced. And it is a well-established fact, that all the forms which we have above described may be produced from one and the same contagious principle. It has been frequently observed that when a case of porrigo appears in a family, if rigid seclusion of the infected subject be not immediately adopted, the disease spreads, and not unfrequently in other forms than the one first developed. At schools, boys frequently contract the disease by wearing the caps of others affected with it; and in boarding-schools, it is sometimes communicated to several of the pupils by means of combs and towels, before the contaminating source is discovered.

However clearly the contagious nature of the different forms of porrigo is established, it is equally certain that the disease is not communicable to all subjects; and that for its transmission by contagion, certain conditions, as respects age, temperament, and state of the integuments, are requisite. We have seen ringworm attacking two or three subjects in the same family, almost at the same time, when the occurrence of the disease could not be traced to contagion. We have been led to believe that the disease, under these circumstances, originates from some dietetic irregularity, or from some article of food being of a bad quality.

From what we have observed, we are inclined to think that porrigo is not so

* I am inclined to think that the eruptive disease so common in dogs, and technically called "red mange," is closely allied in its nature to the disease the pathology of which we have just been discussing.

contagious as many suppose.* Alibert lately seemed inclined to deny, nearly in toto, its contagious nature. The ingenious Baron, however, was rather changeable in his opinions—it is not long since he was as extreme on the other side of the question.

[To be continued.]

SOME ACCOUNT OF MESMERISM.

By BARON DU POTET DE SENNEVOY.

[Concluded from page 423.]

Somnambulist Phenomena.—*Vision (so called) without the intervention of the Eyes.*

WHEN only a few men relate astonishing effects, which they affirm to have themselves produced, their testimony may be contested, and it may be believed that they are in error; especially if these effects are of an order superior to the usual phenomena of nature. Those doubts acquire still greater weight when, having been desirous to show and prove these facts, they have failed in their endeavours. But if these identical facts have been attested by individuals of the highest honour; if thousands of persons again assure us that they have themselves verified them; it is then only fair to believe that some obstacle exists which prevents them from being elicited according to our wish, and that it is very probable that nature may not always obey our desires, more especially when those desires oppose the laws which she has established. To call men charlatans because they are so unfortunate as not to succeed in some delicate experiment, is to forget that the most skilful professors of chemistry and physics, operating on inorganic bodies, and which are consequently uniform in their action, are at any moment liable to fail in their experiments without any one having a right on that account to deny what they have advanced, since it is generally known that extraneous causes, causes often very trifling, may destroy or disturb the conditions necessary to success.

* We have more than once endeavoured to contract the disease, and hitherto without success. But, probably, some may say, (perhaps with truth,) had I been more anxious to contract the disease, I might have been more successful in the experiment.

I am entirely ignorant of the law which regulates the production of the phenomena of somnambulist vision; but I know perfectly well what it is that in certain cases prevents their manifestation. It has been my fate, also, not to be always successful, and I have studied the causes of my failure. The following is the explanation.

When I operated on a somnambulist in silence and *recueillement*, and that I had around me only inoffensive persons who were ignorant of that which was about to be produced, or who awaited it without suspecting my motives, I was calm and tranquil; the action of my own being (*de mon être*) upon the somnambulist, was almost as regular as that of a machine, and what passed in the somnambulist was equally so. Nature then manifested herself unconstrained, and the resulting phenomena had a peculiar character, and almost always satisfactory.

But it was otherwise when my desire of convincing persons who doubted my statements induced me to permit them to be present at my experiments. These persons very soon acted upon me by their incredulity, often expressed by cutting words or sarcastic smiles. I ceased from that moment to be calm and composed; my mind became extremely agitated; my heart beat violently: it was when in this state that I was required, commanded, to justify my assertions. Unwilling to recede I continued the task, and pride prevented me from listening to the voice of wisdom. What was the result? The being whom I magnetised, and who had no cause of disturbance, for he was often ignorant of what would occur during his sleep, no longer went to sleep in the same manner. Sudden starts were observable; his cheeks flushed; his heart beat like my own; and although he might fall into somnambulism, I soon perceived that his sleep was not regular as before, and that my own excitement had thrown him into a similar state. It was in this condition that I urged him to obey, that I solicited him to give me proofs of his vision. He consented reluctantly; for he was aware that changes had taken place in his state; but finally he did consent, and we very soon had proof that his lucidity no longer existed, and that all his annunciations were illusory. My disturbance at this want of success

only added to the already existing difficulties, and rendered all my experiments negative.

Several lessons of this kind at length enlightened me, and I obtained the proof that I had discovered the cause of the non-success of these experiments, when, on repeating them in the presence of the same men, I had sufficient resolution to be unmoved by their remarks, and not to suffer myself to be demoralised by their contemptuous looks. Water, so transparent when calm and tranquil, no longer reflects objects when it is disturbed. The mirror is dimmed by a slight breath, and ceases to give a faithful image. If undue moisture is allowed to approach an electrical machine, the handle will be turned in vain; no electricity will be produced. These transient accidents having ceased, and calm being restored, order resumes its course, admirable phenomena are again presented; but those who have witnessed the disorder only, accuse you of falsehood, and your name is enrolled on the list of charlatans. What is to be done under these circumstances? To be silent, and to pity the men who force you to bend beneath their hasty decisions; to wait for time to enable you to triumph over them: for when men say no, respecting a fact, and Nature says yes, it is very certain that nature will eventually prevail.

Here are cases of vision without the aid of the eyes, attested by men of education. I have selected them from many others, because the greater number of those who witnessed them are living, and hold at this day a distinguished rank in the faculty of medicine at Paris, or in the scientific world.

M. Francœur, a distinguished mathematician, read in 1826, at the Philomathic Society, a paper containing very curious facts. Happening to be at Aix, in the society of some respectable physicians, especially Dr. Despine, head-physician of the establishment there, this latter related to him that he had witnessed for months the phenomenon of the transference of the senses, so singular, that M. Francœur thought it his duty, for the advancement of truth, to give an account of it to the Society.

In the first notice of this memoir, we read that the patient who is the subject

of it had the faculty of seeing, hearing, and smelling, by means of her fingers.

The second is much more remarkable. The observation was made on the daughter of M——, who possessed the esteem of the city of Grenoble, where he lived in retirement, and who was extremely afflicted by the illness of his daughter. He did all he could to conceal this illness, and would not permit any visits of curiosity. Among the different states exhibited by this invalid, and which Dr. Despine describes with great minuteness, he dwells particularly upon that of somnambulism.

"We have seen her select from a packet of more than thirty letters, that one amongst them which had been designated to her. She read on the dial-plate, and through the glass, the hour indicated by a watch. We have seen her write several letters; correct, on reading them over, the mistakes she had made; and recopy one of the letters word for word. During all these operations, a screen of thick pasteboard entirely intercepted every visual ray which could possibly have reached her eyes.

"The same phenomena took place at the soles of her feet, and on the epigastrium; and the patient seemed to feel a sensation of pain on being merely touched."

We find also some very circumstantial facts of vision without the aid of the eyes, in a curious memoir of Dr. Delpit upon two cases of nervous affections*.

"One of the patients read," says the author, "and read very distinctly, when her eyes were entirely closed to the light, by moving her fingers over the letters. I have made her read thus, either in full day-light or in the most profound obscurity, printed characters, opening the first book that came to hand, and sometimes written characters; giving her notes which I had prepared expressly before going to her residence. Was it only the sense of touch which then supplied that of sight? I know not; but I affirm that she read very fluently by passing her fingers over the letters."

This is certainly a very decided testimony, one rendered by a man to whom such facts presented themselves without his seeking them, without his having a suspicion of the possibility of their ex-

* *Bibliothèque Médicale*, t. 5^e, p. 308.

istence, and who, struck by their inconceivable singularity, took up the pen with the sole view of recording them.

Dr. Delpit, in some very short reflections annexed to these observations, appeals to the authority of several known authors, and in particular of M. Dumas. "Five years ago," says that celebrated author, "a young person from the department of l'Ardèche, having come to Montpellier to consult the physicians there respecting an hysterical affection, accompanied by catalepsy, presented an instance of a very strange phenomenon. She felt during the attacks such a concentration of sensibility towards the pre-cordial region, that the organs of the senses were as if entirely fixed there. She referred to her stomach all the sensations of sight, hearing, and smell, which were then no longer produced by the usual organs. This rare phenomenon, observed in a person very much calculated to inspire interest, was an object of attention to medical men, and of curiosity to the public.

"I am well aware," continues Dr. Delpit, "that facts of this kind, in opposition to all the laws of nature, ought not to obtain without difficulty, nor without restriction, the assent of sagacious minds fearing to be imposed upon; but if observations upon it are multiplied, if the most minute circumstances of each observation are scrupulously verified, the possibility of a phenomenon must then be acknowledged, which perhaps only appears so wonderful from the absence of many facts with which to compare it."

These cases are recent, and are of such a nature as to render deception respecting them impossible. Here are testimonies rendered by living authors, above all suspicion of imposture.

It would be very difficult to find the motives for such assertions, if the facts had not been evident. Besides, they are incessantly being renewed. The following is a notice published in the *Gazette de Santé*, of September, 1829. Hear what the editor of the journal says:—

"*Animal magnetism.*—There is at this present moment, in the wards of M. Fouquier, a patient in whom magnetism has developed curious, and sometimes incredible, phenomena. Without ranging ourselves amongst the lovers of the marvellous, it is a part of our duty

to pay some attention to facts when they are authentic. Now these which we are about to relate, having been observed simultaneously by a great number of persons, we think that we may offer them to our readers as deserving of their full confidence.

"A person called Pétronille Leclerc, 26 years of age, a sempstress, had been admitted into La Charité, to be treated for a cerebral affection, accompanied with spasms of an epileptic character: of a very nervous constitution, pale, exhausted by former sufferings, and excessively irritable. She had experienced, subsequently to a fit of anger, a turning upwards of the eye-balls, which had resisted all kinds of medical treatment. The idea occurred to M. Sibire, who attended her, to try the effect of magnetism. He made the first application of it on the 29th of August, and repeated it several times afterwards. Here are some of the most remarkable effects which were noted down, and some of which passed under our own eyes.

At the first sitting, the somnambulist gave several proofs of lucidity. The person who magnetised her presented some objects to her—such as a phial with its contents, some sugar, and bread, which she described perfectly, without seeing them. She had a bandage over her eyes. Besides, in order to answer the questions which were addressed to her, she turned to the opposite side, and buried her face in the pillow. Without being interrogated, she said to the person who held her hand, "You have a headache." This was true; but in order to test her knowledge, the pupil answered her that she was mistaken. "That is strange," resumed she; "I touched some one, then, that had a headache, for I felt it distinctly." She distinguished also different individuals who were present at the experiment, by some particularities in their clothes. One of the most remarkable circumstances is the following:—The magnetiser had retired, promising her that he would return about half-past five, to awaken her. He anticipated the hour of his return, and the somnambulist remarked to him that it was not half-past five. He replied that a letter which he had just received had obliged him to return to her. "Ah! yes," said she immediately, "it is that letter which you have

in your pocket-book, between a blue card and a yellow one." It was found to be exactly as she had stated it.

"A watch was placed behind the occiput, and she was asked what o'clock it was. 'Six minutes past four.' It was seven minutes past four."

The following is another extract, from *La Gazette Médicale de Paris*, October 2, 1832:—

"There is at present under consideration, at the hospital Della Vita, at Bologna, a very extraordinary phenomenon of animal magnetism. A patient in that hospital is seized every three days, at precisely eleven o'clock in the morning, with so violent a convulsion that he entirely loses the faculty of perceiving sensations; sight, hearing, smell, disappear completely; the organs of the senses no longer perform any function; both his hands are clenched so fast that it is impossible to open them; if force were employed, his fingers would infallibly be broken. Dr. Cini, however, son of the painter, who attends him, has discovered, after long and attentive observations, that the epigastrium, at the distance of about two fingers' length above the umbilicus, received, during the convulsive crisis, all the perceptions of the senses.

"If the patient is spoken to, the epigastrium being touched at the same time, he answers, and, if ordered, he opens his hands of himself. If any substance is placed upon the epigastrium, he describes its form, smell, quality, and colour. During the contact of the finger, the convulsion continually diminishes, and seems to disappear; but if the finger is placed upon the heart the convulsion is again produced, and lasts as long as the finger is kept in that situation. If a flute be played on, the epigastrium being touched at the same time, the patient hears the music; and when, without interrupting the performance on the instrument, the finger is removed for a moment from the epigastrium, carried towards the heart, and immediately brought back to the epigastrium, he asks why the music is suspended at intervals.

These experiments have been made in the presence of the professors and the students, and have excited extreme surprise."

In a very curious work, printed some years ago in Germany, some instances

of the same faculty are related, which do not appear to be less conclusive; and these instances are equally attested by good observers.

The author of the work of which we speak, himself an eye-witness of the facts contained in it, has joined to his own narration that of three distinguished physicians whom he had called in to the somnambulist; and these three accounts agree strikingly with his own. Here are some of the principal cases of sight, without the aid of the eyes.

The somnambulist sees a paper written by M. le Baron de Strombeck. She indicates that the writing contains two paragraphs, and how many lines are contained in each paragraph.

"I went to fetch the paper," adds the author; "I counted the lines, and I shuddered as I should have done at the sight of a spectre." The same individual saw, in M. Strombeck's chamber in an upper story, upon his desk, a proof-sheet from his printer, which he himself was not aware had been sent. She described the places of several objects situated behind her, and the position of which had been designedly deranged, unknown to her.

The patient named, to the minute, the hour shown by Dr. Smith's watch, which did not agree with any one of those in the apartment.

She said that Dr. Mercard had money about him in a long green-striped purse, of a kind which had long been out of fashion. This physician had only carried the purse about him for the last few days, and he affirmed that he had never taken it out of his pocket except in his own house.

If we open the work of Pététin *, we find phenomena exactly similar to these, and no longer observed in a single individual, but upon seven women, all falling into catalepsy.

One of these cataleptic persons distinguished, successively, several cards which had been slipped under the bed-clothes and placed upon her stomach. She told the hour marked by a watch inclosed in the hand of one person, and distinguished an ancient medal in the hand of another. On another day the same patient distinguished a letter placed under Pététin's waistcoat, then

* Voyez *Electricité Animal*, par le Dr. Pététin, Paris, 1808.

a purse which had been slipped there by an incredulous person: she also told the number of gold and silver coins in each side of the purse. Towards the conclusion of this last experiment, she gave notice that she was going to describe what was most remarkable in each person present, which in truth she did. She also perceived through a screen that M. Pététin had taken her husband's cloak instead of his own, and caused him to be informed of his error.

Another patient gave no less extraordinary proofs of vision without the aid of the eyes. Being experimented upon by several persons, she was able to distinguish a small piece of platina inclosed in a bit of paper, a ball of copper hidden in a handkerchief, a seal with three faces, held in the hand, and ascertain what was engraved upon them. On one of the faces was a motto, in Italian, in such small characters, that Pététin was obliged to give great attention in order to read it. The patient also described two books which were placed in the pocket of one of the spectators; finally, she read a prescription contained in a sealed paper, and named what it contained: one ounce of bark and a drachm of valerian.

Dr. Bertrand, who gives an account of the work of Pététin, makes the following reflections:

"If Pététin has not falsified, it must be frankly confessed that the patients whose history he has given us had the faculty of acquiring, without the help of the eyes, the knowledge of the form and colour of bodies; and if the facts which he affirms are not true, not only he himself must have falsified, but we are under the necessity of making the same supposition with respect to the relations of his patients, their friends, and the physicians, who were incredulous at first, and ended by declaring themselves convinced. Now I do not fear to say, that the agreement of so great a number of witnesses selected amongst enlightened people, who could have no interest in wishing to deceive; this agreement, I say, to attest, as facts, what were only insipid lies, would offer a most singular moral phenomenon: for the work of Pététin contains the history of seven somnambulists, who all presented the same phenomena; and consequently this marvellous agreement of witnesses to a useless and daring imposture must have been renewed seven

times: it is impossible to suppose this." I could add here to the facts contained in the Report to the Academy already mentioned, my own personal observations, and all those scattered in great numbers in all the works on magnetism; but not one of these observations would be more conclusive than that which I shall extract from the *Dictionnaire de Médecine*, printed at Paris in 1827; article, *Magnetism*.

After having spoken of somnambulist faculties in general, M. Rostan thus expresses himself:

"But if sight is abolished in its natural organ, I have fully demonstrated that it exists in several parts of the body. The following is an experiment which I have frequently repeated; it was made in the presence of M. Ferrus. I took my watch, and placed it three or four inches behind the occiput. I asked the somnambulist if she saw any thing? 'Certainly, I see something shining; it gives me pain.' Her countenance expressed uneasiness; ours must have expressed astonishment: we looked at each other, and M. Ferrus breaking silence, said to me, that since she saw something shining, she would no doubt tell what it was.

"'What is it that you see shining?' 'Ah! I do not know, I cannot tell you.' 'Look well.' 'Stay, it fatigues me—stay, it is a watch.' Fresh matter of surprise. 'But since she sees that it is a watch,' said M. Ferrus, 'she will doubtless be able to tell what o'clock it is?' 'Oh no, that is too difficult.' 'Pay attention, endeavour.' 'Stay, I am going to try. I may, perhaps, be able to tell the hour; but I shall never be able to see the minutes.' And after having tried with the greatest attention, said, 'It wants ten minutes to eight,' which was correct. M. Ferrus was desirous to repeat the experiment himself, and he did so with the same success. He made me turn the hands of his watch several times; we presented it to her without having looked at it. She made no mistake."

Much ingenuity has been expended in arguments to prove that man cannot thus act upon his fellow-man. Our opponents say to us—Explain how this lucidity is produced, and we will believe the possibility of it. Let those who put this question to us, explain how opium causes sleep; how they move their limbs. They themselves con-

ness, they do not know the limits of possibility, yet they deny the existence of a new fact, because their minds are not able to conceive it. They tell us the eye is made for seeing, the ear for hearing; we know it as well as they do; but we know also that the ear and the eye are but instruments, and that the principle which sees and hears is not the organ itself. That this principle is not able to transfer itself to some other part of the body is an opinion which is contradicted by facts, facts produced by other causes than animal magnetism. Both catalepsy and ecstasy, observed by so many able physicians, furnish the most complete and evident proof of this. It is to be hoped that the mystery which accompanies this phenomenon will soon be unveiled.

Let me be allowed, in concluding, to express one regret—that of seeing so many educated men content themselves with denegations, instead of investigating a subject which offers so vast a field for their observations. Let them condescend to make use of a power which they have within themselves. Their discussions will then become profitable, and humanity will return them thanks, for they will have contributed to enlighten its course.

MEDICAL GAZETTE.

Saturday, December 23, 1837.

"Licet omnibus, licet etiam mihi, dignitatem Artis Medicæ tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

ON MEDICAL EXAMINATIONS.

IN our last notice of Mr. Whewell's work on the Principles of University Education, we introduced and commented on some of his observations on the subject of Practical Teaching. We proceed now, according to promise, to follow him into that of Examinations.

For various reasons, of which some are applicable to our case, he regards all compulsory examinations as "necessary evils," and this anomalous character

they certainly, in some degree, possess in medical education. They are necessary, and even indispensable, because it cannot be doubted that the public safety must be compromised if persons be admitted to superintend it, without its being in any way ascertained whether they are fitted for the discharge of the responsible duties they undertake. The destructive consequences so often resulting from the acts of unauthorised practitioners, prove at once the danger of admitting more with no testimony of qualification, and the utter inability of the public to form just opinions of the merits of those who, however different their qualifications, all profess the ability to render them service. It is, of course, a duty incumbent on the legislature to prevent unauthorised persons from thus imposing on the ignorance of the public, but while no laws exist against them which they cannot with ease evade, it must be the chief object both of examinations and education to guard the public against the incursions of those who, with an appearance of authority, might be doubly mischievous.

With some, examinations alone are deemed sufficient for the public protection; and the previous education, it is said, is of no importance, provided any person can satisfactorily prove to competent judges that he is fitted to undertake the care of patients. We need scarcely say, that in university education, where the subjects of examination admit of far more exactness in question and answer than in medicine, Mr. Whewell utterly repudiates such an idea. Such perfection of examinations is purely ideal, and the instances in which the plan is followed in medicine are themselves remarkable proofs of its being a bad one. To make this evident, we need only point to the very low degree of estimation in which those institutions are held, both by the public and by the profession, in which de-

degrees are conferred on persons whose education, if inquired into at all, is allowed to have been conducted at a distance, or to a very slight extent; for instances, we would refer to some of the German and Scotch universities,—mere refuges for the destitute.

This estimation of diplomas, in the present day, evidently depends much more on the degree of education which is required of the candidate previous to the examination, than on the severity of the examination itself. Whatever, indeed, may be the deficiencies of the certificate system, as it is called, the insufficiency of examinations becomes every day more clear. Besides, the class to whom it is said that compulsory education is burdensome, that of the natural geniuses is really too small in number to be legislated for, and when one *rara avis* does occur, he always shows how much better he would have been by a little discipline of education. In short, it is no less desirable than probable, that a proper compulsory education, and suitable compulsory examinations, may *together* be made the means of guarding the public against the perils of medicine.

We have already considered the former; let us proceed to the latter. That the examiners should possess honesty, judgment, and all the other qualifications required of those who are to be judges of questionable matters, needs no argument to prove; and that they should have a knowledge of the subject on which they examine, sufficient to enable them to form an accurate opinion of the degree of acquaintance with it which the candidate possesses, must be equally evident. But there is a question connected with this which has often been discussed by medical politicians, and which Mr. Whewell moots in reference to university examinations: it is, ought the same persons

to teach and examine in the same subject? The practice of our several boards of examiners would appear to answer this in the negative. But there is good reason to doubt whether this system be a wise one. Mr. Whewell justly says, "A man cannot conduct an examination well, unless he be quite master of his subject. If, instead of being an habitual study, in which he is familiar with all that has been done in its various forms, the subject be a portion of literature or science, which he takes up merely as an examiner, his acquaintance with it will probably be meagre, unconnected, and partial." He will not give the student an opportunity of showing the extent of his acquirements; he will not possess the knowledge contained in the most recent authorities, which are just those the student is most likely to have referred to, but will be inclined to question him on those few subjects which, of all he learned, have most faithfully abided by him. M. Bouillaud very candidly asked, when the subject was being discussed not long since in Paris, how he, or any hospital physician of like standing, could be expected to be capable of examining properly in anatomy or chemistry, which, though subjects of study in their pupillage, they had, of course, long since forgotten, while their minds had been engrossed in the objects of their present more immediate pursuits?

Full support to these opinions is furnished by the cases of some of our medical institutions. It is really quite lamentable to hear how easily the better class of students are able to detect the anatomical errors of their examiners at the Apothecaries' Hall; and we feel sure that the reputation of that establishment would be materially increased, if, so long as they think an examination in that subject essential, they would employ some professed anatomist for

the purpose. We mention this deficiency with the less hesitation, because it is really no imputation on the talents of any one to say that he has forgotten his anatomy, after a few years' absence from a personal study of it: it is, indeed, peculiarly characteristic of that science, that it may be easily lost in one half of the time which was necessary to acquire it; and we question whether it be possible for any one who is not continually engaged in examinations, or actual dissections, to retain for many years more than the broad outlines of it. There can be little doubt that it would be nearly the same with the Board at the College of Surgeons, if their practice was not in many parts essentially anatomical, and if many of them had not been in early life teachers of anatomy, so that they learned the principal portion of it so completely, that it forms still some of their most familiar knowledge. They wisely limit themselves to the subjects which they study for their professional practice, and hence their examination presents a remarkable contrast to the other, in the degree of repute in which it is held.

The principal, and indeed the only plausible objection, that we have ever heard made to the appointment of lecturers to be examiners in their own subjects, is, that they would be partial judges, to the advantage of their own, and the injury of others' pupils. A sufficient reason why a teacher should not examine his own pupil is, that the latter would be sure to know and favour the peculiar ideas which the former might entertain, and be almost as sure to choose for the subject of his questions; but we cannot think there is any good reason why teachers should not examine each other's pupils; and while the degree of rivalry which might thus be excited would be itself beneficial, we should not have the folly exhibited of

gentlemen endeavouring to judge of the proficiency of those better acquainted with the subject of examination than themselves.

A chief evil of medical examinations, as at present conducted, is that they fix a standard which all have to attain, and which it affords but little honour to surpass. If it could be that this standard should be such an one that none could pass without its being certain that he was fitted for practice, it would be well, and examinations might then be self-sufficient; but this, we have said, is unattainable. Yet it is to be lamented that, while to many the attainment of the average knowledge is the sole end of study, and while, when attained, there is still an uncertainty whether the candidate be really fit for practice, there should be so little present reward for those who, to make themselves truly competent for practice, exert a most laudable industry. We believe that the standard at present fixed is, as regards quantity, as high as can fairly be expected of the pupils, in the period during which they are expected to study. (We speak here of the College of Surgeons and the Apothecaries' Hall.) But we are sure some improvement might be made by altering the *quality* of the subjects required, by making them more practical.

In the examination at the College of Surgeons, this is, as we have said, a chief characteristic, and as the Board have been lately regularly improving their system at each change, we do not despair of seeing them require a demonstration on the dead subject of some parts of anatomy, and of the more frequent surgical operations. It may be true that they have already a standard which many whom they pass have but ill-attained, but they may rely on it, the desire of obtaining their diploma, whatever be the motive, is too great, in

most students, to let any moderate obstacle prevent them.

The examiners at Bridge-street might take a good place from those at Lincoln's Inn Fields, and seeing the repute the latter are held in, we wonder they are not jealous enough to imitate them.—We would recommend the passage quoted from Mr. Whewell, strongly to them; it points out the propriety of confining their examinations to practical medicine and pharmacy. As the apothecaries of the present day are much more in the habit of writing than of reading prescriptions, it would be well to test how a pupil could do that, by relating a case (a fresh supply being had for each night) and requiring him to prescribe, and even to dispense for it. With properly competent examiners on collateral subjects, and these, and a few other practical tests, we think they might easily put a stop to the practice of *getting up* in a month, a feat not unfrequently performed, by the assistance of very rough grinding, collections of drugs just like that of the examiners, and a view of the plants that are to be used on the particular night.

A word to the Council of the University of London, who evidently find that their situation is growing each day more difficult and responsible. It is so, principally because they were expected to do something new, and they now find few novelties in education are void of danger in experimenting with them. Let them look at the *present* condition, not at the future hypothetical prospects of analogous establishments,—those which have required little or no previous education, *all despised*—those which have required little or nothing else, *neglected*—those which, after a well-judged education, have informed the most practical examinations on practical subjects, *the most esteemed*. And let them read Mr. Whewell's most judicious work—it can be no disgrace to be taught

by such a man, and from every page of it they and all concerned in like pursuits may obtain ideas which no thinking will give them; for his knowledge is eminently practical, and the result of long experience.

SYMPTOMS OF STRANGULATED INTESTINE.

LATE BAYONET WOUND CASE.

To the Editor of the Medical Gazette.

SIR,

As some extraordinary doctrines have been lately enunciated by a well-known journal (I mean the *Lancet*), relative to the symptoms of a strangulated intestine, which, if allowed to pass without examination, may be regarded by some as being actually founded in truth, I would, if the inclosed communication* appears to be deserving of it, request its insertion into the pages of your valuable and extensively circulated periodical.—I remain, sir,

Your constant reader,

THOMAS MORTON.

2, Stanhope-street,
Hampstead Road, Dec. 20, 1837.

Gentlemen,—The object which I have in view, in bringing the subject of this paper before your notice, is to endeavour to show what are the symptoms of a strangulation of the small intestine which can serve as a means of diagnosis between that affection and some forms of peritonitis arising from other causes. I shall also endeavour to ascertain what are the symptoms of a strangulated intestine which may demand the surgical interference of the professional attendant, and which, when present, may leave no room for hesitation, or difference of opinion, as to the proper course that ought to be pursued in the treatment of the case.

I shall also consider it necessary to determine whether, in the case of a fatal termination, there are any morbid appearances on which we can rely *with certainty* as proofs of the existence, during the life of the individual, of strangulation of the intestine, and which, if found to exist, will warrant us in ascribing the death of the patient to the effects of strangulated intestine.

* On the Diagnostic Symptoms of Strangulation of the Small Intestine, and of Peritonitis arising from other causes, &c. Read before the Medical Society of University College, London, Friday, December 15, 1837.

On the other hand, should these morbid appearances be absent, it will be my aim to prove that we are not justified in attributing the death of the individual to strangulation of the intestine and its consequences, but to some other cause; and that he who persists in the expression of an opinion that the death did ensue from strangulation of the intestine, *knowing that these morbid appearances were absent*, lays himself open to the imputation of ignorance, or of being actuated by some improper motive.

Previously, however, to entering upon the consideration of these points, I deem it necessary to make a few observations relative to the meaning which I wish to be attached to certain terms, or expressions, which I shall have occasion to make use of; and by so doing, I trust I shall render myself less liable to be misunderstood.

1st. When I use the word *intestine*, I mean to refer only to some portion of the jejunum, or ileum.

2d. In using the expression *strangulated intestine*, I shall be understood to mean that condition of the bowel which, if *unrelieved*, must necessarily either destroy the patient in whom it occurs, or terminate in the destruction of the vitality of the portion of the bowel which is so constricted: and that where the patient's death shall have been produced by this cause, the portion of intestine which had been the seat of the strangulation shall be found *invariably* to present such remarkable and obvious changes in its appearance and structure, as cannot be overlooked, nor mistaken, in a post-mortem examination that is properly conducted.

3d. By the term *incarcerated intestine*, I would be understood to denote that state of the small intestine, which, when protruded from the cavity of the abdomen, and though firmly embraced by the borders of the opening in the tissues through which it escaped, shall not present any of those alterations of its appearance and structure, which have been already stated as *invariably* co-existent with, and demonstrative of, strangulation of the intestine.

I now proceed to determine what are the diagnostic symptoms, as occurring during life, which may serve to distinguish strangulation of the intestine from peritonitis arising from other causes; and to ascertain what are the circumstances, if any, which demand the interference of the surgeon for their relief.

It is almost superfluous for me to enumerate, in this place, the symptoms of strangulated intestine, for they are well known to all; yet, as I shall be obliged to establish a comparison between the symptoms of strangulated intestine and of peri-

tonitis induced by other causes, I feel myself obliged to mention them, in order that I may support the conclusions which I shall draw from such a comparison.

Thus, then, there is pain in the belly, with tenderness under pressure; desire, with inability, to evacuate the bowels; vomiting, first of the contents of the stomach, and then of the intestinal canal above the seat of the stricture. Generally, the matters vomited assume a bilious character as the case proceeds, and not unfrequently even a stercoraceous odour and appearance. In a later stage, the vomiting becomes more urgent and distressing. The constipation is complete; the pulse begins to fail, becoming feeble, irregular, and intermittent. The countenance is anxious, and haggard in its expression: cold and clammy perspirations ensue, and soon the scene is closed by the death of the wretched sufferer. Now there are two forms of strangulated intestine, and in both the symptoms and pathological conditions are the same; with one exception, however.

In one of these forms there is a circumstance present which is absent in the other, and it is to this circumstance that I request you will give your particular attention. The circumstance to which I allude is, the presence of a tumor externally, and which is connected with some portion of the abdomen. With the appearance of this tumor the symptoms of strangulation commenced, and it is upon the constriction of its contents that they depend.

This tumor is tense, firm, tender when compressed, and resists all attempts which may be made to reduce it by the taxis. There are changes which occur in the appearances of this tumor that are indicative of the condition of the parts that are contained within, for at first the tenderness is only slight, but as the strangulation of the intestine continues the tenderness becomes extreme. In the last stage, however, the tension and pain occasionally disappear.

The earlier of these symptoms are caused by the engorgement and inflammation of the strangulated bowel; the latter by the termination of the inflammation in the death, or gangrene, of the portion of bowel included within the constriction.

The time which is requisite for the development and succession of these symptoms is subject to considerable variation, but in the majority of cases it exceeds 48 hours, and is often prolonged to 4, 8, and even 10 days. In those cases that terminate before the lapse of 48 hours, the individuals have been seized, while in the enjoyment of full and vigorous health, no other circumstance concurring to diminish the intensity of the inflammation, or slacken the rapidity with which it

hurries through the several stages of its progress.

In the second form, where there is no external swelling, such as has been described to mark the first, it will be freely granted, I think, by all, that no operation is demanded on the part of the surgeon: therefore I am prepared to contend that unless the symptoms which exist, and which are referred to strangulation of the intestine, be not accompanied with an external tumor, originating from the abdomen, and which tumor can be shown by reference to the history of the case to be connected with the appearance of the symptoms, and also unless this tumor is firm, elastic, tender under pressure, and incapable of being reduced by the taxis, no operative proceeding ought to be recommended, or had recourse to, with the view of removing the supposed cause of the symptoms. And that although the rational symptoms observed are those which are usually caused by strangulation of the intestine, yet from the absence of this most essential diagnostic circumstance (the external swelling, firm, tender, and painful, and irreducible by the taxis), the surgeon is not called upon to operate; and so far is he from being liable to the charge of culpable neglect for not operating, that he deserves, and will obtain, the commendation of all competent judges, for the accuracy of his diagnosis, and his correct appreciation of the perfect inapplicability of the powers of his art. Let us now examine what are the morbid conditions of the intestine which are invariably found to be present where the strangulation of intestine has been the cause of death.

These pathological conditions are so remarkable in their appearance, and so universally constant in their presence, when strangulation has been the cause of death, that when *they* are absent, we are warranted in denying the truth of the assertion that strangulation was the cause of death.

On opening the abdomen, after death from this cause, there are usually all the evidences of extensive peritoneal inflammation, as evinced by the increased vascularity of the peritoneum covering both the intestines and the abdominal parietes. The extent of this must, no doubt, vary in different cases. The folds of the intestine are adherent to one another by recently effused coagulable lymph. Frequently, also, there is a copious secretion of serum, which may be clear and straw-coloured, or dark-coloured, from the presence of the colouring matter of the blood. It may be turbid, with flakes of lymph; it may partake more of the nature of pus than of serum.

But, gentlemen, these are, comparatively speaking, unimportant changes, and it is to the appearances which I am now to describe that I again request your attention.

It is the alteration which has been produced in the appearance and structure of the portion of intestine which has been strangulated, that you are to bear in mind. The colour of this part of the intestine is changed from its natural light grey tint to a dark red, or mulberry hue. The vessels are engorged with blood. Blood is extravasated into the tissues of the gut. The surface of the bowel is frequently dull from the presence of a layer of adhesive matter, by which it is glued to the adjacent surfaces. The folds of the strangulated portion are thus frequently adherent to one another, though not very firmly.

When gangrene has commenced, the colour is of a deeper cast, usually compared to a chocolate brown or port wine tint. In cases that are still farther advanced, there is a dirty grey and greenish appearance; and the coats of the bowel, which before were only thickened and but little softened, are now no longer able to resist the gentlest impression of the finger, for they give way as easily as paper that has been moistened in water.

Now no one will assert that these appearances are not sufficiently striking to be obvious to the most careless and unwilling observer; and I can venture to say, that no case has yet been known where strangulation of the intestine had produced death, and that these appearances were not found in the post-mortem examination. So invariably constant is their occurrence in such cases, that of themselves they are a sufficient and clear proof that the cause of death was the strangulation of the intestine, or at least had an important influence in the production of the fatal result. On the other hand, it must be observed, that when these appearances cannot be shown to exist, though even looked for more anxiously than usual, that it must be to some other cause that we ought to attribute the production of the symptoms and death of the patient. Indeed, were any one, *under such circumstances*, to assert that strangulation of the intestine had been the cause of the symptoms, and death, it would prove him to be equally regardless of truth, as destitute of character.

We are next to determine, whether a diagnosis can be established from a consideration of the symptoms, between strangulation of the intestine and peritonitis arising from other causes. This will be soon decided—for unless there is present an external tumor of the character previously described, the symptoms are frequently so exactly similar, that any attempt to establish a certain diagnosis must end in a failure, or succeed by chance. Thus the pain and tenderness of the belly, the vomiting, and all the other symptoms of strangulated intestine, are

very commonly observed in simple peritonitis, and many other affections. It must be, then, from a consideration of the history of the case, as well as of the symptoms, that the diagnosis, during life, ought to be founded.

One self-constituted authority has publicly declared *vomiting of bilious matters is to be held henceforth as sufficient proof of strangulated intestine*. But I need not surely attempt to refute a doctrine so absurd and utterly devoid of support. Suffice it to say, that I witnessed a case in the College Hospital of a strangulated hernia, which had existed for 23 hours, and in which the operation was obliged to be performed. The man was temporarily relieved, but sunk from peritonitis 41 hours after the performance of the operation. In the post-mortem examination of that case, the characteristic morbid changes in the intestine that had been strangulated were present to an unusual degree. At the time of the operation the bowel was extremely dark coloured, and the muscular and mucous coats were considered by Mr. Liston, who was present at the examination of the body, to have been divided by the tightness of the stricture in which it had been embraced. *And yet, gentlemen, that man did not vomit until after the operation had been performed*. In peritonitis arising from other causes, such as laceration of the bowels, wounds of the peritoneum, effusions of extraneous matters into the abdomen, &c., there are no special or particular symptoms which will serve as the foundation of a decided diagnosis. In such cases the surgeon must be guided, when delivering his opinion as to the cause of the symptoms, by the history of the case, particularly concerning the mode of its commencement and origin, and also, in no small degree, by his own experience, as afforded by previous cases which shall have occurred to him.

It is obvious that in such cases the surgeon has very little to offer in the way of operative proceeding; but that he must content himself with enjoining perfect rest, avoiding medicine, and regulating carefully the diet, making use of such other antiphlogistic measures as the symptoms may appear to him to demand. In such cases as these the patient almost always, if not invariably, dies from the peritonitis induced by the injuries he has sustained. He seldom lives more than 24 hours; frequently not twelve.

It will be remarked, that in these cases, when they are first seen by the surgeon, the patients are generally found in a state of extreme collapse and depression; but that if they survive a few hours, a transient reaction occurs, and which, I think, is with justice attributed to the commence-

ment of that peritoneal inflammation which will go on increasing until it produces the death of the sufferer.

After death, the real source of the symptoms will be readily ascertained; there is the increased vascular condition of the peritoneum, and which is accompanied by the same morbid changes as in simple peritonitis, but probably to a much greater extent and degree; the wound of the bowel, of the peritoneum, the presence of the extravasated matters, with their appropriate qualities, are now ascertained, for the first time, with certainty.

But there are none of these remarkable circumstances appearances that distinguish death caused by strangulation of the intestine.

Such, gentlemen, are the remarks which I have to offer to your notice upon the subject of the diagnosis between the symptoms of strangulated intestine, and of peritonitis arising from other causes; and I think you will consider me justified in deducing the following conclusions:—

1. That there is no difference in the rational symptoms of peritonitis, from constitutional and other causes, and of strangulated hernia, from which the diagnosis can be satisfactorily made, unless account be taken of the history of the case, or that there is an external tumor which is connected with the abdominal cavity, and which is firm, painful, and tender to the touch, and which resists the employment of the taxis.

2. That when a portion of intestine has become strangulated, and causes the death of the patient, we can have no difficulty in proving such to have been the case, from the morbid conditions which are invariably found to have been induced in the portion of the intestine that had been strangulated, and which conditions are so characteristic of their cause, as to admit of no doubt of the true nature of the case.

3. That when death has followed symptoms resembling those usually observed in cases of strangulated intestine, and those alterations in the colour and structure of the intestine, which have been described as characteristic of strangulation, are absent, we ought not to hesitate to refer the symptoms and death of the patient to some other cause, the effects of which will probably be sufficiently evident to enable us to ascertain its nature.

4. That an *incarcerated intestine* does not induce the same severity of the symptoms, nor is it accompanied by the same pathological changes, as strangulation of the intestine; and that the practice which ought to be pursued in the latter case is not demanded by the former.

5. That in cases of strangulated intestine, the only symptoms which can decide the surgeon to operate for the removal of the stricture is *the presence of an external*

tumor, originating from the abdomen, and which tumor is firm, tender under pressure, and the contents of which cannot be reduced into the abdomen by the taxis.

6. That if a case occur in which the other symptoms of strangulated intestine exist, yet if there is no external tumor, possessing the characters that have been described, to direct the surgeon where to perform his operation for the removal of the stricture, sufficient reason has been adduced that he is not called upon to interfere with his knife, but, on the contrary, were he to operate, he would expose himself to the just imputations of a culpable rashness and ignorance, or disregard, of the history of his art.

7. That in cases of wounds of the abdomen, accompanied by symptoms of effusion of extraneous matters, and lesions of the important viscera contained therein, where there is no external tumor which shall resist the taxis, the surgeon is warranted in referring the symptoms to such lesions, and to the peritonitis excited thereby; and that his treatment of the case, when guided by these principles, is rational, and consonant with the present state of our knowledge.

Such are the conclusions which I trust I am correct in deducing from the argument used; and I shall make only one practical application of them, and it is this:—

That if a case occur in which the abdomen shall have been wounded by a bayonet or other similar instrument, and that symptoms present themselves which may reasonably be attributed to extravasation into the abdomen of the contents of any of the organs contained within that cavity, and to the injury done to the peritoneum and intestine, and to the peritonitis consequent upon these injuries—if, moreover, there was no tumor apparent externally, which was tender to the touch, firm, and resisted the taxis when examined; and also, that after the death of the individual a post-mortem examination was conducted by competent authorities; and that this examination was both publicly and carefully instituted, and at which a wound of the intestine, copious extravasation of blood, and injury to the peritoneum in three different places, along with undeniable evidence of severe peritoneal inflammation, as evinced by the increased vascularity of the peritoneum, the effusion of coagulable lymph, and the tender cohesion of the folds of the intestines to one another, were found; and, above all, mark you, if a portion of intestine was found incarcerated in the bottom of the wound, but which did not present any of those limited changes of colour and structure which are pathognomonic of strangulated

intestine, then we can have no hesitation in declaring our conviction, that the death of the patient arose, not from strangulation of the intestine so incarcerated, but from the lesions done to the bowel and peritoneum, and to the presence of extravasated blood; and that these circumstances do fully, fairly, and without the possibility of contradiction, prove that the surgeon who treated such a case, without reference to strangulated intestine, to be free from the charges of ignorance and inattention that might be brought against him.

MR. S. COOPER AND MR. WAKLEY.

To the Editor of the Medical Gazette.

SIR,

A CORRESPONDENT of last week's *Lancet*, signing himself H. B., gives an incorrect statement of the proceedings of the meeting of medical students of University College, and would thereby lead his readers to draw an erroneous conclusion, and quite opposed to the real facts of the case, viz., that the students concurred in the justness of the strictures upon Mr. Cooper, whereas quite the contrary is the fact, and is sufficiently evidenced by what happened at and subsequently to that meeting, and which H. B. has taken sufficient care to suppress.

When the *Lancet* was denounced by one gentleman as a vile publication, doubtless referring to the low abuse for which it is pre-eminently distinguished, the concurrence of the students in this announcement was clearly indicated by the applause with which they greeted the speaker. Again, when Mr. Cooper entered his lecture-room subsequent to the meeting, the long-continued and enthusiastic cheers and acclamations of the students shewed plainly their feelings towards him, and contrasted strongly with the cordial and heartfelt manner in which three groans were proposed and given for his opponent.—I am, sir,

Your obedient servant,

A STUDENT OF UNIVERSITY COLLEGE.

Dec. 18, 1837.

THE CHARTER-HOUSE SQUARE
INFIRMARY.

To the Editor of the Medical Gazette.

SIR,

I READ this day, in the *Gazette* of the 8th ultimo, a paragraph, headed "Inde-

cent Mode of Advertising," which I presume would not have been inserted had you been aware that the management of the valuable charity, which you say "a surgeon in the City has contrived to establish," is intrusted to a committee of governors, as respectable and influential, to say the least, as that of any charitable institution in the metropolis. By the direction of this committee, the board in front of the premises, upon which is simply inscribed, "Infirmery for diseases of the Rectum, supported by voluntary contributions," was erected. The assertion, therefore, that "a surgeon in the city is annoying the inhabitants of the district, by refusing to remove the board, and even threatens to insert words which shall make it more offensive," must necessarily be, and in fact is, devoid of truth; the individual alluded to not having the power, even if he had the inclination, to carry any such threat into execution.

You surely, sir, cannot be aware of the custom that prevails in London and every other great city, when you assert that this manner of giving publicity to a charity "is an indecent mode of attracting attention," which you "never before heard of from any one who occupied the place of a regular practitioner"—yet those members of the profession, who gratuitously attend the various charitable institutions, the nature of which is made known "by the exposure of a board in front of the house," might, with equal justice, be accused of indecently advertising themselves. The fact is, the paragraph is altogether founded in error: I have, therefore, sent you these few lines, trusting to your impartiality for their insertion, and assured that you would not, knowingly, injure the character and talents of one of the most honourable and useful of its members.

I remain, sir,

Your obedient servant,

A GOVERNOR.

Dec. 2, 1837.

P. S. I inclose you my card for your private information, and I beg to add that though not practising, I am a licentiate of the Royal College of Surgeons, Ireland.

[Our correspondents object not to the erection of a board in front of the institution, but to the words upon that board, and in this objection we concur.—E. G.]

QUACKS AT TRURO.

To the Editor of the Medical Gazette.

SIR,

I beg leave to inclose, for your perusal, two hand-bills, issued by *Dr. Reynolds Fowler* and *Mr. M. Oliver*, two worthies recently settled in this town, as medical practitioners. These hand-bills have been industriously circulated throughout the town and neighbourhood, and the latter person has visited many patients and sent them medicines. Being desirous of knowing on what pretence these gentry had the impudence to proclaim themselves professors of the healing art, I called on them this morning, in company with another surgeon, when *Dr. Fowler* avowed that he had "no license nor diploma, but that he had been a practitioner since he was seven years of age! That the great body of those who had obtained licenses to practise were as ignorant as pigs, and that he defied either the authorities of the Hall or elsewhere, to prevent him from practising when and where he pleased. That he would hold a public disputation with any of the medical men in Truro on any subject, at three days, three weeks, or three months' notice." I called, also, on *Mr. Oliver*, in company with the same gentleman, and asked by what authority he had commenced practice in the town. He said he had been a practitioner before 1815; then said he gave advice gratuitously, and that he had never visited any patients; but that if persons came to him for advice he had a right to give them advice and medicines. On my intimating that I should inform the authorities at the Hall of his proceedings, he said, also, he did not care for them. Thus does quackery raise its unblushing front in the country; and the great practical question is, what power can be exerted to repress it? I leave this communication in your hands to use as you may see fit; and I shall feel much obliged if you will forward this letter to the proper officer of the Apothecaries' Company, to whom I shall be happy to forward any further details that may be necessary to enable them to deter such unworthy pretenders from deluding the public.—I remain, sir,

Your obedient servant,

S.

Truro, Dec. 12, 1837.

[The writer of the above gives his name. His letter, together with hand-bills alluded to, have been forwarded to the Secretary of the Society of Apothecaries.—*Ed. Gaz.*]

LIFE INSURANCE FEES.

To the Editor of the Medical Gazette.

SIR,

AGREEING most fully with you in the views expressed in the leading article of your journal of 18th instant, I trust you will allow me to suggest a mode by which your readers, who, I take it, constitute all the respectable practitioners of London, of all grades in the profession, may have an opportunity of expressing their determination respecting the point upon which we are at issue with insurance companies.

My proposition is this: let every physician, surgeon, and general practitioner, who is disposed to follow the example of the Leeds practitioners, send a note to the Editor of the MEDICAL GAZETTE, expressing his determination not to give an opinion to an insurance company, unless a fee be sent by the company along with the letter of consultation. In this way you, sir, will be enabled to form a list of those gentlemen who may have professional and public spirit enough to come to such a resolution; and such a list, if published in your pages, will soon find its way into the columns of the newspapers, and will prove to the public and to insurance companies that some union does exist among medical men.

For my own part, I cannot allow myself to doubt that, if you would announce your willingness to assent to such an arrangement, and to afford the necessary facility for it, a considerable number of our most influential men would come forward, and thus probably the question might be settled.—I am, sir,

Your obedient servant,

A PHYSICIAN.

November 25, 1837.

[If a considerable number were to send their names, we should readily publish them; but if otherwise, we should think it unfair towards the parties to do so.—*ED. GAZ.*]

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, Dec. 21, 1837.

Joe Poppleton, Almondbury, Yorkshire. — Henry John Watkinson, High Town, near Leeds. — Charles Alexander Holcombe, Cosheston, Pembroke, South Wales. — Robert Cross, Wisbeach, Cambridgeshire. — Frederick William Mackenzie. — Thos. Ashell Henderson. — Jas. Boulton, Ponty Pool. — Wm. Joseph Lunn, Hull, Yorkshire. — W.

Hughes, Cheltenham. — Martin Mauger, Guernsey. — Joseph Warburton, Pateley Bridge. — Alfred Keyser. — Thomas Stringer Hatfull. — Frederick Maxwell Dinwoodie, Appleby, Westmoreland.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, Dec. 19, 1837.

Age and Debility . . . 21	Hooping Cough . . . 10
Apoplexy . . . 3	Inflammation . . . 15
Asthma . . . 7	Bowels & Stomach . . . 4
Consumption . . . 26	Brain . . . 6
Convulsions . . . 20	Lungs and Pleura . . . 4
Croup . . . 1	Jaundice . . . 1
Dentition or Teething . . . 5	Measles . . . 2
Dropsy . . . 4	Mortification . . . 1
Dropsy in the Brain . . . 3	Small-pox . . . 3
Dropsy in the Chest . . . 1	Thrush . . . 1
Fever . . . 12	Tumor . . . 1
Fever, Scarlet . . . 2	Unknown Causes . . . 22
Fever, Typhus . . . 4	
Gout . . . 1	Casualties . . . 1

METEOROLOGICAL JOURNAL.

*Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.*

Dec.	THERMOMETER.		BAROMETER.	
	from	27 to 37	29.75 to 29.72	
Thursday . . . 7	27	40	29.63	29.61
Friday . . . 8	29	39	29.58	29.74
Saturday . . . 9	27	39	29.92	29.97
Sunday . . . 10	32	39	29.93	30.02
Monday . . . 11	26	39	29.92	29.85
Tuesday . . . 12	30	43	29.88	30.08
Wednesday 13				

Prevailing wind, N.E.

Except the 10th and 13th, generally cloudy, with rain at times; a little snow on the morning of the 7th.

Rain fallen, .25 of an inch.

	from	27 to 41	30.13 to 30.15	
Thursday . . . 14	28	39	30.08	29.95
Friday . . . 15	25	45	29.84	29.73
Saturday . . . 16	41	51	29.73	29.69
Sunday . . . 17	43	55	29.34	29.45
Monday . . . 18	40	50	29.84	29.72
Tuesday . . . 19	38	56	29.57	29.30
Wednesday 20				

Winds, S.E. and S.W.

Except the 14th and two following mornings, cloudy, with frequent and heavy showers of rain. Wind very boisterous on the 20th. Remarkably mild during the last four days.

Rain fallen, .975 of an inch.

CHARLES HENRY ADAMS.

NOTICES.

The papers of Sir David Brewster and Mr. Whewell, next week.

We hope to be able to give the first part of Mr. B. Curling's paper, on the Entozoa, with engravings, in our next.

We shall be happy to receive the case alluded to by Dr. Watson, at the conclusion of the paper we published last week.

WILSON & SON, Printers, 57, Skinner-st., London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF
Medicine and the Collateral Sciences.

SATURDAY, DECEMBER 30, 1837.

LECTURES ON THE PHYSIOLOGY AND DISEASES OF THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL
AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of
1836 and 1837,

At the Anatomical School, Kinnerton-Street,
near St George's Hospital,

By CHARLES J. B. WILLIAMS, M.D. F.R.S.

LECTURE X.

Examination of the Chest through the Vital Properties or Functions of its Organs. — Analysis of the General Symptoms (continued). — Expectoration: Explanation of the Act; Means of promoting it. — The Matter of Expectoration; its Nature and Value as a Sign. — Morbid Kinds: Mucous; Albuminous; Watery; Compound Varieties. — Signs derived from the Form and Weight of Sputa. — Pain: its Nature and Value as a Sign; its Varieties in the Chest, and their Causes. — Modes of testing the Sensibilities of the Chest.

THE expectoration is another symptom of thoracic disease, which must be considered as the result of vital as well as physical properties, and therefore I include it under the head of general diagnosis, although it sometimes approaches in character to a physical sign. Now you know that the word expectoration strictly means the act of expelling any thing from the chest; but by a figure of speech, it is also applied to the matter so expelled. We shall find that both the act and the matter of expectoration may present us signs of the condition of the pectoral organs.

If you consider the structure of the bronchial tree, you may perceive that natural breathing tends to prevent the accumulation of matters in its tubes, in spite of gravitation. The area of the smaller divisions of the bronchi is considerably greater than that of their trunks; and it may be represented as the divided base of a hollow cone or funnel, which is concentrated in the trunks, and lastly in the windpipe. Now the air, in the more sudden act of expiration, passes with greater rapidity and force as it converges into these trunks, and therefore tends to carry through them any superfluous matter that may be present on the bronchial surface. This will explain how the finer bronchial tubes of the most dependent parts of the lung are, in health, kept clear of any accumulation. Possibly the ciliary motions of the mucous membrane may, as MM. Purkinje and Valentini have surmised, tend to the same effect.

But it is the forcible acts of special expectoration, *hawking*, and coughing, that tend most effectually to clear the air-passages; and they do this by both increasing the force and fulness of the expiratory effort, and at the same time contracting the upper tubes and trachea, so that the air acts with greater force on any superfluous matter in them. The repeated closure of the glottis in coughing increases the expulsive effort by letting out the air in successive sudden jerks, which are more forcible than any continued act of expiration would be. We see this exemplified in cases where the operation of bronchotomy has been performed. The patient often cannot expectorate effectually as long as air can pass out from the artificial opening, and he is in danger of suffocation in consequence; but on closing this during the act of coughing, the force of the air can be directed in the natural way against the accumulated matter. By attention to this particular, suffocation

has been averted in more than one instance after this operation. In certain diseased conditions of the larynx, the patient cannot close the glottis; and hence also expectoration may be difficult, and the cough assumes a continuous unguidable character, which we might have added as another variety to those enumerated in the last lecture. This is what M. Trousseau calls a *belching* cough.

There is another element essential to the proper performance of the act of expectoration, the capacity to make such a full inspiration as shall carry the air in beyond the accumulating matter, so that it may on its forcible passage out again carry this matter before it. Hence you see why weakness, which prevents a sufficient inspiratory effort, or obstruction of the terminal and most expansible parts of the air-tubes, which renders this effort ineffectual, may stop the act of expectoration, and by permitting the accumulation of matter in the air-tubes, may speedily conduce to a fatal result. Inability to expectorate is the immediate cause of death in a great many cases of various diseases; in fact, it is a part of the article of death itself; and when you hear the rattle in the throat of the dying, you hear the sign of the accumulating barrier which is shutting out the breath of life. Sometimes, even at this stage, there are sensibilities enough in the system to feel the force of a stimulant which may excite the sinking powers to another struggle; expectoration is once more accomplished, and breath once more renewed; and where there is no irrecoverable alteration of structure, this act of expectoration may in some few instances turn the balance in favour of recovery. I need not tell you, then, how important it is to study the act of expectoration, and to acquaint ourselves with those means that may excite or promote it. I dare say that every one of you has seen an instance in which a patient has been snatched from the jaws of death by the timely administration of a diffusible stimulus, such as a warm aromatic draught, with carbonate of ammonia, together with such a change of posture and other circumstances as might most favour the expulsion of the matter that was suffocating him. A great deal may often be done in less urgent cases by attention to the posture of the patient. In most instances the act of expectoration is easiest in that posture in which the respiration is most free, which is commonly the semi-erect posture, but I have known some patients expectorate more freely when lying on one side; and I remember a phthisical patient who really appeared to be several times saved from suffocation by alternating his posture from lying down to sitting up in a

particular manner, suggested by a knowledge of the condition of the lungs in that case. When this expedient was neglected, the patient was so shaken with frightful fits of fruitless cough, and so oppressed with the accumulating matter, which they could not expel, that speedy suffocation seemed inevitable. In some cases, the act of expectoration may be favoured by another kind of action, in which the expiratory muscles are concerned, that of vomiting; and we shall find hereafter, that some emetics may exert an influence of an important nature on the bronchial tubes, besides this mechanical one.

The *matter of expectoration* frequently furnishes us with very instructive signs. It is the product of diseased action, and in its physical or chemical qualities it may inform us somewhat of the nature of that action, of the condition, and sometimes of the position, of the parts from which it comes. As, however, we have seen that the effort of expectoration is sometimes unsuccessful, so here you will find no matter of expectoration to judge of; and besides this, most children and some adults cannot spit out what they expectorate, but swallow it.

The basis of expectoration generally is the secretion of the mucous membrane of the air-tubes. This is naturally a transparent, colourless, slightly glutinous liquid, like thin mucilage. The chief animal matter which it contains is that called mucus, which seems to be a sort of imperfectly coagulated albumen; and the varieties of sputa presented by disease commonly depend on an unnatural condition or quantity of this animal matter. There is also saline matter, which may vary in quantity, and so may the proportion of water. Unfortunately, we know very little of the chemical differences in the various kinds of sputa; but their physical qualities have been sufficiently studied, to enable us to connect some of them with particular pathological conditions in a way that proves useful in diagnosis and practice.

The difference between mucus and albumen seems to consist in their physical condition, rather than in their chemical constitution. Mucus is a transparent glutinous matter, not coagulable by heat, as liquid albumen is, and not solid and opaque like coagulated albumen; but on ultimate analysis it is not found to differ from this principle. When, therefore, we see expectorated matter opaque and solid, or liquid and coagulable by heat, it loses the only distinguishing characters of mucus and is strictly albuminous*. For this

* Since these lectures were delivered, an excellent account of the chemical characters of different kinds of expectoration was read at the meeting of the British Association at Liverpool

reason, I propose the following general classification of expectorated matter:—

The matter of expectoration may be generally divided into the *mucous*, the *albuminous*, the *watery*, and the *compound* kinds.

1. *Mucous* expectoration is that most like the natural secretion, being transparent, colourless, and more or less viscid. It is the general result of simple acute inflammation of the mucous lining of the air-tubes, in which case it is increased in quantity, and particularly in viscosity; in fact, the glutinous character of the sputa, and the tenacity with which they stick together, and to the containing vessel, or fall out in a ropy mass, was described by Andral, and I think correctly, to be a mark and, in some degree, a measure of acute bronchitis. In the most intense forms of inflammation, and where the disease occupies the finer tubes, to the glutinous character of the mucus is added a frothiness, arising from the mixture of those air-bubbles in the tubes, which in their breaking cause the mucous and sub-mucous rhonchi. But the most intense bronchitis is that accompanying inflammation of the parenchyma; here you have the most viscid form of sputum, through which air driven produces the crepitant rhonchus; and the blood in the distended vessels of the engorged parenchyma communicating a little colouring matter to it, gives it that reddish or rusty tinge which is so characteristic of the sputa of peripneumony. The transparent or semi-transparent condition of these viscid sputa distinguishes them from the albuminous kind, into which, however, they pass in the advanced stages of all the more inflammatory affections of the bronchial membrane. The mucous expectoration has commonly a saltish taste, and with its saline matter is probably connected its irritating quality, so marked in the early stage of bronchitis.

2. The varieties of *albuminous* expectoration are pretty numerous, for I comprehend under this head the opaque kinds of sputa which have no remarkable viscosity, such as the purulent expectoration of chronic bronchitis, the fibrinous or polypous sputa of plastic bronchitis, and the more compound combinations of these with caseous and other matters, which are voided in the advanced stages of pulmonary phthisis. This class of sputa denotes an error of secretion, further than the mucous from the natural standard; but their production generally announces a decline

of inflammation from its most acute form. Probably, the very throwing off of so considerable a mass of animal matter is the means of relieving to a certain extent the inflamed vessels; for we frequently find the purulent or polypous expectoration in intense bronchitis attended by a remarkable diminution in the signs of local and general excitement*. But such an expectoration ceases to be a favourable sign when it *continues*, either with undiminished irritation, or with proofs of general weakness; for then a change is implied, either in the structure, or in the habitual action of the membrane, which, secreting pus instead of mucus, goes beyond the mere removal of a temporary congestion, and proves of itself a cause of irritation and exhaustion. I am not sure that I make myself intelligible, but as we shall revert to this subject in connexion with the pathology of bronchitis, I must dismiss it at present. I dare say you have heard much about the modes of distinguishing pus from mucus in the expectoration. On these formerly the diagnosis of pulmonary phthisis was supposed to depend. We now hold all these tests very cheap, not only because it is well known that pus may be produced without any ulceration or consumption of the lung, but also because these distinctions cannot be complete between matters that pass by insensible gradations into each other.

3. *Watery* expectoration is that kind in which a liquid of only slightly glutinous quality is coughed up in greater or less abundance. This appears to contain very little animal matter, and to be rather a diluted mucus than to have in it any thing peculiar. It is often covered with a froth, particularly when it is coughed up with much effort. This secretion I regard as the result of irritation, with a relaxed state of the vessels, rather than of inflammation; but it may occur as a consequence of this lesion, as well as of congestion or obstruction to the circulation of the blood in the lungs. It is the expectoration of what is called humid asthma and pituitous catarrh. Some persons of a relaxed habit have it during a common cold, or any form of bronchial inflammation. It sometimes tastes more salt than usual, and in this case it commonly causes a more incessant teasing cough.

4. Under the head *compound* expectoration, I class various combinations of the preceding kinds, which we meet with in almost every form of pectoral disease. They are either products of different parts, in distinct pathological conditions, al-

by Mr. Brett. He has found that the chief character of the sputa of bronchitis, pneumonia, and phthisis, lies in the presence of a variable quantity of albuminous matter, liquid or solid, which does not exist in natural bronchial mucus.

* A high authority on thoracic pathology, Dr. Stokes, in his late excellent work, takes a similar view of this subject.

though coughed up at the same time; or they may in some cases proceed from the same part in an intermediate pathological state, and capable of secreting different kinds of matter. An example of the latter is the opaque or muco-purulent expectoration of the latter stages of bronchitis, in which the opacity and colour of albuminous matter is apparent, whilst it is held together by a mucus of some tenacity. In the *concocted* sputa of declining acute bronchitis, the mucus predominates; whilst the loose albuminous matter is more abundant where the inflammation tends to pass into a chronic state. The sputa of chronic bronchitis, and in fact of most chronic diseases of the lungs and air-tubes, are almost always more or less mixed; for it generally happens that the different parts of the membranes and tissues are variously affected; and when, as in the advancing stages of phthisis, there is structural lesion or destruction of parts, there is the greater reason for a more heterogeneous kind of expectoration. In these cases, however, the albuminous kinds mostly predominate, in the form of muco-purulent, purulent, caseous, or tuberculous matter, and coagulable or fibrinous lymph, occasionally tinged or mixed with the colouring matter of the blood; these constitute the bulk of the expectoration of the consumptive. In catarrhal diseases of a chronic kind, we very commonly see very opposite forms of sputa expectorated together. Thus in a spitting-dish full of thin frothy watery expectoration, you will often find portions of tough and almost solid semi-transparent mucus, as if some parts of the tubes were throwing off the water, and others the animal matter, in a separate form. When we come to the subject of catarrh, we shall find that these opposite products do not imply an equally opposite pathological condition. After hæmoptysis, it is very common to see fibrinous concretions, together with purulent and mucous matter, all more or less tinged with blood. In other affections, it is not uncommon to see the sputa streaked with blood; and this sign is of less importance when the cough is violent, because it may then merely proceed from a slight abrasion caused by the force of this mechanical action. When, however, there is often blood present, without much force of cough, and especially if there be pus with it, we may suspect the presence of ulceration in some part of the air passages. The colouring matter of the blood, in an altered state, may also be combined with other forms of sputa. Thus, in scorbutic persons affected with humid catarrh, or bronchitis, the expectoration is a thin reddish-brown

liquid, like prune juice, or diluted treacle; and in the last stages of pulmonary disease, the colouring matter, from the final pulmonary congestion which precedes death, is seen in the dirty reddish-brown or greenish tinge of the purilagenous sputa.

You see, then, that the matter of expectoration will often inform us of the pathological condition of the lungs and their tubes; and its quantity or quality may sometimes suggest proper remedies. In some cases you may learn other things from it. Thus, when in consumption, tubercular matter, with portions of pulmonary tissue, are expectorated, the conclusion is obvious. You will also sometimes see the expectoration present physical signs of the state of the interior by its containing albuminous or compound matter, moulded into the shape of the tubes or cavities from which it comes. The large rounded flocculent muco-purulent sputa of advanced phthisis, are often such as could only accumulate in a cavity; and the tubular or vermicular albuminous matter which is coughed up in the plastic kind of bronchitis, sufficiently explains whence it comes, by its being an exact mould or cast of the bronchial tubes, sometimes in an arborescent form, from several of their divisions.

I must not omit to notice a test, which is erroneously used to determine the nature and source of sputa, whether they float or sink in water. Now the floating of a sputum merely depends on the number of air-bubbles retained in it, and although pus alone, or tuberculous matter alone, will not retain these bubbles, yet a small addition of tenacious mucus will enable them to do so. Again, although the sputa formed in ulcerous cavities are less likely to contain air and to float than those formed in the tubes, yet we not unfrequently find the concocted expectoration of acute bronchitis, which is formed exclusively in the large tubes, sink in water; whilst the mixed product of a vomica and the adjoining tubes, which has been churned together with air, floats. This *hydrostatic* test of expectoration is then a very inconclusive one; but it may be useful in sometimes causing a rough separation of the albuminous matters from those of a more viscid mucous kind.

I might occupy you with other details regarding the matter of expectoration; but I have said enough to illustrate how it may prove useful in diagnosis and practice. You will now, I think, perceive how much more valuable its indications may become when conjoined with the physical signs, by which you may often

detect the position, and measure the amount of the local disease, of which the matter expectorated is the product. We shall find many exemplifications of this position hereafter.

The only other morbid phenomenon connected specially with the modified vital properties of the organs of respiration is *pain*. Now you know that pain may arise either from an excessive impression on the nerves of sensation, or from an excessive sensibility of these nerves, to which common circumstances of position, motion, &c. then become painful. The latter is the more common cause of pain in internal diseases, but it is not unusual to find them combined, as when a tumor, or effused matter, presses on or stretches parts morbidly sensible. The most common causes of pain are inflammation, and those kinds of vascular excitement that are allied to it; this vascular excitement is generally attended, in the first instance at least, with an exaltation of the nervous function. But the nervous function may be *primarily* excited; and although the increased sensibility thus produced seldom lasts long without more or less stirring up the function of the vessels also, yet we may for a time have pains purely nervous, such as pleurodyne and pectoral neuralgia. Further, as inflammation is not the only cause of pain, so the pain present in inflammation is by no means an index of the extent of the inflammation, nor even of its situation. Most extensive inflammations have been known to occur, not only in the parenchyma of the lungs, but in the bronchial membrane and pleura also, without producing any pain; and it frequently happens in phthisis, that the pains chiefly complained of are low down in the sides, when the disease is almost entirely in the upper lobes of the lungs. So likewise in bronchitis and pneumonia, the pain is often confined to the sternal, lateral, or scapular regions, whilst the disease occupies other parts.

There are, nevertheless, some general characters with regard to pain, which may render it useful as a symptom of disease of the chest. It is commonly remarked, that the pain of parenchymatous and bronchial inflammations is dull and diffused, whilst that of inflammation involving the serous and fibrous membranes of the pleura and pericardium is of a sharp lancinating character. This is generally but not constantly true; and we may find it explained by the circumstance that the par vagum, which supplies the bronchi and lungs, is by no means so sensitive a nerve as the spinal intercostals, which are distributed on the pleura and pericardium.

For this reason, too, there is more apt to be acute pain when the costal pleura, or the coverings of the great vessels, are inflamed, than when the pulmonary pleura is the chief seat of disease. Observe a further distinction in the variations of these various kinds of pain, and you will be confirmed in the opinion that they belong to different orders of sensibility. The dull, heavy, or aching pain of bronchitis, or pneumonia, is generally pretty constant, although it is increased by full inspiration, exertion, or the breathing of cold air; yet even then it gives the feeling of soreness under the sternum, rather than of severe pain. It often resembles the pain of dyspepsia, which is probably seated in a branch of the same nerve, and is also usually referred to the sternum. The degrees of pleuritic pain, on the other hand, are sudden, extreme, and intolerable. If it be not felt in ordinary breathing, a long breath, or a cough, just sufficient to bring the membranes to a requisite degree of tension, causes that sharp stitch of the side—that sudden catch of the breath, that has been considered so characteristic of pleurisy. When it is constant, the patient is obliged to hold his side to diminish its severity, by restraining the motions; and thus placed in opposition to the sensation which prompts the act of respiration, this sharp pain may cause such a voluntary restraint of these acts as to bring the patient to the verge of asphyxia. It is under these circumstances that the breathing becomes partial, as I formerly described to you; and patients whom pain constrains to breathe only with the diaphragm, or with one side, will perform this supplementary respiration so well, that they are completely free from pain, although the inflammation is as acute and the membranes as tender as ever. Sometimes you may detect the latent tenderness by pressing between the ribs of the affected side; but you will be more likely to succeed if you restrain the supplementary respiration by pressing on the abdomen, or on the healthy side, and then desire the patient to cough, or to take a sudden long breath. If there be any exalted sensibility, or tenderness, it is pretty sure to be discovered by this means; and I have several times met with patients who denied having any uneasiness, or tenderness, yet they winced at the pain developed in this way.

As a general rule (not, however, without some exceptions), we may consider a fixed permanent pain, or a permanent tenderness, which you see depends on the same pathological cause, an indication of inflammation, or congestion, or some analogous condition of the vessels; and when present it deserves attention, not only as an object of treatment on its own

account, but also as an index, which, together with the pulse, cough, fever, and other general symptoms, shows the increase and diminution of the complaint, and the effects of remedies, even before these become manifest from the physical signs. Still, if we trust to it alone, it will negatively deceive us in those numerous cases of extensive disease in which it is absent, or scarcely complained of; and it will positively deceive us in those cases in which modified nervous sensibility, a mere neurosis, is the only or the chief disorder.

LECTURES

ON THE

ENTOZOA,

OR

INTERNAL PARASITES OF THE HUMAN BODY,

Being Part of a Course on Morbid Anatomy, delivered at the London Hospital,

By T. B. CURLING, Esq.

THE researches of naturalists have shewn that most species of animals are infested with various beings endowed with distinct vital properties or organizations—creatures that live and propagate their kind at the expense, and in the interior of other animals, which afford them a habitation, nutriment, and heat. And certainly, amongst the numerous accidental formations developed in the human body, there are none which merit more the attention of the physician and of the physiologist, than these organized but independent productions. They have been called, from the situation in which they are found, *Entozoa* (from *entos* within, and *ζῶον* animal), a name given to them by Rudolphi of Berlin, who has particularly investigated their organization. These animals, which have attracted but little attention in this country, are developed in various situations of the body, both in parts having communication with the external surface, as the alimentary canal, and in the interior of the best protected organs, as in the substance of the brain, liver, and muscle. It is necessary that the *Entozoa* should be distinguished from the *sporicius worms*; that is to say, from those living beings that are also found in the interior of animals, but which have evidently been introduced there from without. Thus, insects or other animals sometimes work their way into the skin, or become lodged in the nasal sinuses, and in the stomach or intestinal canal. At certain periods of

the year, the internal surface of the splenic portion of the stomach of the horse is often found covered with a number of small worms, commonly called *botts*, which adhere very closely to the mucous membrane, and cannot be detached without violence. Now these animals do not originate in the stomach, but the eggs from which they are produced being deposited about the shoulders and knees of the horse, come within reach of the tongue, and by this means are introduced into the body, where, after undergoing their first transformation, and becoming changed into larva, they leave the stomach, pass into the intestines, and quitting the body along with the fæces, undergo a further change in the open air from the form of a worm to that of the fly known to naturalists by the name of *æstrus communis*.

Entozoa have been found in nearly all classes of animals. Thus they have been discovered in birds, reptiles, and fishes, as well as in the mammalia. The invertebrata have likewise their entozoa, for it has been ascertained that they exist in the various species of insects. But what is still more wonderful, an immense concourse of minute parasites have been found infesting a small entozoon, the abode of which is the aqueous humour of the eye! In these lectures, however, I shall treat only of those entozoa which are found in the human body, and as many of them will no doubt come under your notice in practice, it is desirable that you should be able both to recognise them, and to treat the disorders to which they give rise.

The entozoa are found either upon free surface, in natural cavities, or in the parenchyma or substance of organs. The different species have their own peculiar localities. Thus some, as the *Ascaris lumbricoides* and the *Tænia*, are never found in any other situation but the alimentary canal, whereas the *Strongylus gigas* resides in the urinary passages. So likewise, the *Trichina spiralis* is only met with between the muscular fibres, and the *Filaria* in the cellular tissue. Their organization is also very variable. There are some Entozoa that consist of nothing else but a parenchymatous mass without any appearance of organs, whose rude and imperfect organization only entitles them to be placed in the lowest rank in the scale of animated beings; whilst we find others, complicated in structure, possessing an evident muscular system, a complete alimentary canal and well-developed organs of generation, with distinct sexes, rudiments of a circulation, and even a nervous system.

The Entozoa have been classed by naturalists according either to their habitation, their form, or their structure. Rudolphi's classification, which is perhaps

the best known and the most esteemed, is founded principally upon their organization. It is, however, objectionable in several particulars, and requires modification in the present more advanced state of zoological science. I shall follow, therefore, a more comprehensive arrangement, which has recently been proposed by Mr. Owen, the accomplished Professor of the College of Surgeons, who, in addition to many highly valuable contributions to the science of comparative anatomy, has done more than any other anatomist in this country to ex-

tend our knowledge of the interesting class of animals now under consideration; and I may refer those who desire a fuller account of their organization than the limits of these lectures admit of, to his able article on the Entozoa, contained in the Cyclopædia of Anatomy and Physiology, to which I must acknowledge myself indebted for many of the particulars that I shall bring before you.

Mr. Owen classes the internal parasites of the human body according to their organization, as follows:—

ENTOZOA HOMINIS.

1. PSYCHODIARIA, from <i>ψυχη</i> , life, and <i>ειδος</i> , like.	1. Acephalocystis endogena, found in.....	Brain. Liver. Heart. Spleen. Kidneys, &c. &c.
	2. Echinococcus.....	Brain. Liver. Spleen. Omentum.
2. PROTELMINTHA, or early form of the Entozoa, from <i>πρωτος</i> , first, and <i>ελμινς</i> , worm.	3. Cercaria seminis	Male semen.
	4. Trichina spiralis	Voluntary muscles.
3. STERELMINTHA, or solid worms, from <i>στερεος</i> , <i>solid</i> , and <i>ελμινς</i> , <i>worm</i> .	5. Cysticercus cellulosæ	Muscle. Brain. Eye.
	6. Tænia solium, or common tape-worm	Small intestines.
	7. Bothriocephalus latus	Small intestines.
	8. Polystoma pinguecola	Adipose tumor connected with the ovary.
	9. Distoma hepaticum, or liver flake	Gall bladder and biliary ducts.
4. CÆLELMINTHA, or hollow worms, from <i>κοιλος</i> , hol- low, and <i>ελμινς</i> , <i>worm</i> .	10. Filaria medinensis, dracun- culus, or guinea-worm	Subcutaneous cellular tissue.
	11. Filaria oculi	The liquor Morgagni of the eye.
	12. Filaria bronchialis.....	Bronchial tubes.
	13. Tricocephalus dispar.....	Large intestines.
	14. Spiroptera hominis	Urinary bladder.
	15. Strongylus gigas	Kidney.
	16. Ascaris lumbricoides	Small intestines.
	17. Ascaris vermicularis, or common thread-worm	Rectum.

Were I to include the numerous Entozoa infesting the animal kingdom, this list might be greatly extended. The various species of internal parasitic animals is, indeed, very considerable, and their study constitutes an important branch of zoology.

I shall not occupy your time by pointing out particularly in what respects this classification differs from the arrangements of

Rudolphi and Cuvier. It is sufficient to remark, that Rudolphi distinguishes them after their external form, whereas Cuvier has classified them after their structure, into the *hollow worms* and *parenchymatous worms*. Linnæus divided the Entozoa according to their seat, into those developed in the cavities communicating with the external air, the *intestinal worms*, and into those developed in the interstitial struc-

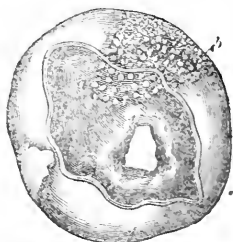
ture of organs, the visceral worms,—an arrangement preferred by Cruveilhier on account of its practical nature, though less scientific than the preceding.

You will perceive that I have enumerated seventeen distinct species, many of which, it will be shewn, differ widely from each other in structure and organization. In fact, these animals are not grouped under one head in consequence of any common, exclusive, or distinguishing characters, but are associated together chiefly on account of the nature of their local habitation—the interior of other animals. Many are so common, that perhaps there is scarcely an individual present who has not met with them; some are less frequent, and are indigenous in particular districts or countries; whilst others are so rare, that they have only been detected once.

1. *Acephalocysts*,

(from *ακεφαλή*, without a head, and *κύστις*, a bladder), sometimes called *dead hydatids*. These consist of isolated albuminous cysts, of a spherical form, found in various situations of the body, floating free and independent in a fluid contained in another cyst, connected with the organ or part in which they are developed. Their structure is exceedingly simple, consisting merely of a thin delicate membranous cyst, filled with a transparent colourless serum. The membrane varies in thickness, is smooth externally, whilst its internal surface is generally studded with minute white bodies, which give it a granular character. It is of the consistence of half boiled white of egg, of a semi-transparent pearly appearance, and tears readily and equally in every direction. In the larger specimens the albuminous membrane can be separated into two or more layers. Mr. Owen states that the phenomenon of endosmose is readily seen on placing the recent acephalocyst in a coloured liquid, little streams of which are gradually transmitted, and mingle with the fluid of the parasite. The fluid contained in these hydatids is remarkable on account of its perfect transparency and limpid nature. It consists almost entirely of water, with a very minute quantity of albumen, and does not coagulate on the application of heat or by the action of alcohol. I have observed that when the secretion around the acephalocyst is discoloured, turbid, or purulent, the fluid within still preserves its limpid character. As the acephalocysts present no appearance of organization, are impassive under the action of different kinds of stimuli, and manifest no contractile power, except such as evidently results from elas-

ticity, as, in short, they neither feel nor move, it is a question with naturalists whether they are entitled to be ranked even in the lowest class of organized beings. Rudolphi and Cuvier have indeed rejected them from the class of Entozoa. Laennec, who has given an excellent account of the acephalocysts, founded his opinion of their animal nature chiefly on the mode in which he observed their reproduction to take place. Thus, a number of small bodies of a grey or white colour, more or less regularly rounded, are developed between the laminae of the cyst, and subsequently being detached from the internal surface, enlarge and become separate hydatids. These vesicles formed in the parietes of the cyst may be observed of various sizes before they are cast off from the parent animal, being sometimes so minute as only to be perceived by means of the microscope, at other times exceeding a line in diameter. It often happens that a large acephalocyst includes several smaller ones, one within the other, all similar in structure. We may even find four or five thus included, like one box contained in another. It was owing to this arrangement that Hunter called them *pill-box hydatids*. It is explained from the circumstance of the germ or minute acephalocyst being detached from the internal surface of the cyst where they increase, and in the same manner propagate their kind, so that the successive generations produce the appearance which I have described. In the acephalocysts found in the ox and other ruminant animals, the vesicle is detached from the external surface of the parent, whence the two species have been distinguished; that of the human subject by the term *Acephalocystis endogena*, and the other by the term *A. exogena*. As, then, the acephalocyst



Acephalocystis endogena.

possesses the property of reproducing its species, and furthermore, grows by intrinsic power of imbibition, we must, with Mr. Owen, consider it, though at the bottom of the scale of organization, "still not the less as an independent organized species, sharing its place of development

and sphere of existence in common with the rest of the entozoa."

Cruveilhier, who has written an elaborate article on *Acephalocysts*, in the "Dictionnaire de Médecine et de Chirurgie Pratiques," distinguishes them, independently of their situation and connexion, into the solitary (*acephalocystis eivenita vel sterilis*) and multiple (*acephalocystis socialis vel prolifera*;) a distinction of some practical importance. The first kind is the most frequent in animals. It is rarely developed singly, several being generally produced in the same organ or in different parts of the body. The multiple, on the other hand, which occurs most commonly in man, rarely co-exists in several organs, or in several parts of the same organ.

Acephalocysts, unless loose in a serous cavity, are always inclosed in an adventitious cyst, which separates them from the surrounding parts. The outer surface of this containing cyst is adherent; the internal or free surface is generally rough and irregular, and is sometimes lined by a semi-transparent albuminous membrane. When there are several separate parasites within, the cyst contains a serous fluid; but when the *acephalocyst* is solitary, it often fills the cyst, lining, as it were, its internal surface, but always remaining distinct and unattached. These adventitious cysts are composed of the condensed cellular tissue of the organs in which they are found, and they must be viewed as belonging rather to the part in which the *acephalocysts* sojourn, than to the animals themselves, for they are the result of the irritation excited by the presence of the *hydatids*, and in this respect closely resemble the formation of gall-nuts. These cysts are liable to undergo changes analogous to those experienced by serous cysts in general. They may be thickened and fibrous, and rendered even cartilaginous or bony. I have seen them putting on exactly the appearance of the interior of a large aneurismal sac. They are liable, too, to become inflamed, when, instead of a serous fluid, they may contain pus, which sometimes so acts upon the *acephalocysts* as to destroy or rupture them, leaving only some scattered fragments floating in the turbid secretion. Andral states, that he once found the *débris* of membranes of *acephalocysts* floating thus in the midst of a vast abscess in the neighbourhood of the kidney.

Acephalocysts are developed in the liver more frequently than in any other organ. They have been found, however, in the brain, heart, lungs, mamma, spleen, kidneys, ovaries, lachrymal gland, thyroid gland, tonsils, between muscles, and in the interior of bones; in fact, in almost every organ and part of the body, except the

intestinal canal. As they increase in size, they occasion a corresponding atrophy of the surrounding parts. The most dense and unyielding structures are unable to resist their progress. Thus there is a case on record, in which they had produced a hole through the body of the scapula. They sometimes work their way to the surface of the skin, or reach some mucous surface, whence they are discharged. There are cases known in which they have been formed in the liver, and subsequently expectorated from the lungs. Authors have related cases where individuals have, at various times, coughed up many hundreds of them; and we have a preparation in the museum, taken from a woman, who, for some time before death, gave exit to *hydatids* produced in the liver, both at a wound on the right side of the abdomen and by expectoration from the lungs. They have been known, when originating in the kidneys, to be voided in fragments with the urine; and in other instances they have been ejected from the stomach, or have passed by stool. A less fortunate result has been the sudden rupture of the cyst, either spontaneously or by violence, and the escape of the *hydatids* into a large serous cavity, where they have produced active inflammatory action, terminating in death. The ancients were aware of this, and held the opinion that the rupture of *hydatids* was the sole cause of dropsy; a view which it is somewhat surprising to find received the sanction of so accurate an observer as Morgagni.

Although *acephalocysts* have a tendency gradually to increase, the tumor which they occasion sometimes disappears without any opening being afforded for their escape. This would appear to be owing to the destruction of the animal. Dr. Bright has shown that the offspring of the parent *hydatid* are often so numerous, and increase so rapidly, that they not only press upon and destroy each other, but sometimes rupture the parent cyst. The *acephalocyst* may also be ruptured accidentally, or it may be destroyed by inflammation and suppuration of the containing cyst. It is probable, too, that in the liver they are occasionally destroyed by the access of bile, notwithstanding that they are inclosed in a cyst. When this takes place, the membranous substance curls itself up, shrinks, and is contracted into a small compass. The fluid is gradually removed by absorption, and the containing cyst, by degrees, closes upon its contents. I have in several instances found in the liver, the *débris* of what must, at some former period, have been *hydatids* of considerable size. They consisted of a small cyst, of the consistence of leather, and rough internally, containing a membrane closely

packed in numerous folds, mixed up with albuminous and friable matter, deeply tinged with bile. On carefully unravelling the membrane, it was found to be the remains of one or more large hydatids. In other instances, doubtless of longer duration, the mass has presented a cretaceous and more consistent character.

It is scarcely necessary to allude here to the singular opinion maintained by Dr. Baron, of Gloucester, that tubercles are essentially hydatids. The theory of this distinguished physician, which was founded principally upon the observation of the frequent co-existence of tubercles and acephalocysts in the lungs of ruminant animals, a circumstance rarely remarked in man, has not received the assent of modern pathologists.

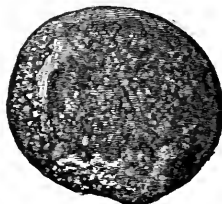
The parts in which acephalocysts are developed, appear to undergo no changes beyond those which the bulk of the animal occasions—that is to say, the structures in the immediate vicinity of the containing cyst are perfectly sound and natural. There are no general symptoms that indicate their presence in the system; the derangement which they produce being attributable to the displacement and atrophy of the parts around. They often, indeed, attain a large size before the existence of a tumor is detected. Possessing, however, a natural tendency to increase and multiply, their presence in the cavities of the body becomes a serious evil, since they interrupt the functions of important organs, and in this way ultimately cause death. Such was the case in the boy from whom the preparation which I now show you was taken. Paralysis and apoplexy were occasioned by the production of a large acephalocyst in the lateral ventricle of the brain, which contained sixteen ounces of limpid serum. It is, however, in the abdomen that we more frequently meet with them; and for information relating to the disturbance which these bodies occasion, when developed in this situation, I must refer you to an excellent paper on the subject, recently published by Dr. Bright, in the Guy's Hospital Reports, and to the more

ample account of them by Cruveilhier, already alluded to.

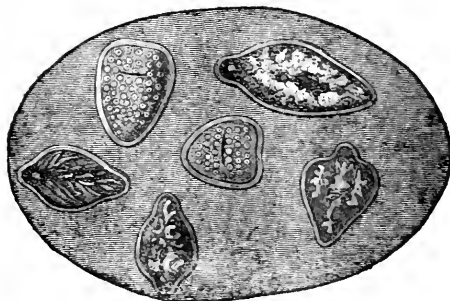
As to the treatment which acephalocysts require when their existence is detected, I have but little to say. When of small size, a puncture with a small trocar, where it can be made with safety, affords the best chance of effecting a cure. If the hydatids are large, this is attended with some risk, from the constitutional irritation consequent upon inflammation and suppuration of the cyst. In superficial situations, as in the extremities, we might endeavour to rupture the animals by violence, as by a smart blow. It has been proposed to attempt their destruction by the agency of electricity; by acupuncture; or by the constant application of ice to the swelling, under the supposition that the parasite would be destroyed by lowering its animal temperature. I should be inclined to adopt the simple operation of breaking up the hydatid with a cataract needle, which, if used with care, can scarcely be attended with any risk.

2. *Echinococcus*

(from *εχινος*, echinus, and *κοκκος*, signifying a body of a rounded form). The echinococcus is found in cysts in the brain, liver, spleen, and omentum.



It consists of a cyst analogous in structure to that of the acephalocyst, containing a number of animalcules, provided with four suckers and a crown of hooklets, which float in the fluid within. It was in consequence of the presence of these animalcules that these animals were called, by Rudolphi, *living hydatids*. The echinococcus is very rarely found in man.



Vermiculi of the Echinococcus, highly magnified.

Neither Rudolphi nor Bremser have met with it. It was first observed in the human body by Meckel. Zeder discovered several in the brain of a young woman, occupying the third and fourth ventricles. Müller has recently described a case, in which a species of the echinococcus was voided with the urine, by a young man labouring under renal disease. One of the best authenticated examples of the occurrence of this entozoon in the human body, is a case published by Rendtorf, in a thesis on Hydatids. The sac containing the vermiculi was developed in the brain. It was of large size, and weighed two pounds three drachms, and filled the right ventricle, which was so dilated as to be reduced to half a line in thickness. —As a cause of disease, we must view this hydatid much in the same light as the acephalocyst.

OBSERVATIONS

RELATIVE TO

CHANGES IN THE CRYSTALLINE LENS,

AND

THE CAUSE AND CURE OF CATARACT.

BY SIR DAVID BREWSTER, K.H. F.R.S.

To the Editor of the Medical Gazette.

SIR,

I BEG to thank you for calling my attention to the queries of your correspondent from Glasgow, which I am the more anxious to notice, as I fear I have neglected to acknowledge a private communication from the same quarter.

In the Treatise on Optics, to which he has referred, it would have been out of place to have entered more minutely into the subject of the changes in the crystalline lens, and their connexion with cataract; and I was the less desirous of doing this, as I had contemplated the publication of a work "On the Diseases of the Eye, considered as an Optical Instrument." As I fear, however, that I shall never find leisure to execute such a task, I shall endeavour to give as distinct an answer as I can, to the *eight* queries of your correspondent.

1. The method of ascertaining that the change on which presbyopia depends

commences generally at the margin of the lens, is to place a taper, or small luminous image, at the distance of thirty or forty feet from the eye. The image of the taper, in place of being perfect, will be elongated in one direction, very frequently downwards, so that the eye is often unable to see the black wick of a candle, in consequence of part of the flame being brought down upon it. If the change were general, or symmetrical, a convex lens would remedy the imperfection of vision which it occasions; whereas it is well known, that until the change has gone round the margin of the lens, the eye does not derive any aid from spectacles. I have especially studied this fact in my own case, and seen it in many other cases.

2. When this change has become symmetrical, and advanced regularly in a sound eye, the application of a lens remedies the imperfection of vision arising from the increase in the focal length of the crystalline. But if the change has not advanced favourably, either from an improper use of the eye, from general bad health, or from other causes, the fibres, and even the laminae, *separate*; that is, they cease to be in *optical contact*, in consequence of the lens not being supplied with a sufficiently aqueous secretion. Now when light passes through a part of the lens where this separation has taken place, it is reflected and decomposed by the fibres, so as to produce not only the prismatic colours, but irregular luminous figures, round the candle. If we now take a plate of brass, with a small hole in it, we may so place it before the eye as to exclude all the light, except that which passes through the diseased part. When this position is found, the eye is *blind*. It can see no objects distinctly, because all the sound part of the lens is shut up. If, on the contrary, we take a small-headed pin, and place the head of it so as to prevent any light from falling on the diseased part of the lens, while the sound part receives rays from any visible object, the vision will be *perfect*. If only a small part of the lens is sound, we may enable a blind eye to see distinctly, by excluding all the light except that which falls on the sound portion of the lens. I have repeatedly performed this experiment, to the surprise of the patient. The colours above referred to are always seen in the crystalline lenses of *animals*, when the laminae or fibres are separated.

3. The *evidence* that a derangement of fibres arises from mismanagement of the eye when presbyopia commences, is not very ample, and has more the character of inference than of demonstration. The separation of the fibres, indicated by coloured images, and irregular luminous branches round and near the principal image, took place in my own eye, and continued for nearly eight months; during which I studied the changes which took place, and made drawings of the luminous figures. As I was in the habit of exposing my eyes to the severest trials, often for twelve and fifteen hours a day, I could not doubt that this *first stage of a cataract* was owing to the greatest mismanagement. It is, besides, universally admitted, that at those periods when the constitution undergoes a change, the greatest attention to the general health is necessary; and surely, in such a delicate fabric as the crystalline lens, composed of *millions* of fibres, it is highly necessary, when it begins to change its mechanical condition, that it should not be exposed to the same hard work which it might at other times be capable of performing.

4. The experiments by which the seat and extent of the disease may be ascertained, have been stated in section 2.

5. I fear it would be presumption in any one, not a medical practitioner, to answer your correspondent's fifth query, respecting the proper remedies for stopping the progress of these changes. I cannot err much, however, in stating what I did in my own case, and leaving your readers to judge whether or not it had any share in the result of restoring my eye to a degree of strength which has enabled it, during the last fifteen years, to perform the hardest work to which that organ can be applied.

Considering the disease as arising from a defect in the secretions by which the lens is supplied with aqueous fluid, I deemed it necessary to give up all experiments; to abandon reading at night; to preserve the eye from strong and numerous lights; to pay the greatest attention to diet; to take regular exercise; but above all, to keep clear the *primæ viæ*, which I did by taking daily the *pulvis salinum compositum* for nearly eight months. It is a curious circumstance that I first saw the

coloured images, when I was making experiments with my eye, while waiting for the moves of a slow chess-player; and *eight* months afterwards, in the same room, and similarly employed with the same companion, I all at once observed the images disappear, the fibres and laminae having then been brought into optical contact. Ever since that time my eyes have been exposed to severe work, and the one which had begun to give way, is now the one which I invariably use in all difficult experiments.

6. I believe it is by no means common to see the coloured images that have been above described, but it is extremely common to have the lens so disorganised by inequality of density in different parts, arising, no doubt, from differences in the refractive power of the secretions, that a distant luminous object is not only seen of an irregular form, but is often subdivided into two, or even more, amorphous images. This I have found to be the source of a great many defects of vision which had been ascribed to very different causes.

7. If spectacles are so constructed as to exclude the rays which fall upon the diseased part of the crystalline, the patient will have the advantage of good vision; but if the sound part is very small, it will be difficult to keep the small aperture opposite to it.

8. As long as the sound part of the lens is of such a magnitude that the patient can see through a small aperture, the art of applying and using which he will acquire by experience, the continued use of the aperture, or the spectacles in which it may be placed, will of course be beneficial.

I have thus answered your correspondent's queries in the way I would have done fifteen years ago; but I have recently been led, by experiments on the crystalline lenses of animals, to more extended views, of which I shall endeavour to give a brief and general idea, referring the reader for more minute information to a paper in the forthcoming volume of the Philosophical Transactions, and to the Reports of the Proceedings of the two last meetings of the British Association.

In these researches I was led to the discovery that the capsule of the crystalline lens is a membrane which performs the functions of *endosmose* and *exosmose*, keeping up a due proportion between

the aqueous element in the aqueous chamber and the lens. Even in the dead state this membrane imbibes distilled water so greedily, that the lens which imbibes it becomes quite soft, expands, and bursts. Viewing the capsule in this light, there can be no doubt that *soft cataract* arises from an excess of aqueous humour imparted to the lens through the capsule, and that *hard or dry cataract* arises from a defect of *water* in the aqueous humour, or an *excess of albumen*.

Let us now suppose that the skilful oculist has ascertained that there is a disorganisation of the lens which does not yet appear as cataract. He will, of course, first try the remedies already mentioned, as they are equally suitable for both varieties; and he may probably succeed in restoring a healthy action in the parts. But if he finds the disease gaining ground, he must resort to more direct methods before the lens becomes irrecoverably injured.

In this state of matters he should puncture the cornea, so as to get a small portion of the aqueous humour, in order to ascertain, from its refractive power, whether it contains too much or too little albumen. In either case it might be advisable by a partial evacuation of the aqueous humour to reduce the quantity of the diseased secretion, in the hope that a healthier one might be supplied; but if the disease should continue to advance, distilled water, or a solution of albumen, should be injected into the aqueous chamber, to restore the aqueous humour to its proper condition. If the aqueous humour first taken out should prove to have the ordinary refractive power of that humour, and to appear otherwise in a healthy state, in that case it would not be advisable to introduce either water or albumen. It is quite possible that the capsule of the lens might lose its power of supplying aqueous fluid to the lens, or might supply it too copiously; but such a defect is likely to shew itself either in partial or general opacity. At all events, this opinion will only be entertained in the case where the aqueous humour is found to be in a healthy state.

Although these views are applicable principally when the disease is taken early, yet even if a lens had *white opacity*, which might not be accompanied with any other disorganisation than the mere want of a fluid to restore the

optical contact of the fibres, I would not despair of effecting the desired change by the methods above given.

It was stated, as an objection to the preceding views, by a distinguished anatomist, when I submitted them to the Medical Section of the British Association at Bristol, that it would be dangerous to inject water into the aqueous chamber. Having been occupied at another Section, I had not an opportunity of answering this objection; but I conceive that I deprive it of all its force, when I state that the celebrated oculist, Professor Maunoir, of Geneva, is in the practice of supplying, by injection, the aqueous humour which is often lost during the operation of extracting the lens.

I am, sir,

Ever most truly yours,

D. BREWSTER.

Allerley by Melrose,
Dec. 9th, 1837.

HISTORY OF DISCOVERIES IN THE NERVOUS SYSTEM.

To the Editor of the Medical Gazette.

SIR,

I SHALL be greatly obliged if you will allow me to explain in your journal a passage in the History of Physiology, which forms part of my History of the Inductive Sciences: and I make this request with the less scruple, since the passage in question has been objected to publicly as well as privately. In vol. iii. p. 425, of the work just mentioned, I have noticed, among the discoveries in nervous anatomy, "the remarkable one made in our time by Sir Charles Bell and Mr. Mayo, that the two offices of conducting the motive impressions from the central seat of the will to the muscles, and of propagating impressions from the surface of the body and the external organs of sense to the sentient mind, reside in two distinct portions of the nervous substance; a discovery which has been declared to be, doubtless, the most important accession to physiological knowledge since the time of Harvey."

This statement has been objected to in the Edinburgh Review, as highly unjust to Sir Charles Bell. In a letter addressed to the Editor of that journal,

and extensively circulated, I stated that I was quite ready to declare my assent to the view given by Dr. Henry (who assigns the honour of the discovery exclusively to Sir C. Bell), "if, in revising the original memoirs, and consulting the best authorities, this view should appear to be well founded." I have now thus re-examined the subject, and am prepared to state the result of my reading and reflection.

I must observe, in the first place, that I have here nothing to do with other parts of Sir Charles Bell's labours besides that which bears upon the discovery of the distinction between nerves of sensation and of volition. As I have already stated, my History of Physiology was professedly intended to be directed only so far as to exhibit, by examples of peculiar portions of the subject, the place and order of that science in the present scheme of human knowledge. Nothing can be more absurd than to blame me for leaving Sir C. Bell's other researches unnoticed. It neither suited my plan nor my power to estimate all the researches of great physiologists. If I have given a just view of the nature and bearing of some of the principal positive and definite physiological discoveries, I have succeeded in the only task which I attempted.

Confining myself, then, to the discovery of the distinction of sensitive and motive nerves, I must remark that according to the mode in which I have viewed all such events, the discovery consisted of two parts: the conception in *idea*, and the confirmation of this by *facts*. I shall speak of these in succession.

In this, as in other cases, it was only by degrees that the discoveries brought the true idea into a clear and definite form. The distinction of sensation and volition had indeed so long been familiar to the minds of physiologists that no originality of thought was needed to hit upon it. But Sir Charles Bell appears at first to have pursued his researches with reference not to *this* distinction, but to others. In his "Idea of a new Anatomy of the Brain," (1811) he endeavoured to establish the opinion that different endowments are in the same cord of the spinal nerves, and held by the same sheath; and the nature of *this* difference was, that both sensation and volition belonged to the

anterior origin of the nerve and another function to the posterior origin. Yet along with this, he appears to have entertained, as a conjecture, the doctrine now in question. For in a letter written by him, and published in Dr. Cook's work on Palsy (vol. ii. pt. i, p. 57, 1821) he says, "the nerves of sensation and motion are bound together in the same membranes for convenience of distribution, but there is reason to conclude they are distinct throughout their whole course." But notwithstanding this conjecture, in his memoirs of 1821 and 1822, the reference of his researches is not to this difference, of sensation and motion, but to another view, resembling his original one. His main point in these memoirs is, that certain nerves are nerves of sensation and volition, and that certain others are "respiratory nerves." Thus, *Phil. Trans.* 1821, p. 417, "the fifth nerve, the nerve of mastication and sensation;" in 1822, p. 310, "this (the respiratory) system of nerves is superadded to that of mere feeling and agency."

Mr. Mayo, in his *Anatomical and Medical Commentaries* for August 1822, (p. 122) proposed objections to Sir C. Bell's attempt to establish the difference of regular and respiratory nerves; and employed the familiar distinction of sensation and volition as the true interpretation of the results of his own experiments. Thus he asserted that the *portio dura* of the seventh pair is a nerve of voluntary motion, and certain branches of the fifth pair nerves of sensation only. In the meantime (in August 1822) M. Magendie, at Paris, made experiments by cutting the posterior roots of the *spinal* nerves of puppies. "I was ignorant," he says, (*Journal de Physiologie*, tom. ii. p. 376) "what would be the result of this trial. I at first thought that the limb corresponding to the nerves was entirely paralysed: it was insensible to the strongest pinching and squeezing, and appeared to me also immoveable; but soon, to my great surprise, I saw it move in a very apparent manner, although its sensibility was still extinct." He repeated his experiments, and began to consider it as probable that the posterior roots of the nerves of the spine were more peculiarly appropriated to sensibility. He then found, by cutting the anterior roots of the spinal nerves, that the limb became immoveable, while it unequivocally retained its

sensibility. M. Magendie's narration appears to show that he had not previously entertained the idea of this distinction, as a guide to his experiments. In the meantime Mr. Mayo was advancing to the same point. For having satisfied himself that the fifth nerve consists, like the spinal nerves, of two portions, a ganglionic and a non-ganglionic part, of which the former is a nerve of sensation, and the latter a nerve of motion, he "was led to conjecture that the double roots of the spinal nerves have also functions corresponding with those of the fifth. . . . When I was engaged," he adds, "in experiments to determine the fact, M. Magendie's were published, which established the justness of my conjecture." (Commentaries, July 1823, p. 10.) The same idea was decidedly adopted by Sir Charles Bell at or about the same time, retaining however his views of the respiratory function of the nerves. For in his next memoir, read to the Royal Society, June 13, 1823, he says (p. 300) "the fifth nerve is the nerve of sensation to the head and face; the *portio dura* (of the seventh) is the motive nerve of the face and eyelids; and the respiratory nerve, and that on which the expression of the face depends." And in the same memoir (p. 303) he asserts that "all the nerves which bestow sensibility have ganglia at their roots, and those which have no ganglia are not nerves of sensation, but are for the purpose of ordering the muscular frame."

Thus the proposition, that there is one set of nerves with ganglions for sensation, and another set without ganglions for motion, was clearly brought into view. Our next business would be to inquire to whom the *experimental establishment* of this proposition is mainly due. This question it is very difficult for a person to decide who has not an intimate and extensive knowledge of physiological researches. A large portion of the credit must be assigned to Sir Charles Bell. For although his experiments on the spinal nerves, made in 1809, had not led him to assert the proposition, they were readily seen to be evidence of it when it was once asserted. His experiments on the nerves of the face, when they had been corrected by Mr. Mayo, further confirmed it; and the earnestness and perseverance which he displayed in seeking to establish differences in the

functions of the different nerves, had a leading influence on the progress of the subject, both through his own labours, and through the impulse he gave to those of other inquirers. Moreover, the very striking and attractive view which he at first presented of the nature of the primary distinction of function in nerves, (including its bearing upon the expression of countenance,) however this view might afterwards be limited or modified, was probably one of the main causes of the advance made in this subject. At the same time I also think it cannot be doubted that Mr. Mayo shewed this view, in its original form, not to be borne out by the experiments. Mr. Mayo also added materially to, and in some instances at least corrected, Sir C. Bell's experiments, and the interpretation of them: as, for example, when in animals, in which the fifth pair of nerves had been cut, he ascribed the disuse of the lips in eating to the loss of sensibility, while Sir C. Bell, less justly, had at first ascribed it to the loss of muscular power. And M. Magendie's experiments on the spinal nerves, while they appear to be independent of the researches of the two English anatomists, were accepted at the time as sufficient to establish, at least with a high degree of probability, the doctrine which he collected from them.

It is not necessary to my purpose to trace the further *confirmation* of this doctrine by succeeding observers. I will only remark, that though the experiments of Panizza and Tiedemann appear to have been highly important for this purpose, the most decisive labours seem to have been those of Professor Müller, of Bonn, who was justly dissatisfied with the vagueness of M. Magendie's statement, in which the proposition was only asserted as approximately true; and who made a remarkable series of experiments on frogs. (Hildebrandt, Handbuch der Anatomie, III. 359. Müller, Handbuch der Physiologie, 1824. I. 627. Treviranus, Organischer Leben II. 38.) I may add that though Treviranus expresses some doubt of the exactness of the proposition, it appears to be at present generally accepted by the soundest physiologists.

Looking upon the case as above stated, with the best judgment I can use, I find myself led to the conclusion that no injustice was committed by join-

ing Mr. Mayo's name to that of Sir Charles Bell, as I did; but that, as I have already stated, this is not to be understood to imply that they had equal shares in the discovery. If I should have occasion to reproduce this part of my history, I should wish to describe the discovery as having been "made by Sir Charles Bell, Mr. Mayo, and M. Magendie; the two latter physiologists having corrected and completed the researches of the former;" to which might be added the above notice of its confirmation. And it is to be observed that this statement is independent of all the credit which may be due to Sir Charles Bell for his share in the discoveries respecting the functions of the nerves—vital, nutritive, sympathetic, instinctive, automatic, respiratory, or by whatever names they may be described, which have been a main object of his labours.

Having thus, to the best of my ability, redeemed my pledge to reconsider the subject, I have only to beg your professional readers to bear in mind the attack made upon me, and the necessity under which I was placed of showing that I had not discharged unfaithfully my duties as an historian, in order that they may acquit me of presumption in thus occupying your pages.

I have the honour to be, sir,

Your very obedient humble servant,

W. WHEWELL.

Trinity College, Cambridge, Dec. 11, 1837.

REPORT

OF A

CASE OF IMPERFORATE ANUS;

IN WHICH THE RECTUM WAS FOUND TERMINATING IN A CUL-DE-SAC, WHICH HAD AN IMMEDIATE COMMUNICATION WITH THE ADJACENT URETHRA.

To the Editor of the Medical Gazette.

SIR,

By inserting the following case and dissection in your valuable journal, I shall feel obliged,

I am, sir,

Your most obedient servant,

J. GORHAM, M.R.C.S.L.

225, High Street, Borough,
Dec. 8, 1837.

Mrs. Nunn was safely delivered of a male child, and, ten hours afterwards, I

was informed by the nurse that no passage existed whereby he might void his excrement; and on examination I found to my surprise, that the anus was imperforate, and there was a bluish discoloration of integument on the spot where the anus ought to have existed.

Twelve hours after birth I made a puncture with a common lancet through the skin, at the point of discoloration, but without entering the rectum: I then incised transversely for about half an inch, and introduced the point of a lancet backwards and upwards, in the direction of the os coccygis. A little wind now escaped, but no meconium. A clyster was attempted to be administered, but it returned by the side of the pipe. I now placed a pledget of lint in the opening, and left the infant.

I may remark that the infant did not cry during the whole of the operation, neither was it convulsed. Ten hours after the operation he has passed urine, but no meconium; no swelling in the region of the anus; no hæmorrhage; the countenance is sallow.

The infant died in a few hours after this visit; but not in convulsions.

Post-mortem appearances. — The small intestines appeared healthy throughout. The duodenum was empty. The jejunum contained some small isolated portions of meconium, while the ileum, besides containing portions of meconium, was also distended with flatus. The transverse and part of the descending colon were full of meconium. The whole of this large intestine was excessively distended with flatus, so that the sigmoid flexus was much straightened.

The rectum was distended with gas and meconium, and terminated in a pouch. At the very bottom of this pouch a small foramen was found, with radii of puckered intestine passing from it in every direction. It was of sufficient size to admit a probe of about half a line in diameter, which passed immediately into the urethra. The bladder contained some urine, but no meconium. On passing a probe into the urethra, it entered at one time the bladder, and at another the pouch of the rectum; this was fully explained by making a longitudinal incision through the upper surface of the urethra, and thus exposing the canal.

A small abnormal opening now presented itself immediately anterior to the prostate gland. This opening was

about the size of three-quarters of a line, and after a passage of about two lines opened into the rectum immediately behind it.

The ureters terminated about a line and a half distant from each other, so that no trigone could exist.

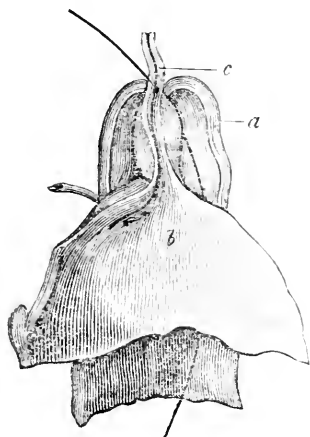


FIG. 1.

- (a.) The rectum slit open longitudinally on its anterior part.
 (b.) The bladder opened in the same manner.
 (c.) A section was continued along the upper part of the urethra, and on its under part an opening is seen anterior to the prostate. On passing a bristle through this opening, it immediately enters the rectum.

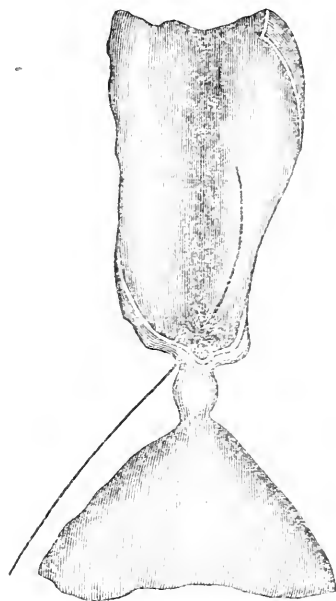


FIG. 2.

The bladder has been turned forwards, so that its posterior part is now evident. By this are exposed the termination of the rectum in a *cul de sac*, and the radiated appearance around the opening.

FATAL CONVULSION DURING THE INJECTION OF NÆVUS.

To the Editor of the Medical Gazette.

SIR,

EITHER as a warning from the operation of injecting the vascular nævus, according to the plan detailed in the *MEDICAL GAZETTE*, October 1, 1836, or as an instance of the distressing casualties which beset the hopes of our profession, the following impressive case seems due to the medical world, and you will oblige us by the insertion of it.

The nævus was situated over the angle of the right maxilla, in a healthy and remarkably precocious child, nearly two years old, and had increased from

a slight speck at birth to the size of half a small orange. It was principally subcutaneous, but had a coloured portion in its centre, which seemed covered by cuticle only. On the 19th of October the tumor was pierced with a lancet used for the nasal duct, and injected by the transfusion syringe; the mixture used being nitric æther, with one-fifteenth of nitric acid. No decided effect was produced, so that either too little of the injection had entered or the mixture was too slightly stimulating, and in about a week the operation was repeated; the proportion of nitric acid being increased to one-tenth. No more effect, however, followed this than the former operation.

On the 15th of November the injection was again had recourse to; using, instead of the nitric acid and æther, the

liquor ammoniæ, weakened so that the nose could remain applied to the bottle. The perforation was this time made from the opposite side, and more attention being paid to the compression of the tumor, the piston was felt to descend more, and the tumor appeared more injected. At this instant the poor child suddenly ceased its crying, and at the end of a slight convulsion, continuing not a minute, lay upon the table a corpse.

As to the cause of this melancholy result, the first hurried impression at the conclusion of this harassing scene was, that the attack was merely *coincident* with the operation; that, in fact, it was one of those sudden and fatal succussions of the nervous system which now and then carry off children, apparently without reason, and of which an instance occurred here a few months ago, in a child who was playfully smiling on its mother's knee, and there died almost without a struggle. This also is Mr. Lloyd's idea, to whom the case has been reported, and who in his answer (of which he permits the fullest use) gives the following instance:—

"I was examining a child's throat, as his mother considered there was some difficulty in swallowing, and he had not been quite well for a few days before. Just as I had got the spoon on his tongue (he had resisted very much, and was in a state of great excitement), he threw himself backwards, uttered a scream, and was a corpse." He also adds:—"I may confidently state, that I have performed the operation considerably more than a hundred times, without the occurrence of any thing like a serious symptom;" and "I repeat, that I feel confident that the operation you performed could not have been the cause of the melancholy death of your poor little patient."

Yet though it is impossible to deny that such was the secret of this appalling case, and the harassed feelings cling to the supposition, it seems at least as impossible to deny, that some of the stimulating fluid was forced into the divided veins, thence carried along the external jugular and subclavian to the heart, and thus produced the death; or that a strong irritant, applied to the branches of the seventh and third nerves, is as likely to excite a fatal succussion of the whole nervous system, as those doubtful yet accredited agents,

dentition and gastric sordes. Either of these occurrences would be decisive against the operation, and their possibility demands the publication of the case.

No post-mortem examination was allowed.

However unlikely it is that air should be forced any where but into the cellular tissue of the nævus, it may be well to state, that it was impossible in this case, as the syringe was first filled with particular care, and afterwards held nozzle upwards, to insure the perfect filling of its pipe, and the escape of any air, if possibly any were left in.

We are, sir,
Your obedient servants,

T. PAGET,
Junior Surgeon to the Leicester General
Infirmary, and Consulting Surgeon to
the County Lunatic Asylum.

FRANK FULLAGAR.

Dec. 5, 1837.

REMARKABLE CASE OF TRIPLETS.

To the Editor of the Medical Gazette.

SIR,

PERMIT me, through the medium of your periodical, the MEDICAL GAZETTE, to make known to the profession the following interesting case:—

On Sunday morning, the 10th inst., I was called upon to attend Mrs. Hamet, residing at No. 7, Upper Park Place, Regent's Park, in labour with her sixth child. On my arrival, at half-past ten, I found she had been in labour some hours; the membranes had been ruptured, and on making an examination per vaginam, I discovered a breech presentation; the feet were brought down, and the child delivered in the usual manner. On laying the hand on the abdomen, it was evident the uterus contained another child. On making a second examination, per vaginam, the membranous bag of the second child was detected, and of such volume, and containing so much fluid, as to render it impossible to determine the presenting part. In half an hour after the expulsion of the first child, the pains, which had ceased for a short time, recurred, and before the hour had elapsed the membranes were ruptured, and at least two quarts of fluid escaped; the head of the second child descended, and was,

with the body, quickly expelled. The hand being again laid on the abdomen, detected another child, which, after a few minutes, like the second, evolved under the head presentation, bringing with it its own placenta entire; subsequently the placenta of the two first-born children were expelled perfectly distinct from each other.

The peculiarities of the case are, that the children, two boys and one girl, were born alive, well formed, and apparently of full growth; that each had its own placenta and membranes unconnected with those of the other, and expelled separately; that the second and third membranous cysts were simultaneously ruptured after the expulsion of the first child. The woman is in her 34th year, has suffered no inconvenience, and is at present doing extremely well.

I have the honour to be, sir,
Your obedient servant and constant
reader,

W. LOVEGROVE.

13, Baker Street.

CASE OF AMAUROSIS AND PARALYSIS OF THE EYELID.

To the Editor of the Medical Gazette.

SIR,

IN the diagnosis of obscure disease, it is most important to consider, in addition to the existing symptoms, not only the peculiar idiosyncrasy of the patient, and the characteristic (or, as Sydenham would term it, the *epidemic constitution*) of the season, but also the medical topography of the situation in which the patient resides.

There is no cause which produces affections so varied, so anomalous, and so obscure, as marsh miasmata, or malaria. Not only do we observe all the modifications of *pyrexia*, taken in its most extended sense, but also many maladies purely of the nervous system. A most intelligent physician, of Holland, informed me, not long since, that it was not uncommon in that country to see patients suffering from all the symptoms of apoplexy, originating from the same source as intermittent fevers, and cured by the same remedies. Rheumatism and neuralgia are also to be often traced to the same origin; but it is not often that we see paralysis produced by marsh miasmata. The following case, however, would seem to

have been produced by it, and was cured by the treatment which such an opinion suggested. It occurred under the care of my friend, Mons. J. de la Harpe, physician to the Cantonal Hospital of the Canton de Vaud, at Lausanne, who has allowed me to make use of it.

The Canton de Vaud, in Switzerland, which extends along the shore of the Lake of Geneva, is bounded on the south by that fine expanse of water, extending over upwards of 400 square miles, and on the north by the Jura Mountains. Although, from its vicinity to so extensive a Lake, it might be supposed that intermittent fevers are very prevalent, yet this is not the case; but, on the contrary, they rarely happen excepting at the upper part, at the mouth of the Valley of the Rhone, and in this situation many circumstances concur to produce them. Where the Rhone enters the Lake of Geneva, there is a considerable extent of alluvial and marshy land, having all the characteristics of soil and vegetation common to such situations, and affording a most interesting spot for the investigations of the botanist. The valley of the Rhone, shut in from the cold winds by lofty mountains, and presenting a long extent of exposure to the southern sun, is, during the summer, almost suffocatingly hot; while the Rhone, being supplied from the snows and glaciers of the Alps, is particularly full at that season, and a stream of cold exhalation arises from its surface, almost certainly productive of fever to those who, without additional covering, approach its banks. The above sketch of the medical topography of the country, is, perhaps, desirable for the elucidation of the following case:—

Jaques Deleysin, aged 28 years, agriculturist, of a strong and robust frame, was attacked, without any known cause, in May, 1837, with pains on the left side of the head, which were soon followed by a weakness of the left eye. This was succeeded by paralysis of the superior eyelid and complete amaurosis, while the pains of the head increased in intensity. These symptoms continued, with very little variation, till the middle of July. The medical men who were consulted recommended successively leeches, blisters, and anodyne liniments to the painful part, and many remedies internally, without any material allevia-

tion. The leeches relieved the pains for the moment, but they returned with greater violence afterwards. Finally, a few days before his admission into the Cantonal Hospital, they had applied two moxas; one to the nape of the neck, the other on the left frontal bone.

On the 13th July the following were the symptoms that presented themselves:—Nearly complete paralysis of the upper eyelid of the left side; the patient could raise it but very little. On elevating the eyelid with the finger, the pupil was observed to be very much dilated, and insensible to light; the vision on that side was lost; there was no other mark of disease in the eye. There were very severe and lancinating pains in the upper jaw, the temple, the forehead, and in the whole parietal region of the head on the same side. The moxas were beginning to suppurate around the eschar; the pulse was small, rather weak, but not accelerated. The pulsation of the carotids indicated no cerebral congestion; all the other functions were natural. At the first aspect it was thought that there was some affection of the brain, of which the paralysis of the retina and of the eyelid might be the result; but the absence of every other cerebral symptom seemed to exclude that idea. On questioning the patient more closely, he stated that the pains were not always of the same degree of intensity; that they were more severe in the morning, and became more calm during the night. He added, that he had occasionally some moments of positive ease, but that ordinarily the pain only remitted, but did not cease entirely. He has slept but little for a long time past. The pains were so severe that he could not help crying out, or even weeping. On reflecting that the man came from the situation where intermittents prevail, (almost the only place in the Canton), the impression that remained was, that this was a marked form of intermittent; and as nothing appeared to contraindicate the employment of quinine, he was ordered $1\frac{1}{2}$ grs. of the sulphate every three hours. The moxas were dressed with simple ointment, to promote their healing.

From the second day of the employment of the quinine, the patient experienced decided relief. On the fourth day the eyelid had recovered some degree of movement, and the eye had recovered some degree of vision. The

pains ceased on the sixth day, and the amaurosis diminished rapidly.

The 23d July, ten days after his admission into the hospital, the case was completely cured. From the 17th he had employed a spirituous ophthalmic lotion.—I am, sir,

Your obedient servant,

J. C. Cox, M.D.

Naples, Oct. 1837.

CHOREA FROM PREGNANCY.

To the Editor of the Medical Gazette.

SIR,

SHOULD the following case of chorea appear worthy of notice, I would beg as a favour its insertion in the pages of your journal. It appears to me to possess singular features, as seeming to arise from uterine irritation consequent on pregnancy, its remittent type, and its being confined to one half of the body laterally—a circumstance which I have never before observed, and which might entitle it to the name of hemiplegic chorea, in distinction to that form affecting only the upper extremities, or any single limb or set of muscles.

On November 6th, I was called to see Mary Bond, æt. 23, a patient of this institution (General Dispensary), residing at 37, Duke-street, Smithfield, who had recently married, and till then had always enjoyed good health, with the exception of a slight degree of menorrhagia, or rather more frequent return of menstruation than usual. Her friends, who called me in, informed me that since the period of her marriage, which took place about the beginning of September, she had never menstruated, and that during the last week she had been affected with fits, returning regularly about 7 P.M. daily. Accordingly, about that hour I called, and found her lying on the bed, with distortion of the left side of the countenance, kicking violently with the left leg, and striking continually with the arm of the same side, which motion she was wholly unable to cease or restrain, and had bruised the limb severely against the bed-post. These fits never wholly left her; she always had tremor and slight twitching in the limbs of that side during the day, which became ungovernable towards the evening.

She was a tall thin young woman, of fair complexion and light hair, very pale, and with a small, frequent, and compressible pulse, and an appearance of general want of tone in the system. She said she had no pain whatever, excepting an uncomfortable feeling caused by constant spasm of the orbicularis palpebrarum, and corrugator supercilii. On inquiry she told me she had occasional pains in the loins and back of the neck, which she referred to taking cold, but which were not confined at all to either side of the body. She added, that she suffered from morning nausea, slight feverishness, and headache; and on examining the breasts, I found the areola of the nipple wider than usual in the unimpregnated female, and of a darkened hue. From the constant muscular exertion she was in profuse perspiration; and the violence of the spasms, and the state of lassitude in which they had left her, had wholly deprived her of rest for the two previous nights. As this was the case, and the bowels were slightly confined, I ordered—

Pil Sapon. comp. gr. v.; Hyd. Chloridi, gr. iv. ft. pil. ij. hora somni sum.
Pulv. Rhæi, c. Hyd. Chlor. gr. xv. eras mane sumend.

On calling the next day, I found that the opium had disagreed with her, having caused for a time pain in the head; and that since taking the medicines she had suffered, at different periods of the night and day, three distinct paroxysms of chorea. As the bowels had been freely moved, and there was now no pain in the head, I prescribed—

Ferri Sesquioxidi, 3j. ter die sumend.

and on repeating my visit on the 10th, she told me she had already experienced great relief, having slept soundly, found her appetite improved, and the fits less violent and of shorter duration, though she was rather frightened at the black colour of her stools.

Rep. Ferri Sesquioxidi.

The dose to be gradually increased; and as the bowels were not quite sufficiently open, I ordered—

Olei Ricini, ʒss. pro re nata sumend.

and daily exercise for a short time, when the weather was such as to allow it.

After this I did not see her till the 19th, during which period she continued improving, having by my directions increased the dose of iron to nearly ʒiij.

Calling then, I found a material improvement; the motion in the leg had nearly ceased, and the arm was so much under control, that she was able to hold her needlework whilst sewing, although there was towards evening an increase of movement, especially in the arm, which, for her own relief, she placed in a sling. The pain in the neck and loins had wholly left her; she slept soundly; the symptoms of pregnancy appeared more confirmed; and her menses, which previous to marriage always came on every fortnight, had not reappeared. Her pulse was fuller and stronger, her appetite good, and her cheeks, instead of being pale, had assumed the colour of health.

Rep. Ferr. Sesquiox. et Ol. Ricini.

On next seeing her, on the 25th, she declared herself perfectly well, and free from all tremor of the limbs, of which she had regained perfect command; at her own request I merely ordered a repetition of the medicines, in case of relapse, and desired her to give up the Dispensary letter.

Not venturing to offer any theory as to the cause of the complaint, its singular confinement to one-half of the body, or its periodicity of return, but contenting myself with merely stating the facts,

I remain, sir,

Your obedient servant,

GURNEY TURNER.

Dec. 7, 1837.

OBSERVATIONS ON QUARANTINE,

AND

DESCRIPTION OF THE LAZARETTO AT
MARSEILLES.

To the Editor of the Medical Gazette.

SIR,

THE detention in quarantine establishments, of travellers arriving in European ports from countries literally subject to attacks of the plague, is, to say the least, of a very unpleasant and sometimes not improbably a disastrous occurrence, to the subjects of such detention. If, however, the evil rested here, with the interests of a few individuals, the matter would not be one of any very great importance to the public in general; but since the principles of the quarantine system are carried out to such an extent as to effect very materially the commercial relations of

nations and the interests of large communities, any inquiry into the motives and arguments on which this system is founded, may well be considered a matter of public importance; the more especially as of late years the same principles, and the same line of action, have been brought to bear upon the occasional visits of Asiatic cholera, as in cases of plague and yellow fever.

The general principles on which the quarantine establishments of Europe have been founded, all of course rest on the *supposed* properties of contagion or infection; in other words, the *supposed* power of propagating or communicating a similar malady, which, in certain diseases, resides in the persons of men or animals affected by such disease, in the substances which are or have been in contact with them, and in the atmosphere immediately surrounding them.

The duration of quarantine depends on the length of time considered necessary for the purification and removal of the germs of contagion from the persons or substances supposed to be infected. By some unknown process of reasoning, the sanitary legislators of Europe have arrived at the opinion, that various substances possess various capacities of retention with regard to these germs of contagion; that some are highly susceptible and peculiarly retentive of the infectious principle; and that others, which do not, to common understandings, seem much to differ from them, have been said by the same authority to be incapable of either receiving or communicating the seeds of the dreaded disorder.

The substances considered to be most susceptible and most retentive of the plague infection, are, all cotton, wool, and hair, raw or manufactured, feathers, furs, paper, books, &c., silver, gold, copper, wax, sugar, coffee, tobacco, &c.

On the contrary, wood, iron, wheat, dried fruits, water, and other liquids, and many other articles, are considered incapable of receiving the infection, and from these opinions arise a number of remarkable incongruities in the *working* of the quarantine system, as I shall presently be able to show.

As the question of contagion or non-contagion is one which has so repeatedly exercised the inquiry of medical writers as to offer but an unpromising ground for present discussion, I propose confining myself to offering a few remarks on what I have personally

observed on this subject in those countries where the plague is not infrequent, and on those details of quarantine to which I have been personally subjected.

It is not unworthy of remark, that until within the last few years, quarantine has existed only in Christian countries. Of late, indeed, both the Sultan of Stamboul, and the Pasha of Egypt, have introduced the system, rather as an appendage to their other European importations than upon any sounder principles; indeed, the whole practice of the native population of those countries where the plague annually occurs, is opposed to the theory of isolation, as may be gathered from the writings of all eastern travellers. This has been attributed to the all-pervading influence of the Mahomedan doctrine of fatalism on the minds of the people; but it should be remembered, not only that the extent of this influence has been greatly exaggerated, and that a considerable portion of the population of the Levant are not Mahomedans, but Christians, of the Greek and Latin churches, but also that the customs of nations are generally found to be based upon something of experience, and from some notions of their fitness and utility.

To suppose that the Turks and Arabs, as a body, are careless of their health, and unsolicitous of means to avert or remove serious disease, but ill accords with what I have witnessed of their character; on the contrary, among no people is there a greater respect for, or a fuller confidence placed in the professors of the healing art, and none are more desirous of benefiting by the skill and resources of European practitioners.

Poverty, ignorance, and superstition, no doubt produce their legitimate effects; but we have only to recal the scenes witnessed by members of the Boards of Health in this country during the prevalence of the cholera, to assure ourselves that the doctrines of Mahommed are not necessary to the production of a sluggish indifference to danger, and a disregard for the means of averting it, among the pauper population of any town or country. Of the European residents in those countries in which the plague prevails, neither the medical practitioners or others are at all agreed as to its contagious or non-contagious qualities, as to the substances capable of communicating it, or as to the proper precautions to be observed. At Alexandria,

in Egypt, some Europeans perform a strict voluntary quarantine during the whole continuance of the disorder; others pursue their usual occupations and preserve their accustomed intercourse with the world, and both plans it would seem occasionally succeed and occasionally fail. At the same place many of the French and Italian medical officers of the Pasha's service, took every possible precaution against the communication of the plague from their patients to themselves; dressed in a kind of envelope of oil-skin, having their saddles covered with the same material, a supply of aromatic vinegar constantly to the mouth and nostrils, and never touching an affected person with the bare hand; yet these precautions failed in several instances to procure an immunity from the disease, while an Englishman, Dr. Laidlaw, practising in the same city, a man of a firm and decided turn of mind, despising every kind of precautionary measure, visited and manually assisted a considerable number of plague-struck individuals, without the slightest apprehension of danger, and without any evil result. One case attended by Dr. Laidlaw is worth recording: A captain of an English merchant vessel, in the harbour of Alexandria, was seized with symptoms of plague (then raging in Alexandria,) and placed under the care of Dr. Laidlaw. A large bubo formed in one groin, suppurated and sloughed extensively. One morning Dr. L. was sent for in haste, and on arriving, found that considerable hæmorrhage had taken place from the wound in the groin. From the circumstances of the case there was no doubt but that the blood had issued from the femoral artery, and Dr. L. immediately resolved on tying the external iliac artery, an operation which he performed with equal skill and success. The hæmorrhage ceased, the ulcer healed, and the man perfectly recovered. Now if ever the plague was to be propagated by contact from one individual to another, one would suppose that it would have been communicated in this instance. Dr. Laidlaw, however, related the circumstance to me many months after its occurrence. I need hardly say, that Dr. L., who has had charge of a plague hospital at Alexandria, and therefore enjoyed very favourable opportunities for observation, and for the formation of a correct opinion, is a *non-contagionist*.

On arriving at Kiumh, on the right or eastern bank of the Nile, in my route from India, in April, 1833, I was informed that the plague was raging at Siout, Manfalout, and other towns and villages down the course of the Nile; and this I found to be the case. The Arab fellahs, or cultivators of the soil, miserable, half-starved, and not half-clothed wretches, barely able to earn sufficient to support existence, were sinking in numbers under the disease; but though my fellow traveller and myself daily visited the villages where the disorder prevailed, and though our servants frequented their bazaars, still we never heard the doctrine of contagion broached, or for an instant suffered the idea of danger from the communication of the disorder to enter our imaginations.

Any person who has voyaged on the Nile must have observed the countless flocks of pigeons which swarm in the Egyptian villages; they are familiar in the streets; they roost on the low huts; and must necessarily have much communication with infected substances. Living animals and feathers are two articles most highly susceptible, retentive, and communicative of the germs of contagion, according to the received doctrines of all quarantine establishments. But how put the laws of isolation of person in force against the pigeons? and if this is impossible, it is evident that individual isolation of infected persons is, in Egypt at least, quite out of the question. I shall have occasion to point out, that precisely the same arguments apply to the lazarettos of Europe.

Mohammed Ali, the Pasha of Egypt, has during the last twenty years driven a very flourishing trade in cotton. This cotton is grown on the banks of the Nile in all Egypt, from Alexandria to the cataracts of Assouan, and is brought down the Nile to the depot at Alexandria, whence it is shipped to various European ports, both British and continental. This cotton being picked and packed into bales by the fellahs, perhaps at the very time (nay, to my own knowledge, at the time) when the plague is raging in their villages, and not improbably by the hands of men actually at the moment suffering from plague, is naturally enough considered by the sanitary authorities of European quarantines as the substance most susceptible of the contagious germs, and

best calculated to retain it to the longest period. All ships laden with cotton, and all passengers in such ships, are therefore subjected to a quarantine of considerable duration in all, but of variable periods in the continental ports.

In the summer of 1836 I embarked at Alexandria, in a French brig laden with cotton, and bound for the port of Marseilles. Her cabins were stowed with bales of cotton, and her decks so entirely covered with them, that we walked constantly on the cotton. We dined in the open air upon these bales, and in short lived upon, and almost in cotton, for about three weeks. Not a person on board, to my knowledge, complained of a headache during that period; but on arriving at Marseilles our troubles began.

Having hoisted the yellow flag as a signal of a foul bill of health, the ship was brought to an anchor in the quarantine harbour, which was then crowded with other vessels also undergoing quarantine. As each vessel touched its neighbour, of course the whole were connected; but as some were from Smyrna, some from Constantinople, others from Bona and Algiers, each vessel had a different period of quarantine to undergo. Had a sailor of a vessel from Smyrna, which had perhaps but three days more to remain in quarantine, touched a sailor or a rope belonging to another vessel, his period of quarantine would have been extended to the same number of days as that of the man he had touched. But the rats and mice of each vessel most probably exchanged visits in spite of the quarantine regulations; added to which, the fomites of the contagion must have been pretty effectually dispersed (if the doctrine be worth any thing) over the adjoining vessels, on opening the ship's hold, and removing her cargo thence to the lazaretto. As a passenger I was immediately removed to the lazaretto, there to remain in quarantine during twenty-one days.

The lazaretto at Marseilles is a very extensive establishment, being an inclosure of some acres of ground, open on one side to the sea, surrounded by a high double wall, and guarded by military sentinels. It contains within its walls apartments for individuals in quarantine, the house and garden of the captain of the lazaretto, the offices of a restaurateur, and spacious warehouses

and large yards for the deposit and exposure of merchandize. Each individual in quarantine has an apartment assigned to him, and a guardian attached to him, whose duty it is to watch over all his actions, and prevent or report any contact between him and any other person or thing undergoing quarantine. This guardian will also act as a servant. A considerable space of ground is allotted to the prisoners (for such they really are) for exercise, each walking out attended by his guardian. All communication with the town is carried on at the barrier, or toward entrance of the lazaretto; and here those in quarantine may see and converse with their friends from the outside, under certain regulations. To every individual in quarantine, strangers, guardians, or labourers, are assigned a certain number of strokes on a bell, by means of which all orders and communications are conveyed. When summoned to the barrier, the person in quarantine enters one of a series of small rooms, one side of which is covered with a network of wire, and separated by an interval of about six feet from a similar row of chambers, in the opposite one of which his friend from the town is placed. Here conversation may take place in comparative privacy, while at the same time contact is most effectually prevented.

Any attempt at escape would be most severely punished; if the individual had reached the outside of the lazaretto, and touched any person without the walls, he would be liable to capital punishment, and would be shot by the sentries if detected in the act of escaping.

On entering the lazaretto I first underwent a fumigation, or *parfum*, as it is termed, produced by pouring sulphuric acid over common salt. This was repeated at the middle, and at the termination of my period of confinement. My baggage was examined, and all woollen or stuff articles removed to undergo certain purifications. My letters were taken from me, and some canisters of gunpowder confiscated to the use of the captain. I was placed in a small room with a brick floor, bare walls, and an iron bedstead, and a keeper assigned to me. This room formed one of a row of similar ones, inhabited by individuals of various nations, each having a different period of quarantine allotted to him. The last

room in the row was smaller than either of the others, and its window secured with an iron grating. This was destined to be the abode of any unfortunate wretch among us who should shew any symptoms of commencing plague. In such a case the unhappy sufferer would have been shut up in this miserable hole, an object of terror rather than of compassion, and would have stood a fair chance of perishing, as much from neglect and despondency as from any actual disease. The whole yard was surrounded by a wall, and its gate closed every evening at eight o'clock.

A tenant of the yard I occupied was somewhat out of health, and a physician was sent for, who came accompanied by the captain of the place. He stood about two feet from his patient, looked at his tongue, asked a few questions, and prescribed. Had he touched such person, he would have been compelled to perform the same length of quarantine as his patient. Yet this physician and the captain himself having walked amid bales of infectious cotton, and approached closely to infectious persons, (for if not infectious, why confine them in a Lazaretto) were permitted, by the regulations of the establishment, to re-enter the town of Marseilles, and mix with its inhabitants. In the case of a poor boy who had injured his arm, and therefore required manual treatment, a young medical *élève* was placed in attendance, (and also in quarantine) upon him. The keepers or guardians are also, nominally, under the same restraints and regulations. Conversation is allowed, and indeed every thing but actual contact or communication with a conducting medium. Thus a person may throw a book or a stick to another, but if he hand it to him the quarantine of each must be equalized. Yet those persons who may not hand a stick, may sit together on the same bench without danger. The guardians play at quoits together, it being decided that iron is not susceptible of the contagion, while a piece of silver money must be placed in a basin of vinegar before it can be passed from one to another. These worthies freely circulate among themselves, cigars, tobacco, &c., although they are prohibited articles. Moreover, money judiciously applied will here, as in all other parts of the world, render the guardians incapable of seeing any un-

fortunate mishap which may occur to an individual in quarantine. In short, the contradictions and puerilities which come under observation in this establishment are innumerable, and cannot but lead to the suspicion that the whole affair is altogether useless, if not worse. Rats abound in the Lazaretto; sparrows fly in and out without regard to the sentries in the barrier, and above all, the captain of the establishments keeps rabbits, some of which are running about wild in his garden. Indeed, during a residence of twenty days in this large, and, to the French nation, most expensive establishment, I saw more than enough to convince me that the isolation of persons must be impracticable by any other means than rigorous solitary confinement, and enough to induce me to think that even could it be carried on to its utmost extent, it would be an unprofitable and useless measure as regards the preservation of the public from the propagation of the plague.

It appears, indeed, to be now a very general opinion, and one which will, no doubt, every day gain strength, that the plague can be secondarily communicated only under very peculiar circumstances, if at all; and these circumstances are, chiefly, confinement of the sick in the deteriorated atmosphere of an unventilated apartment. If the force and energy of this propagating power be in proportion to the state of the immediate atmosphere, as to ventilation and purity, the isolation of the person affected must be a matter of very secondary importance, compared with the necessity for altering and renewing the atmosphere around him.

The former is a precaution of doubtful, the latter one of certain value; and this is precisely what makes the difference between a plague-hospital and a lazaretto. The former, if well arranged and properly conducted, is, as at Alexandria, a most valuable public institution, inasmuch as its object is to relieve actual suffering, and remove existing disease. The lazaretto, on the contrary, in its double office of isolation of *suspected* persons, and isolation of *suspected* goods, is a cumbrous and comparatively useless piece of machinery, instituted for the removal of that which does not exist.

In speaking of the isolation of goods, I remarked that they are all exposed to

the air, and made to undergo certain purifying processes in the yards and warehouses of the Lazaretto.

Amongst others the bales of cotton brought from Egypt are each opened, exposed, and manipulated by a particular class of labourers, who remain in quarantine during the period required for the goods on which they are employed. These are, for the most part, healthy, active, young men, who, labouring in their vocation in the very heart and centre of the supposed infectious material, are yet preserved from the disorder in a very remarkable manner, supposing any danger of contagion to exist. It would be desirable to ascertain with accuracy how many cases of plague have occurred among these people during the last twenty years.

The period during which the quarantine of goods is to be prolonged, does not appear to be fixed according to any rules either of experience or of inference. It varies in different countries, and is much more severe in the Austrian ports than at Marseilles, and seems to be dictated merely by caprice, or to suit the particular views of the legislators. When, indeed, we recollect that it was once gravely stated that the plague in Paris was derived from some old rags which had lain several years undisturbed in an old well, and that it is now as seriously urged that 20, 30, and even 60 days are occasionally necessary for the removal of the contagious fomites from cotton or feathers, we cannot but be desirous of ascertaining on what grounds these decisions have been founded; there can be no reason for disbelieving the story related by Alex. Benedictus, in his book "de Peste," of the feather-bed which retained infectious properties during seven years, when we are called upon to suppose the same circumstance possible during a period of seven weeks. If it be once allowed that the much-dreaded fomites can attach themselves to and reside in any substance, whether animate or inanimate, I do not see how any limit is to be fixed to the term of their residence; if once they take up their habitation, I imagine they will not be, in the general acceptance of the term—tenants at will. If this be true, nothing short of an *eternal* quarantine can *positively* secure against the communication of the infection. Under these circumstances, the time cannot, I hope, be

far distant when a clearer view of the subjects of contagion and quarantine, than habitual fears and long-standing prejudices at present permit, will be adopted by the governments now most zealous in maintaining rigorous measures, erroneously supposed capable of preventing the danger, even if it did exist, or of averting a calamity which derives its origin from causes not to be controlled by the operation of the quarantine laws.

I am, sir,
Your obedient servant,
D. W. NASH.

1, North-place, Cheltenham, Nov. 24.

HYDROPHOBIA—RECOVERY.

To the Editor of the Medical Gazette.

SIR,

I AM anxious to lay the following case before the profession, and especially as there was amongst the medical attendants great variety of opinion with regard to its nature. One would have it to be mania, another inflammation of the brain: *ramollissement* of, and tumor in the brain, were likewise mentioned. With regard to the opinion I formed of its nature, I deem it right to say, that I relate only what I myself saw, as during the three first days I scarcely ever left the patient.—I remain, sir,

Your humble servant,
WILLIAM DU NEAUME,
M.R.C.S.L.

Jersey, Dec. 7, 1837.

On Saturday, Feb. 11th, at 10 P.M. I was called to see Mr. L. F. "who," I was told, "was dying." I found him in a sitting posture, supported by pillows. His face was flushed and his eyes shut, but, on being opened, the pupils were sensible to the stimulus of light. His respiration was *convulsive*, and the pulse quick. He continually repeated the word "*achève*." He had been in this state some minutes. The history I had of the case was this:—He had returned from sea a fortnight since, and during the latter part of the voyage had suffered much from anxiety of mind. His friends had found him quite altered; he was lonely, and shunned society; he was sleepless, and

wished to be always walking. He had before experienced a few slight convulsions, and he could exactly tell how and where he had been seized, and what he had done during the paroxysm. He now occasionally spit out his saliva, and turned violently on his abdomen, making efforts as if to throw it off by vomiting. I immediately applied twenty-four leeches to his temples, and occasionally ammonia to his nostrils, and ordered him to have a turpentine enema. After being in this state twenty or thirty minutes, he repeatedly sighed, and (as it appeared) gradually recovered himself. I was, however, surprised to find that he had all along been in his perfect senses. He had heard and recollected, and when his eyes were opened he had seen every thing about him. He was also sensible that he had continued repeating the word "achève," which was part of a prayer he was saying when the paroxysm came on. I left him at twelve. He expressed himself much relieved; indeed he seemed quite well.

12th.—At five in the morning I was called, as he was again in the same state; the chief symptom being, as in the former paroxysm, that the respiration was convulsive. I again occasionally applied ammonia to his nostrils, and, when he could swallow, gave him a few drops of liquor ammoniac in water. I ordered cold applications to the head, and an effervescing draught to be taken. At 8, I found that he had remained well since 5 o'clock. His face was still flushed, and pulse quick. As he was much troubled with flatus, I ordered an assafoetida enema.—I must now describe his state between the paroxysms. He always lay on his back, with his eyes widely and constantly open and turned upwards; the pupils were dilated, but naturally contractile. When a question was put to him, he always answered correctly. He appeared engaged in deep thought. When told to shut his eyes, he would do so, but he could not keep them shut. When a paroxysm of convulsions came on, it was generally announced by his spitting out his saliva with violence; his countenance becoming exceedingly animated, and his mental faculties much increased.

I applied a blister to the calf of each leg. The cold applications to the head were continued, and during the day he had several purgative enemata.

He had no paroxysm during this day, but at 9 p.m. he became much excited. This I attributed to the irritation of the blisters, as the slightest touch of the blistered part greatly increased the excitement. I could not take off the blisters without the assistance of several friends. His appearance was now much altered; his countenance was exceedingly animated, and his eyes brilliant; his manner was also remarkable. I now offered to him some water to drink, but the instant it was presented to him he was convulsed, and violently gnashed with his teeth. Soon after, Mr. C., who passed the night with me beside him, brought the basin near him, in order to wet the cloth about his head; he was again convulsed, and tried as if to plunge violently into the water. I applied twenty-four more leeches to his temples, and ordered cold applications to be continued to the head. I stayed with him the whole night. He had numerous paroxysms, during which his mind appeared to be perfectly lucid, as he requested us by name to hold him, and he wished us to stay always one at each side and another at his feet. Of the approach of a paroxysm he was always sensible, and on one occasion that only one of the attendants was with him, he requested the others to be called, "as one alone," he said, "could not hold him."

13th.—The countenance of the patient was still more animated; the face flushed; the eyes more brilliant and red; the pulse rather quick; the tongue natural. He complains of a burning pain in the chest. He is quite rational, but very irritable. It was agreed, in consultation, that he should have the following mixture:—

℞ Tinct. Opii, ℥℥. ; Vin. Antim. ʒiij.;
Spiritus Ammon. ʒj.; Mist. Camph.
ʒviis. M. Capiat cochleare magnum
2dâ quâq. horâ.

I continued watching the case closely, as there appeared to me something uncommon in the symptoms. There was no delirium whatever, but his mental faculties were wonderfully increased. He could hear the lowest whisper; the smallest cinder falling would make him start up; the noise made by the water falling when the cloth was dipped into it, was insupportable; a carriage passing threw him into a convulsion; his

vision was also wonderfully increased. Sometimes the dread of water was exceedingly well marked, at others not so. He could at times manage to get down a little, but that little seemed to distress him greatly. He was asked if he would like an orange. Having expressed a wish to have one, it was given to him, with the top cut off; it was curious to observe him bring it to his lips, but the instant the moisture touched them he was convulsed. This was well marked. He himself asked "how it was that when he brought the orange to his lips he was so pulled back?" On inquiring, I found that two years ago he was bitten by his own Newfoundland dog, in the upper part of the left arm: the dog was then greatly irritated, but it was stated that it was alive and well. He had since often complained of pain in the part, and there was now, at the bitten part, some redness; which, however, might have resulted from his being held by that part during the convulsions; and that arm was throughout observed to be more convulsed than the other. At this time the bitten part could not be touched without inducing convulsive twitches, and the patient himself asked why this occurred when that arm was touched, and not the other. His appearance was now frightful; his tongue was greatly protruded, and his jaws were incessantly moving; he was continually spitting out his saliva, which it was evident he could not at all swallow. Sometimes when asked if he would have a drink, he would say, "I wish I could drink, I know it would do me good, but I cannot." During this day he had several paroxysms; in the afternoon a straight-waistcoat was put on, not because he evinced any disposition to do harm, but because it enabled fewer hands to hold him during the paroxysms: those who were often or constantly with him, observed throughout that he was anxious not to do any harm.

At 9 p.m. it was decided in consultation that he should be largely bled, and that he should have the following draught:—

R. M. Camphoræ ʒiiss.; Tinct. Digitalis, ℞x.; Acid. Hydrocyanici, ℥ij.; Sol. Acetat. Morphię, ℥v. M. Fiat haustus. Capiat haustum statim, et repetatur 2da quaque hora ad sex vices si opus sit.

A vein was opened in the arm, and it

was not closed till the blood ceased of itself to flow. He took the first draught at 10; at 12 he was pulseless; at half after 12 the pulse was again perceptible; he then took the second draught.

Feb. 14th.—He had no convulsion during the night; the bandage about his arm having become loose, he had lost some more blood. He had taken five of the draughts. He was now pulseless and insensible, with extremities cold. At 12 he had a violent paroxysm, which lasted fully two hours; after this he had only a few, and very slight. It was not until this period that his mental faculties were impaired. Before, he spoke much about religion, and quite rationally, but not so now.

He was salivated; had a blister to the scalp, and occasionally leeches to the head; under which treatment he gradually recovered.

One of the medical attendants gravely asserted, "that it was because his throat was dry that he could not drink." The following fact, however, shows that his aversion to fluids did not result simply from the difficulty experienced in swallowing them. He would occasionally ask to smell Eau de Cologne; at times it was grateful, but at others insupportable. I could not at first account for this; but I observed that when it was brought near him, he carefully turned his eyes away from it, and as long as he did not see it, it was grateful; but if it was so placed that he could not avoid seeing it, or if it was shaken before his eyes, then he could not bear it. This I repeatedly tried, and also Mr. L. B., and always with similar results. With regard to the pulse and tongue, they give little information: the pulse was quicker than natural, and the tongue clean throughout; the bowels were all along obstinately constipated—so much so, that twelve drops of croton oil did not affect them. I must not omit that he always wished the persons most dear to him to keep at a distance from him, being evidently apprehensive of doing them harm.

Now I would ask, if this be not a case of rabies, what is it? I am aware that it may be objected to its being rabies, that the dog was only irritated, and not rabid; that so great a lapse of time intervened between the bite and the occurrence of the above symptoms; that the dread of water, though generally well and strongly marked, was occasionally absent; and lastly, that

the patient has recovered, for rabies is deemed incurable. If, indeed, it be allowed that a dog greatly irritated can communicate rabies, the last objection falls to the ground; for can we conceive it possible that a dog should communicate rabies if at the time it be not rabid? If, then, in one single instance, a dog which remained afterwards well has communicated the disease, I think we are bound to consider it curable, at least in the dog. Dr. Colles, in his lectures, mentions a case which occurred to him. A lady, on seeing her lap-dog attacked by another dog, ordered her footman to separate them; in doing so, the man was bit in the thumb. Both the dogs were chained up, and continued well, yet the man died of rabies. Now I do not believe the powers of imagination so great as to cause the peculiar symptoms of true rabies, though they might cause hydrophobia.

I may observe, that those who only saw the patient occasionally, or for a short time, might easily believe him out of his senses; but all those who were with him constantly are agreed that he was not mad, but in his perfect senses; his manner being very remarkable, but quite different from what occurs in delirium or mania. I shall offer no apology for describing this case so minutely, for I consider it interesting, whether it be regarded as one of rabies (which I believe it to have been) or not.

[We should like to have further particulars about this very interesting case; particularly as to the mode in which the "salivation" was produced.—E.G.]

CASE OF

RETENTION OF URINE,

FROM TUMORS WITHIN THE BLADDER;
WITH A SACCULATED BLADDER; AND A
LARGE ACCUMULATION OF CALCULI.

To the Editor of the Medical Gazette.

SIR,

In the second volume of your journal, I published the case of a patient, who, during a severe illness, was found to have an unnatural enlargement connected with the bladder, in which the urine had been so long retained as seriously to endanger his life; but where the quantity of water passed, and the absence of the swelling above the pubes usual in cases of distended bladder, de-

layed, to a very late period, the employment of the catheter.

The remarkable circumstance in the case, as stated in the memoir referred to, was, a tumor in the abdomen "of an oblong form, situated in the right hypochondrium near the outer edge of the rectus muscle, extending nearly from the eleventh rib to the right side of the symphysis pubis, and being particularly prominent about the situation of the inner abdominal ring. It somewhat distended the integuments, so as to be perceptible to the eye, and might be considered to be about three inches in width." It is further stated, that the swelling corresponded with the description of no kind of hernia; was not elastic, betrayed no fluctuation, seemed to possess considerable solidity, and was not tender to the touch. The introduction of the catheter emptied the tumor of its urinary contents, and the patient rapidly recovered. The conclusion I came to was, that the case was one of preternaturally enlarged bladder; the mucous membrane having been pressed by the accumulation of urine, through the muscular coat; thus forming an enlarged sac through right hypochondrium.

Before this illness, Mr. Jones, the subject of the communication, had occasionally experienced some difficulty in passing his water; a bougie was introduced to ascertain if stricture existed, and the condition of the prostate gland was examined by the rectum, but no adequate cause for any obstruction to the free discharge of urine could be detected. On his recovery from this attack, which was in November 1827, he was not able to make water; it was evident that either some mechanical obstruction to the exit of the contents of the bladder existed, or that there was a complete atony of its muscular coat; there was no obstacle to the introduction of a catheter, nor was any calculus discoverable. He was taught to employ the catheter himself, and found it necessary to introduce it from four to six times in the twenty-four hours. At first he used a silver instrument, but afterwards gave preference to the elastic gum catheter. His health, which before his illness was not robust, was more impaired after that period: he was not often confined to bed, but complained of debility, and latterly had found walking fatiguing.

For the last twelve months his health had more declined: he was compelled to employ the catheter more frequently.

At times the drawing off the water was very painful to him; on which occasions it was scanty, and contained much purulent mucus. Sometimes small scales of calculeous matter stuck in the apertures at the point of the catheter, and rendered its withdrawal painful; and whenever a metallic instrument was used for some months before his death, it was felt to grate against a calculus.

He was under the necessity of taking opium for a considerable time, though not in large quantity; latterly he took a tea spoonful of the tincture at bed-time, and a small quantity once or twice in the day. He continued to walk to a short distance almost daily until the beginning of the present year.

On the 7th of February, 1837, he was seized with the usual symptoms of influenza, which was then universal in this city, in a severe form. Bronchial inflammation came on: he became much exhausted by the complaint; and the disorder of the bladder was more troublesome, the urine requiring to be drawn off every hour. The catarrhal symptoms, however, subsided, but he did not rally from the attack, and died in eleven days from the commencement of influenza, in the 66th year of his age.

As no objection was made to a post-mortem examination, the opportunity was not neglected of ascertaining, as far as was practicable, the original cause of impediment to the exit of the urine from the bladder; the nature of the distended sac, which was so remarkable a feature during the illness of 1827, and the condition of a bladder which, for nine years, had never evacuated its contents by its own muscular efforts.

On opening the abdomen, the fundus of the bladder was seen above the pubes, apparently distended with urine, but really containing very little: its coats were thickened, and its surface irregular, several rounded prominences being observed upon it. These projections, upon being felt, were found to contain calculi. During the unavoidable moving of the bladder, in the progress of the dissection, the calculi fell out of their cysts, and were heard to rattle within the bladder when it was at all inverted.

The object in this examination was to remove the bladder entire with the ureters, that these parts might afterwards undergo a careful inspection. On separating the connexions on the right side, on a level with the fundus of the

bladder, a cavity was unexpectedly penetrated, from which a gush of three or four ounces of clear urine took place; this proved to be the right ureter greatly distended, and brought much nearer to the parietes of the abdomen than was natural. The ureter was then divided at the part where it was wounded, and the portion connected with the bladder was removed with that viscus, as well as the left ureter, throughout its whole length. The right ureter was then traced to the pelvis of the kidney, and taken out with a part of the kidney. The right kidney shewed no peculiarity; the left was not minutely examined.

The appearances of the removed parts were as follows:—

The right ureter was much thickened in its coats, and enlarged in its calibre; the little finger could be introduced into it. At its connexion with the bladder it was particularly thick; and the bladder, at the point where this canal entered it, felt almost cartilaginous.

The left ureter was a little larger than natural.

The bladder and prostate gland were slit open from the caput Gallinaginis to the fundus. The prostate gland was increased to the size of a small orange. Attached to the prostate within the bladder were three distinct tuberculated, semi-cartilaginous excrescences: two of them were on the left side of the urethra; one was very prominent and loosely adhered by a narrow neck, and was about the size of a small hazelnut; the next, more in contact with the urethra, was less circular, and appeared more like an irregular projection of the gland itself. The third, on the right side, was the largest tubercle, and was firmly attached to the prostate. When the divided portions of the prostate were put in apposition, it was clear that these tumors entirely closed the aperture into the urethra from the bladder; the looser, and rounder one, might be aptly compared to the bullet valve of the stomach pump.

Within the bladder an extraordinary number of calculi presented themselves, of all sizes, from that of a moderate Spanish chestnut, to the dimensions of a small sweet-pea seed. Many of these were still embedded in pouches formed between the muscular fibres of the bladder; and similar cavities, from which other stones had escaped, were very numerous.

Of those stones that were removed, there were thirteen weighing, when dry, from four drachms and a half to ten grains, and about fifty smaller ones, weighing together two drachms and a half: their total weight was rather less than four ounces. Some of the smaller stones were so embedded in their pouches, that they could not be removed without squeezing them out, in consequence of their being larger than the mouth of the cavity containing them: a proof that these at least were formed in the part where they were found. In those parts of the bladder where they were embedded, the muscular fibres were so strongly marked as to give the surface very much the appearance of the fleshy bands and pillows found within the ventricles of the heart.

The valvular apertures where the ureters terminate in the bladder, were previous and natural.

A peculiarity in this case was, the uniformity in shape of nearly all the calculi, which were, with scarcely an exception, tetrahedral, even the minutest exhibiting the same appearance. Very few shewed any marks of attrition; and several were so situated as to be protected from rubbing against others, by their firm inclosure in their respective sacs. These calculi therefore exhibited their original form, having suffered no disturbance from contractions of the bladder, or attrition with other calculi. To any other cause than the effect of the muscular fibres in modifying their form, I am unable to attribute their angular shape, but it does not seem very easy to determine why so much similarity of form should have existed. How far this shape is common to calculi thus deposited in sacs, and excluded from friction with other stones, I am unable to say. In the beautiful collection of calculi in Mr. Richard Smith's museum in the Bristol Infirmary, consisting of four hundred specimens, I do not find one resembling them. They approximate most in figure to some biliary concretions, but the smooth sides of such formations seem to depend upon attrition within the gall-bladder. At what period these concretions began to be formed it is impossible to determine; had any existed at the time when the inability to void urine became confirmed, it is not probable that they would have escaped detection during the various explorations of the bladder which were

had recourse to, or the frequent introduction of the metallic catheter at that time employed. For several years he used elastic gum instruments, and had rarely occasion for surgical assistance in the employment of them.

The calculi, according to the analysis of a friend, consist almost entirely of phosphate of lime: a white sand, which coats most of the specimens, and is found to intervene between some of the layers of the solid parts, is uric acid.

Upon reviewing the symptoms in this case, and comparing them with the post-mortem appearances, there seems little room for doubting that the distended sac, which was so remarkable a feature during the patient's illness in 1827, was formed by the right ureter. No part of the bladder itself afforded any evidence of preternatural elongation: and though the ureter was now not more than a fourth part of the size of that tumor, the thickened state of the lower part of the tube, and of the bladder at the point where this canal opens into it, favour the probability of its having been formerly in a state of considerable distension.

The case presents an interesting example of a bladder which for nine years had never evacuated its contents by natural efforts, and of the changes produced within its cavity by so unusual a condition. The calculi lodged in the numerous sacculated cavities of the bladder, confirm the opinion of many writers, that such concretions are often deposited from the mucous secretion of that viscus, and not entirely from the urine: they were found to be as abundant at the fundus (where probably little urine reached them, from the bladder never being very full,) as they were at its lowest part near the neck; and many were lodged in little hollows of the mucous membrane, with only a very small opening communicating with the cavity of the bladder.

It was remarkable that the presence of so large an accumulation of foreign matter in the bladder produced no very material aggravation of the patient's inconvenience. More irritation than had formerly existed certainly supervened during the last year or two; purulent matter was often drawn off, and a more frequent introduction of the catheter was required; but as the coats of the bladder did not contract upon the stones, the ordinary sufferings of this

distressing complaint were not experienced.

The propriety of recommending the patient to undergo an operation for the removal of the calculi, was of course duly considered; but no hesitation was felt in the decision that, in such a case, it was inadmissible.—I am, sir,

Your obedient servant,

J. B. ESTLIN.

Bristol, December 15, 1837.

DIAGNOSIS IN ADHERENT PERICARDIUM.

To the Editor of the Medical Gazette.

SIR,

I TAKE leave to inclose herewith the case alluded to in my last communication, of adhesion between the heart and pericardium: ascertained by symptoms during life, and demonstrated in the examination of the dead body.

I have the honour to remain,

Your obedient and obliged servant,

THOS. WATSON.

Henrietta-street, Cavendish-square,
Dec. 23, 1837.

On the 11th of October, 1836, I was asked by my colleague, Dr. Sweatman, to see a patient with him, in St. John's Wood:—a Mr. S.

I was introduced to a gentleman, 39 years old, with a pale face and sharp thin features, sitting up in bed, breathing shortly and laboriously. His legs were anasarcaous; and his belly was tense and fluctuating. I had been previously informed that for years he had been given up to intemperance in drinking, and to indolent and low habits of life. Twice or thrice he had been affected with delirium tremens.

He complained to me that he was troubled with wind, shooting upwards through all the left side of his chest: on further inquiry I found he meant that he had much pain there.

A diffused sibilous wheezing was audible in the upper portions of the lungs on both sides: both sides were dull on percussion, in front and behind, at their lower part;—and on the right side no sound of respiration could be heard in that part. The jugular veins were swollen and tortuous. His pulse was frequent, and very feeble.

On applying my ear to the præcordial

region, I was immediately sensible of a very loud and harsh *to and fro* sound; a noise of rubbing, apparently close to the ribs, drowning all other sound in that space, keeping time with the alternate motions of the heart, and equally manifest when the patient held his breath. This sound was distinctly perceived by Dr. Sweatman.

I stated to him my conviction that our patient was labouring under recent and acute pericarditis. I concluded that he also had hydrothorax, and that, with respect to the heart, there was dilatation of its right cavities at least.

I ventured to predict that either Mr. S. would speedily die, while the rubbing sound continued, in which case the pericardium would be found inflamed, but unadherent, or only partially adherent, to the heart:—or, if he survived a few days, the remarkable sound of friction would altogether cease, which would indicate, with certainty, that adhesion had taken place.

He had been suffering in the same manner for three or four days; the pain in the left side, and (in all probability) the inflammation of the pericardium, having commenced on the 8th of October.

In the spring of that year, after having from indolence kept within the house for several months, he had crossed from the Isle of Man to Liverpool, and suffered a good deal from sea-sickness during the passage. As soon as he landed, he set out, in company with two other gentlemen, to walk a distance somewhat exceeding a mile. His companions, engaged in conversation, inadvertently outstripped him; and after some time they were called back, and found him supporting himself against a wall, very pale, greatly distressed in his breathing, and unable for some time to proceed. He attributed all his subsequent complaints to the over-exertion he then made; for he had been ailing thenceforward, though not confined to the house, till the attack on the 8th of October.

Mercury and some diuretic medicines were prescribed, and leeches were applied to the painful side.

I visited Mr. S. again on the 15th. He had been greatly relieved by the treatment adopted, being free from pain, and less swelled. The *to and fro* sound, however, continued; but it was less harsh, and confined to a smaller space.

On the 20th I saw him for the third and last time alive. The rubbing noise was entirely gone, and a dull, systolic, deep-seated bellows sound, had taken its place. He was making a vast quantity of urine, the dropsical affection had greatly diminished, and he professed himself to feel much better; but his pulse was scarcely perceptible.

On the 31st it was noticed that he spoke in an odd and unusually loud tone, and his pulse, as Dr. Sweatman informed me, could not be felt; but he said he suffered no pain, and he seemed to breathe with tolerable ease.

In the course of that evening he sat up in bed to swallow a dose of medicine; and having done so, he leaned his head against the nurse, who was supporting him, and presently expired.

The body was opened the next day by Mr. Shaw, in the presence of Dr. Sweatman and myself. I did not hesitate to express my certain persuasion that the heart and pericardium were adherent.

The abdomen was first examined. It contained a large quantity of clear serous fluid. The liver was covered with a thin layer of reticulated lymph, and connected by a few slender and soft strings of lymph to the diaphragm.

There was much fluid in each pleura also. The right lung occupied exclusively the upper part of the cavity, its inferior margin being turned upwards, and fixed by a strong band of adhesion, half an inch in length, to one of the ribs, about midway between the diaphragm and the clavicle; so that the fluid must have been poured out, and have floated the edge of the lung to that height before it adhered.

The left pleura was quite healthy.

The heart was large. The cavity of the pericardium was entirely obliterated, the opposed serous surfaces of the membrane being united by means of a thick layer of soft mottled lymph. They were easily separated by very gentle traction.

All the cavities of the heart were larger than natural; those of the right side were much dilated; the left were filled with dark clotted blood; the walls of the ventricles being considerably thickened. Both the mitral and the aortic valves were morbidly thick and stiff, but there were no "vegetations."

The aorta expanded, just beyond its valves, into a small pouch, in some spots of which circular depressions were visible. Here the coats of the vessel were thinner than elsewhere, and translucent, presenting apparently the first steps towards aneurism. At a greater distance from its mouth, the surface of the aorta was rendered irregular by small opaque, yellow patches, projecting its inner membrane.

To make the case complete, I have given the history and the dissection in full; but its chief interest consists in the confirmation afforded, by the inspection of the dead body, to the special diagnosis derived from the auscultatory symptoms during life.

The existence of the *to and fro* sound was good evidence (as I thought) that the pericardium was made rough by inflammation; the total cessation of that sound was to me a sign that the pericardium had become fixed, more or less extensively, to the surface of the heart, and the further continuance of the rubbing thus rendered physically impossible. In many previous instances, reflection upon this succession of phenomena had satisfied my mind as to the state of the parts within the chest. On this, and on one former occasion (see *MED. GAZ.* vol. xviii. pp. 701, &c.), an early examination of the affected organ—the actual spectacle of the serous surfaces glued together by tender and recent lymph—brought the force of demonstration to that which before had only been a matter of circumstantial evidence.

I may add, that my colleague, Dr. Wilson, has just dismissed from the hospital a lad, in whom, during an attack of acute rheumatism, a well-marked *to and fro* sound arose, continued a few days, then ceased entirely, and was succeeded by a strong systolic bellows or cooing sound. Dr. Wilson showed me also, some days ago, a young woman, in whom, as I well remembered, the same rubbing sound had been equally distinct and temporary, in a severe attack of the same disease which she suffered in the hospital two years before. She has now the appearance of perfect health, and the cardiac sounds are not unnatural. But there can be no reasonable doubt that, in both these persons, the pericardium is adherent to the heart; and such cases are common.

MEDICAL GAZETTE.

Saturday, December 30, 1837.

“Licet omnibus, licet etiam mihi, dignitatem
Artis Medicæ tueri: potestas modo veniendi in
publicum sit, dicendi periculum non recuso.”

CICERO.

A CASE OF LIFE-INSURANCE.

A REMARKABLE trial, lasting three days, which turned upon the validity of a policy of life-insurance, lately took place in the Court of Queen's Bench. We take the report from the *Times* of December 15th.

An action was brought by the Eagle against the Atlas Insurance Company, to recover 4000*l.* upon a policy effected by the former company upon the life of Mr. John Cockrane. The life of this gentleman was insured on the 24th of September, 1834, for four years. The Eagle Office gave him 12,000*l.*, on condition of receiving 16,000*l.* if he survived the above time; and having advanced the whole of the money in the first instance, insured his life in the Atlas, the Hope, the Promoter, and the Union Offices. Mr. Cockrane died in April 1835. The Sun alone paid the sum insured, the others refusing on the grounds—1st, that at the time the insurance was effected, Mr. Cockrane was afflicted with a disease tending to shorten life; 2dly, that his habits were those of a confirmed drunkard; and 3dly, that these facts had been concealed, by which circumstance the policy became void.

The first witness called was Mr. H. Porter Smith, the actuary of the Eagle Insurance Company, who deposed that, in 1834, an application was made to lend Mr. J. Cockrane a sum which involved an insurance of 12,000*l.* for four years; that he was examined by Dr. Spurgin and Mr. Sayner, the medical officers of the institution, and reported

an eligible life; that as the Eagle does not insure more than 5000*l.* on one life, he gave Mr. Cockrane a note to the Atlas, and soon received an answer accepting the proposal. Mr. H. P. Smith applied in the same way to the Economic; but on the 22d of September, Mr. Downes, of that office came, and said that there was an objection to the life.

Mr. Sayner and Dr. Spurgin were the next witnesses, and deposed to having examined Mr. J. Cockrane in September 1834, and having considered him an unexceptionable life.

Then came Mr. Bennett, the medical attendant of the deceased, who stated that, in September 1834, Mr. Cockrane was in good health. He gave answers to the office at the request of Major Williams. Among other things Mr. Bennett is made to say, “I have heard people say that he was given to drinking, but I do not know that it was the common talk, and never considered his habits intemperate.”

A cousin of Mr. Cockrane's was next examined, who did not visit him; he stated that he had no appearance of being addicted to drinking.

Then there comes one Rickman, a news-keeper, who confesses, on cross-examination, that he has seen Mr. Cockrane take shrub and peppermint at all hours, perhaps five or six times in the course of the day, according to the distance they went when driving out together; and that he had also two or three houses in the neighbourhood at which he would take a glass of ale, or sherry and water, before going out. He has seen Mr. Cockrane's servants dine with him at his own table, and believes they did so every day.

On re-examination, Rickman says, with a coolness worthy of a higher station, “Cockrane never appeared to me to take any thing to hurt a man. I

never saw him drink more than the rest of the company. I only saw him intoxicated fifty or sixty times in four years. His health did not seem to be impaired by what he drank."

After the testimony of another cousin to the goodness of the constitution of the deceased, we come to the very curious evidence of Frederick Hammond, his groom. He stated that he had "seen him tipsy a hundred times, perhaps, but not beastly drunk." When he was going about he drank sherry and water, brandy and water, ale, rum, shrub, and gin and peppermint; it did not matter what. He would sometimes lie in bed for three or four days at a time, and then he had liquors in his bed-room, and the servants drank theirs also in his sleeping-room—"we all had our song together."

He adds, "Mr. Bennett has seen us in the bed-room with the liquor. * * There was always liquor about when my master was at home. I have heard Mr. Bennett say it was a pity that Mr. Cockrane drank, and that it was his rattling about so that made him ill."

The depositions of a number of persons residing at Paris and Calais, at the latter of which places Mr. Cockrane died, were now read. One of the witnesses deposed that the death was caused by severe inflammation of the lungs; the others stated that he was an insurable life.

The evidence of another witness, stating that the appearance of Mr. Cockrane, in 1834, was that of a strong healthy young man, and that "he looked in the face something like a strong Welch girl, as he had no whiskers," closed the plaintiff's case.

Mr. Serjeant Wilde, who now addressed the jury for the defendant, observed, that even the case as set forth by the plaintiff's witnesses, must decide their verdict in favour of the defendant; but that if any doubt remained in their

minds, it would be totally removed by the evidence which he should bring forward. He abandoned the first and second pleas, which alledged that at the time of effecting the insurance, Mr. J. Cockrane was afflicted with a disease tending to shorten life; but he contended that his habits of life were known to the Eagle office through a communication made to them in the report by Mr. Travers, examining surgeon to the Economie; and that as these facts had been concealed by them from the defendant, the policy was void, and his client entitled to a verdict.

Among the numerous witnesses called by the learned Serjeant, Dennis Wooding deposed that, in 1833 and 1834, Mr. Cockrane "was almost always muddling; his eyes were ready to start from his head, and his hands shook so that he could not sometimes write his name for two or three days." Mr. Missing, a barrister, deposed to Mr. Cockrane's coming to his house to dine in a tipsy state, so that he could not eat, but drank only.

A publican, whose house was frequented by the deceased, states that he had frequently seen him intoxicated, and that Mr. Bennett remarked he was taking all the pains he could to kill himself.

Mr. Russell, who kept the George, at Kingston, deposed that the deceased once came to his house with Rickman, and died there. "He went to bed soon after dinner; and shortly after he went to bed, some musicians played under the window. He rang for his servant, and I saw them dancing about the room together. He was intoxicated. He got up the same evening, and they all left together. This took place the latter end of September 1834." On cross-examination, the witness stated that Mr. Cockrane associated so much with his servants, that, for the first two years, he thought they were all grooms.

Another witness, ostler to Rickman, says that Cockrane used to get the better of the liquor he had taken at night by repeating it in the morning. Witness dined with the deceased in 1834, the remainder of the party consisting of five servants.

Mr. Travers visited him at home, in bed, at half-past 4, in a state of stupid intoxication, and then went to see Mr. Bennett. His medical attendants led him to suppose that his habits were not confirmed, but that he was only occasionally in liquor. Mr. Travers, however, suspecting that all was not right, made a special report, and the directors of the Economic rejected the life.

Mr. Aston Key saw Mr. Cockrane, at the Promoter office, on the 24th September, 1834. His first impression was that he was under the influence of liquor, but his opinion was altered by the certificates of Mr. Bennett and Mr. Neale*.

We have been compelled, by want of room, to abridge the evidence extremely; yet we hope we have retained enough to make the case intelligible. Young Cockrane appears to have had an invincible taste for low company; sometimes drinking spirits with publicans and mews-men, sometimes dancing about his room, or playing at skittles, with his servants, and at all times enacting the part of a groom, with a fidelity unrivalled on any stage; so that for two whole years he deceived a most practised eye, and was the very character for which he dressed! As for the drinking part, most of the witnesses whom we have quoted, and many whom we have not had room to quote, testify to his being continually tipsy, flustered, or drunk. From patrician Champagne down to plebeian gin, nothing came amiss to him; "from morn

to dewy eve," rum-and-milk, ale, brandy-and-water, shrub, and wine, had their turns.

"He certainly does appear," says Lord Denman, "to have been one of the most wretched young men one has ever heard described. Well born, without a single decent habit; well connected, without one respectable companion; the young son of a widowed mother, without any appearance of feeling or affection."

In a medico-legal point of view, the case presents some difficulties.

For the plaintiff it might possibly be argued, in the first place, that Cockrane's life was really an insurable one, and would have been so, even if all had been originally known that came out upon the trial; that the drunkenness which renders a life uninsurable must be an habitual and deep intoxication, and not a mere habit of muddling; that if all habitual deviation from extreme sobriety rendered a policy void, life-insurance must be at an end, or confined to *tea-totallers*; just as if we strictly interpreted the warranty that the insurer is not liable to any disease tending to shorten life, we should exclude all who were subject to headache, dyspepsia, or winter-cough, *i. e.* ninety-nine in a hundred.

In the second place, it might be urged for the plaintiff, that he had no knowledge of the facts now produced; that Mr. Bennett, the medical attendant of the deceased, positively certified that he was not intemperate; that Dr. Spurgin and Mr. Sayner were both satisfied with his appearance; and that the temporary doubt created by Mr. Travers's report was speedily cleared up by the explanation given by Mr. Bennett, namely, that Mr. Cockrane's confused appearance and short answers arose from grief, occasioned by the recent death of his mother.

To this it might be replied for the defendant, in the first place, that Cock-

* Lord Denman, in his charge to the jury, asks, "Who is Neale?"

rance's life was not substantially and truly an insurable one: that Mr Aston Key, after whose "not very favourable" report the life was accepted by the Promoter, says in his evidence, that if he had heard at the time one-tenth of what he has heard during the trial, he should have said the life was not insurable; that to make a policy void, it is not necessary that the drunkenness should be of the kind called "bestly" by one of the witnesses, but that the constant muddling which disorganizes the brain, the liver, and the heart, is quite sufficient; that two cases in which it was proved that habits of intoxication were concealed from the insurers, although the individuals were at the time apparently hale and healthy, have been decided against the plaintiffs*; and that a habit, scarcely if at all compatible with long life, ought not to be compared with dyspepsia and other ailments which, in many cases, do not sensibly affect the duration of existence. And, in the second place, even if we do not regard the facts of the case as they now appear in all their deformity, but as they might reasonably appear to an impartial judge in September 1834, we have the very unfavourable certificate of Mr. Travers, and the not very favourable one of Mr. Aston Key, which, if communicated, would have settled the business, and prevented any further insurances.

Lord Denman charged the jury for the defendant, on the ground of the certificates given by Messrs. Bennett and Neale having been contrary to plain facts which must have been well known to them: the jury found for the plaintiff, probably believing that though the certificates were far from being correct, the plaintiff had not sufficient ground for distrusting them, more especially as Lord Denman allowed that the reports

made by medical examiners to their officers were confidential communications, not to be divulged.

The point, it must be owned, is a dubious one; and we shall not be surprised to hear of an application for a new trial.

CASE OF GLANDERS IN THE HUMAN SUBJECT.

To the Editor of the Medical Gazette.

SIR,

THE inclosed is the history of a case of *Glanders*, copied from my note-book, which occurred in a patient of Mr. Morgan's; being one of his dressers, the case consequently came more directly under my notice. If deemed sufficiently interesting, I shall feel obliged by your inserting it in the next number of your valuable periodical.—I am, sir,

Your obedient servant,

J. G. DA. C. DENHAM.

Library, Guy's Hospital,
Dec. 12th, 1837.

Thomas Worcester, æt. 21, of plethoric habit, was admitted into Philip ward, Guy's Hospital, on the 27th November, 1837, under the care of Mr. Morgan, labouring under acute synovitis. A short time after admission the following history was obtained from him:—He states that he is a carman, in the employment of Mr. Nichol, of Dowgate Hill; has enjoyed good health for the last four years. About a week prior to admission, it appears that he injured his left knee, by its becoming jarred between two barrels; for the first four days after the accident he experienced but little inconvenience; but early on the morning of the fifth day he awoke with excruciating pain in the knee, and on the same day applied at the surgery of the hospital, where he was seen by Mr. Rose, one of the dressers for the week, who gave him some medicine and cupped him on the knee, by which he was greatly relieved; but on the following day and night he suffered a great deal of pain, and on the next morning, being brought in a cart to the hospital, he was received into the above ward.

Nov. 27.—Present condition is as follows: Knee swollen, hot, and painful; countenance anxious; breathing hurried; tongue furred. Pulse 96; skin hot and dry; bowels costive. He was shortly seen by Mr. Morgan, who ordered the following:

* Beck's Elements of Medical Jurisprudence, 5th edit. p. 385.

R Hydr. Chlor. gr. i. cum Extr. Coloc. co. gr. vj. statim sumend. Haust Magn. Sulph. hor. post pil. Hirud. No. xx. parti dolent.

Leg to be kept semiflexed, and supported by pillows.

28th.—Bowels have been freely opened; knee is still very painful; did not sleep during the night; tongue continues furred. Pulse 98; skin hot and dry; he was ordered—

R Pulv. Jacob. gr. iv.; Hydr. Chlor. gr. ij.; Pulv. Opil. gr. i. statim cap. Hirud. xx. ut antea applicentur.

29th.—Much the same as yesterday.

Rep. Hirud. et Med.

30th.—Slept better last night; knee not quite so painful; complains of thirst; urine is high coloured; tongue still continues furred.

R Haust. Efferves. t. d.

R Julep. Menth. Pip. Ziss. cam Liq. Op. Sed. (Battley) nxxx. horâ somni, o. n.

Dec. 1st.—Is better to-day; knee continues swollen; slept very well last night; skin not so hot; pulse 96; tongue cleaner.

Rep. Med. Efferves.

2d.—Knee very painful; had an attack of rigors last night; tongue furred; skin hot; pulse 106; complains of sensation of chilliness over his body. Mr. Morgan ordered the leg to be kept in an extended position by means of a splint.

Rep. Medic. ut antea. Omit. Haust. Anod. h. s.

3d.—On entering the ward this morning, the nurse states that he had had a very restless night, and during sleep was constantly talking about horses. There is a slight discharge to-day from the right eye, and several faint spots seen over the face and body. The general swelling of the knee has greatly subsided; on the inner side a small abscess has formed, which has been opened, and a poultice applied.

4th.—The spots observed yesterday are much more visible to-day; the knee still continues discharging; skin hot; upper lid of right eye rather more swollen than it was yesterday. Mr. Morgan saw him at 1 p.m., and wished him to continue the effervescent medicine, and stated most probably we should in the evening be able to decide what the eruption was; at present there are several small red indurated points, one or two vesicular at their summits, which induced Mr. Morgan to suppose it might be a case of modified small-pox.

5th.—The right eye is very much swol-

len and painful; the discharge from it has increased; there is also a sero-purulent discharge from the nose. Pulse 120; bowels open; head painful; delirium at times present. The eruption has at present a good deal of the appearance of small pox; many are decided pustules, with an inflamed base. Skin is hot and dry; the knee is improving, but there is still a discharge of pus from it.

R Pulv. Jacobi, gr. iv.; Hydr. Chlor. gr. j. statim. Rep. Med. Efferves.

6th.—The right eye is completely closed; lid much swollen; and the forehead on the same side has a crimson appearance. The discharge from this eye is becoming purulent; the left eye is also becoming affected; there are several fresh spots about the arms and legs; skin still continues hot; head painful; he was delirious during the night. Bowels freely open; pulse 120, having a good deal of power; breathing hurried.

Rep. Pil. ut heri, cum Hydr. Chlor. gr. ij.

7th.—Has several fresh spots; those having first appeared are losing their small-pox character, although there are several which still resemble that eruption. There are this morning on the lower extremities several purple spots, about the size of a four-penny piece, the base of which can be felt indurated. Both eyes are now closed; there is great tumefaction over the forehead, accompanied with tension, heat, and pain. There is excessive discharge from both eyes, and also from the nose, and the right cheek is also slightly swollen; the discharge seems to be of a sero-purulent character. Pulse 120, and continues full; skin hot; tongue covered with a brown fur; passes both stools and urine involuntarily; motions are dark-coloured and offensive. Mr. Morgan ordered face, eyelids, and forehead, to be scarified, and the effervescent medicine to be repeated; poultices to be applied afterwards to the above parts.

8th, 10 A.M.—Discharge from eyes and nose has increased considerably; tongue very much swollen, and covered with a black fur. Shortly after yesterday's report, about half a cupful of dark blood came away from his mouth. The indurations already observed beneath the purple spots have increased in size. He was delirious during the night. Pulse is at present 110, feeble; respiration hurried; the integuments about the knee have commenced sloughing. Mr. Morgan saw him at half-past 1 p.m., and ordered the following:—

Port-wine poultice to knee.

R Julep. Ammon. ʒij. t. d.; Pulv. Ja-

cobi, gr. ij.; Hyd. Chlor. gr. ij. h. s.; and port-wine, beat up with eggs, to be given three or four times in the course of the day.

During the afternoon he was seen by Dr. Addison, who thought, from the present appearance and peculiarity of the eruption, that it resembled more than any thing the eruption peculiar to glanders; but not having seen a case of this disease, he stated that he could not confidently decide as to its nature.

5 P.M.—Continues much the same as in the morning; his pulse, if any difference, is rather quicker. The pulse 140; the indurated swellings above noticed have increased since the morning, and are now covered by an inflammatory blush. Mr. Key saw him at 9 o'clock P.M., who also coincided in the opinion that it resembled a case of glanders. Many of the hard swellings had by this time softened down. His breathing is now very laborious. From this time he gradually became worse, and died at a quarter past twelve.

Further particulars.—This being rather an interesting case, I was anxious to obtain a further history of the patient; consequently I called upon his mother, on Friday evening, the 8th instant, and obtained from her the following account:—She stated that her son had enjoyed very good health during the last four or five years; he had the small-pox when he was six years old, and at present he was in the employment of Mr. Nichol, of Dowgate Hill, for whom he was taking care of a sick horse; and also that she never heard him complain of having been *hit on the hand, or any other part of the body*, by the horse. This statement not being sufficiently satisfactory in my mind to account for the disease, I went, therefore, yesterday morning and called upon Mr. Nichol, from whom I obtained the following additional history. He stated that the patient had the charge of one of his horses, which had been ill for some time, but was now *rapidly recovering*. The veterinary surgeon had told him the horse had farcy. From Mr. N. I also learnt that the patient was, in his habits, very intemperate; so much so, indeed, that he was unable frequently to pay for his lodging, and was obliged consequently to sleep under the manger of the sick horse. This very horse happened to be on Mr. Nichol's premises when I called; I therefore saw him, and the only peculiarity I observed about him was a discharge, of a mucopurulent character, from the nose, and two or three swellings on the legs; from one of them there was a slight discharge.

Worcester's body was not examined after death.

ACCIDENTS ADMITTED AT THE LONDON HOSPITAL.

Mr. LUKE, Surgeon.

Mr. CURLING, Assistant-Surgeon.

Dec.	Sex.	Age.	Case.
5.	M.	13	Scald on the foot.
	M.	29	Contused back.
	M.	60	Sprained ankle.
	M.	24	Cut on the forehead (a.)
	F.	36	Contused foot.
6.	M.	2	Severe scald on the leg and body.
	M.	50	Strangulated inguinal hernia, reduced (b.)
7.	M.	24	Severe contusion of the hand.
	M.	16	Bite of a dog on the leg; parts excised.
	M.	41	Fractured ribs.
	M.	34	Severe inflammation of the knee-joint from a blow.
	F.	16	Fracture of the fibula.
8.	F.	21	Contused knee.
	F.	50	Strangulated femoral hernia; operation (c.)
	M.	45	Contused knee.
	M.	67	Contused knee.
9.	M.	60	Contused side.
	M.	36	Sprained ankle.
10.	M.	8	Contused scrotum.
	M.	32	Cut on the forehead.
	F.	26	Sprained ankle.
11.	M.	33	Fractured ribs.
	M.	27	Contused side.
12.	M.	17	Foot completely torn off by machinery (amputation).
	F.	5	Severe scald on the face and arms.
	M.	5	Severe burn on the face, body, and arms.
	F.	58	Burn on the neck.
	F.	40	Scald on the foot.

In-patients..... 28

Out-patients..... 34

Total during the week .. 62

(a.) This man quitted the hospital contrary to advice the day after admission, but returned again in three days, with erysipelas of the head and face, and was again taken in. It is a general rule at the hospital to admit all patients applying with cuts on the head, since, when treated as out-patients, in consequence of intemperance and exposure, they frequently return affected with erysipelas. The patient is doing well.

(b.) This patient was brought to the hospital from some distance in the country, by a surgeon who had made ineffectual

efforts at reduction. The hernia was reduced, however, in a short time, by one of the house-pupils.

(c.) *Femoral Hernia—Difficult Diagnosis—Operation.*

This patient was a female, aged 50, who was brought to the hospital, Saturday, Dec. 8th, in the evening, supposed to be labouring under strangulated hernia. She was weak, and gave indistinct replies to the questions put to her; she stated, however, that she had been subject to a swelling in the left groin for two years, but that it had become painful since Monday last, from which time no motion had passed from the bowels. There were two small tumors, about the size of hazel-nuts, moveable, round, and closely connected in the situation of the femoral ring; they were slightly painful on pressure, received no impulse on coughing, and at their base there was something like a narrow neck leading towards the ring. The abdomen was tense, tympanitic, and painful on pressure; the pulse small and quick. She had been sick before admission, but could now retain fluid upon her stomach. Considerable doubt being entertained as to the existence of hernia, she was ordered—

Magnes. Sulphat. ʒij. in Aq. Menth.
Piperit. donec alvus responderit; and a purgative clyster.

Dec. 9th.—About 2½ P.M. she had a motion, consisting of a quantity of hardened feces and the enema. In the morning, about 9, as she was unable to pass her water, it was drawn off, and the clyster repeated, but without being followed by evacuations. The purgative draught was omitted, but resumed again at 2 P.M.

9½ P.M.—She was evidently much worse. Vomiting; extremities cold; pulse almost imperceptible; bowels still constipated. It was now thought advisable to make an examination of the inguinal tumor; and the patient was operated on in the ward near the fire.

The two small tumors were found to consist of adipose matter, invested with condensed cellular tissue. Beneath, but closely adherent, was perceived a whitish membranous substance, which being divided, was found to be a hernial sac, with a very small portion of dark coloured intestine adherent to it. The neck of the sac was afterwards divided with a bistoury, to remove all constriction, and the wound was then closed. A quantity of turbid serum escaped from the abdomen after the opening of the sac. During the operation the woman was in such a state of collapse, that it became necessary to give her brandy; and she only survived it five hours.

The body was examined eleven hours after death. The peritonæum generally was found much injected. A small protruding portion of the ileum, like the end of a finger-glove, and of a dark colour, was adherent to the peritonæum at the femoral ring. This was the part met with in the operation; but about an inch and a half beyond there was an evident constriction of the whole calibre of the intestine, with thinning of the coats. Whilst handling the gut, a quantity of liquid feculent matter escaped from an ulcerated opening at the contracted part. There were several ounces of turbid serum, mixed with feculent matter, in the general cavity; but whether any part of the latter escaped during life, could not be satisfactorily determined. Heart, lungs, and liver, sound.

This case is of considerable interest, and of an instructive nature. The diagnosis was extremely difficult. An accurate history could not be obtained, as the woman gave conflicting statements. On one occasion she said that there had been a larger tumor when she was first taken ill, but it had subsequently gone away. This would agree with the morbid appearances, since it was found that the contracted and ulcerated part of the intestine was not strictured at the time of the operation. Indeed, there is every reason to believe that the intestine had been reduced before the patient's admission, although the ill effects of the stricture, ulceration and contraction of the intestine, adhesion to the sac, and peritoneal inflammation, remained to impede the functions of the intestine, and were ultimately the cause of death. Had not the two adipose tumors been accidentally developed on the exterior of the sac, it is very questionable whether any hernial tumor would have been detected. When the patient was first admitted, in consequence of the glandular feel of these tumors, the statement of the woman that she had been ill nearly a fortnight, and the absence of vomiting, the swelling was pronounced not to be hernial, and the case peritoneal inflammation. As the obstinate constipation continued, and was succeeded by vomiting, and as the patient was gradually declining, it was eventually thought desirable to make an examination of the swelling; and although no benefit can be said to have resulted from the operation, the case tends still to confirm the general rule, "that in all cases of tumor, the hernial nature of which is doubtful, it is better, provided the symptoms of strangulation continue, to operate."

PHYSICAL SOCIETY, GUY'S HOSPITAL,

December 16, 1837.

MR. ASTON KEY IN THE CHAIR.

MR. MOLLOY read a thesis,

On the Influence of the Circulation in producing certain Cerebral Affections.

The object of the author, in this essay, was to show the great influence exercised by the vascular system, in producing certain cerebral affections. For a considerable time past, it had been very much the custom with pathologists, when tracing the origin of disease, to refer it ultimately to the nervous system. The universality of this explanation was objected to by the author; 1st. By reference to the physiology of that system, which demonstrated that was adapted to receive and transmit impressions; but, as far as our knowledge yet extended, was never capable of originating them. 2d. In many of those instances where the nervous system was admitted to be deranged, the cause of such derangement might, with a little consideration, be traced to the circulating system.

After discussing the physiology of the cerebral circulation, the author proceeded to comment upon those diseases in which the vascular system was undoubtedly disturbed: such were cerebral congestion and apoplexy.

The latter affection being of most frequent occurrence, occupied the greater part of his attention; and the action of the heart (hypertrophy of) and arteries in their production, was dwelt upon at considerable length. Other causes of cerebral lesions, less generally known, were referred to—viz. those arising from various conditions of the pulmonary system, and of the cavities on the right side of the heart; also tumors pressing upon the jugular and other veins, whereby the free return of blood from the cerebral mass to the heart was impeded.

Dr. GUY rose to mention a fact which he thought might bear upon the subject of the cerebral circulation. It was, that the body being placed in the inverted posture, the pulse became diminished in frequency. This result was in direct variance with that obtained by Dr. Graves, from similar experiments: this difference, Dr. GUY thought, might be explained by the supposition that Dr. Graves had allowed muscular action to be used by the subject of experiment, while assuming the inverted posture; whereas he himself had taken the precaution of performing his experi-

ments with the aid of a revolving board. Dr. GUY left the explanation with the Society.

MR. HILTON remarked, that there was a simple anatomical explanation for this phenomenon. When the body is inverted, a great accumulation of blood in the brain takes place, from the agency of two powers—gravitation, and the action of the heart: the former, because the column of blood in the inferior cava (unsupported by valves) and lower veins, pressing by gravitation into the right auricle of the heart, fills it, and prevents the free admission of that from the superior cava;—the latter, because the heart still continues to send blood into the brain, which, from the operation of the first cause, cannot be readily returned. Hence arise congestion in the brain, diminished respiration, and a sluggish pulse.

MR. GREENWOOD wished to know if it was well proved that the circulation of blood through the vertebral arteries was more necessary to life than that through the carotids.

MR. KEY said, that the experiments of Sir Astley Cooper and of Mr. Bransby Cooper upon dogs, published in the Guy's Hospital Reports, appeared to have completely decided the question. A case, however, occurred in Guy's some years ago, in which he had tied the carotid artery on the distal side of an aneurism: the patient was carried to bed, and died three hours afterwards, tranquilly, as if going to sleep. Upon inspection, the carotid artery of the opposite side was found obstructed to such a degree that a probe could scarcely be passed*.

The discussion now turned upon several points connected with the pathology and symptoms of certain cerebral lesions; in the course of which, many interesting facts, with observations, were brought forward by Dr. Whiting, Mr. Hilton, Mr. Roderick, Mr. Aspland, Mr. Chevers, Mr. Haygarth, Mr. J. Chapman, Mr. Key, and the author of the paper. These our limited space prevents us from giving at full length, whilst it would be doing injustice to their value to attempt a mere outline.

At the close of the discussion, thanks were voted to Mr. Molloy for his communication, and the Society adjourned to Saturday, 30th of December, at 8 P.M.; when Mr. Birkett will relate some cases of Psoas Abscess: Dr. Hughes in the chair.

* The preparation is in the Museum of the hospital, and the case was reported at the time, in the MEDICAL GAZETTE.

FURTHER REPORT
OF

CASES OCCURRING IN THE
CLINIQUE OF M. LOUIS,

At the Hôpital de la Pitié, Paris.

By HENRY CURLING, Esq.

CASE I.—*Pleurisy—Dilatation of the Bronchi—Fœtid Sputa.*

A FEMALE cook, aged 36, of a strong constitution, was admitted, under M. Louis, March 1st, 1837. She stated, that she had been very subject to colds for several years, and that, eighteen months ago, she had an attack of inflammation of the lungs; since then she has often "heard noises in her chest," but always on the right side. She has had the *grippe*, and was in La Charité, which hospital she left eight days since, but was not quite convalescent. Previous to her entry, her neighbours had remarked the fœtid odour of her sputa. The day after she left La Charité, she felt a pain in her right side, which was increased by coughing, and accompanied by fever and anorexia. These symptoms have continued, so that she walked with difficulty to the hospital.

Her countenance is pale, otherwise natural; heat of skin natural; pulse 68; coughs at intervals; sputa few in number, very fœtid, round, flattened, and of a greenish hue, floating in a salivary fluid; pain inconsiderable, situated laterally two inches above the right false ribs. Posteriorly, on the right side, percussion yields an obscure sound in the two inferior thirds extending laterally, as far as a perpendicular line dropped from the axilla. Bronchophony in many points of this space, and very well marked about two inches above the false ribs. Gargouillement and cavernous respiration heard in the same spots anteriorly. Percussion is obscure on the right side, to within two inches of the clavicle; the respiration is not bronchial, but harsh; doubtful ægophony and a little cræpement. On the left side, percussion and respiration are healthy, with the exception of a subcrepitant rhœphas posteriorly and inferiorly.

She has had three children; commenced menstruating at 15 years, and since she has been in Paris (four months) has menstruated every fortnight. The youngest child is sixteen years old.

Ptisan. Sodæ Chlorid. ʒj. in die sum.

March 2d.—The symptoms the same as yesterday. Percussion and auscultation afford the same results. She has never expectorated blood. Sputa not fœtid.

To omit the Chloride of Soda. Eau de Vichy, half a bottle daily.

On the 4th inst. she left her bed, but auscultation, daily repeated, did not vary in its results. On the 6th, the retentissement was almost amphorique. Her strength improved daily, and she soon gained her original embonpoint. At the end of the month she entered the service of the hospital as a nurse, but the gargouillement and pectoriloquy still existed. The sputa were not, however, fœtid, and percussion was not so dull on the right side.

REMARKS.—M. Louis's first impression, before he had fully interrogated the patient, was, that the disease was pneumonia; but reflecting on its slow march, on the great extent of the dull sound, on the little fever which had attended it, and on the gargouillement and cavernous respiration, he attributed these symptoms to gangrene of the lung. The diagnosis was apparently confirmed by the presence of fœtid sputa at the commencement; which symptom is considered pathognomonic of gangrene. But this symptom is not always constant; M. Louis has seen many cases in which the fœtid sputa have disappeared and returned, and yet, after death, gangrene of the lung has been discovered. Other physicians have remarked the same, so that this symptom seems to be of doubtful value. M. Louis has also observed fœtid sputa in pneumonia. But there is another disease very frequently accompanied by fœtid expectoration—viz. dilatation of the bronchi. When the bronchi are dilated, their parietes are thickened, the surrounding lung is compressed and rendered denser, and therefore percussion is generally duller on that side. But this would not account for the extensive dullness in this case; so that M. Louis ultimately attributed the symptoms to pleurisy, complicated with dilatation of the bronchi. The situation of the cavity at once showed that it had not a tubercular origin; besides the patient had no symptoms of this disease.

Dilatation of the bronchi is not always a chronic affection. Three years ago, M. Louis had under his care a female, suffering from a very severe headache, which, during a long period, had resisted all treatment. The pain ceased, however, after the application of a blister to the head. When on the point of leaving the hospital a cough was developed; and, on examining the chest, the existence of tubercles was discovered at the summit of both lungs. After the expiration of three weeks she died. Tubercles were found in every stage, and some of the bronchi were very much dilated. When she was first admitted into the hospital, her chest

was examined with great care, to detect some cause for the headache. It was again carefully examined when the cough commenced, but at neither of these periods was the dilatation detected; so that at that time it apparently did not exist.

CASE II.—*Chlorosis—Pericarditis.*

A woman, aged 30, was admitted into La Pitié, March 21st. She says that she has not been quite well for eighteen months. Her menstruation has become scanty, of a light colour, and followed by a little leucorrhœa; her countenance has grown pale, and she has been frequently troubled with headache, but her appetite has always continued good. About a fortnight ago her face began to swell, particularly her eyelids; her legs were also in the same state, and she was at the same time attacked with palpitations and dyspnoea, and a little slight cough.

21st.—She presented the following state:—Dark hair; a pale-yellow complexion; tongue and lips pale; face and eyelids swollen; no desquamation of the skin; the lower extremities pit slightly on pressure; she complains of headache; has never expectorated blood, nor had palpitations or swelling of the feet, before this time. A bruit-de-soufflet in the precordial region, but the respiration is audible there; continual bourdonnement in the carotids; percussion not so loud over the precordial region as elsewhere; a little eraquement inferiorly and posteriorly on both sides.

R Ferri Carbon. gr. iv. bis in die sumend.

23d.—The palpitations and bruit-de-soufflet continue; pulsations of the heart deep and obscure; a dull sound upon percussion between the sternum and left mamma, for about $1\frac{1}{2}$ inches; respiration inaudible in this space; there is also a slight projection of the chest, extending to within two inches of the clavicle. Pulse 92, regular, and without intermittance. Percussion is painful on the left side; sleeps with her head elevated, but only during the last month; heat of skin natural.

Ptisan to contain Potass. Acet. ʒss.
Diet. Lavement. Venæsectio ad ʒx.

24th.—Feels relieved; pulse 84 yesterday evening, 100 this morning, but accelerated by the visit. Tongue and cheeks more coloured; respiratory murmur audible in the precordial region, and a louder sound upon percussion. Pulsations of the heart more superficial, but not distinct.

Pil. Digitalis, gr. j. Venæsectio si opus sit.

On the 25th, the palpitations ceased; the obscurity of sound was less on the left side; the pulse was 104.

On the 28th the pulse was 86, and she had a little appetite.

She was ordered Carb. Ferri, and to continue the Digitalis.

April 1st.—There was no œdema, and scarcely any difference of sound between the right and left sides, and the respiratory murmur was every where audible. She soon left the hospital convalescent.

There were two distinct epochs in this case. At the commencement, the patient's disease was chlorosis, indicated by alteration in the menstruation, loss of colour, and headache. The palpitations, which were developed at a later period, might be attributed to that disease; but the dull sound upon percussion, the alteration in the form of the chest, the slight fever, and the absence of impulsion, seemed to indicate the existence of pericarditis, and to exclude the supposition of the palpitations being attributable either to hypertrophy of the heart or chlorosis. Still the effusion might depend on the latter disease, though, according to M. Louis's experience, effusions into the pericardium are comparatively very rare, even when they exist in the pleura. The symptoms of pericarditis are frequently very obscure. The heat, in this case, was but slightly raised, and the pulse was regular. M. Louis has found the pulse regular in about half the cases which have fallen under his observation; it, however, often varies, being regular one day and irregular the next.

Dry cough often accompanies pericarditis, and seems to be caused by the pressure of the pericardium upon the diaphragm. In the present case there was a little pulmonary catarrh.

CASE III.—*Gastritis.*

A female servant, aged 57, was admitted into the hospital, March 27th, having been ill eight days; but four or five days previous to this period, she had not felt quite well. On the 19th instant, about five in the morning, she was attacked with vomiting, and a pain in the epigastrium, followed by headache, and an acute pain in the loins, which have continued to the present time. The vomiting has been repeated, and she has not eaten any thing from the commencement of her illness, and has had no stool for seven days. She is naturally of a strong constitution; her senses are all healthy; her tongue natural in its circumference, but a little greyish and villous in the centre; considerable headache; pain in the epigastrium, extending to the umbilicus, and latterly to the false ribs; the pain much increased by pressure. The epigastrium is depressed, but affords sound upon percussing; complains much of the pain in her loins, but it does not inter-

ferre with her movements; respiration healthy; complete anorexia. Pulse 72 yesterday evening, with a little fever; 80 this morning.

Ptisan. Diet. Emollient Cataplasma to be applied to the epigastrium. Hirudines xx. ad lumbos applicand. Injection of infusion of senna.

March 28th.—No nausea, but vomited yesterday a little green fluid; less pain in the loins; tongue natural; no stool.

Rept. inject. cum Sodæ Sulph. 3j.

On the 30th instant the pain in the epigastrium had entirely left her, and she had a little appetite; in a week she was convalescent.

REMARKS.—The loss of appetite at the commencement, succeeded by complete anorexia, continued pain in the epigastrium, and repeated bilious vomiting, indicated a lesion of the gastric mucous membrane.

Gastritis is a very rare affection, that is to say, when it does not complicate other diseases; it is even more rare than pericarditis. M. Louis has seldom more than two cases during his clinique, which lasts three months. They always terminate successfully, and require only the administration of cooling drinks; opiates to calm the vomiting; and bleeding if there is considerable fever.

CASE IV. — *Hæmatemesis — Pneumonia — Death*—*Varicose state of the Veins of the Stomach.*

A man, aged 72, became a patient under M. Louis, in the month of February, and gave the following history of his complaint:—About fifteen years ago, after a violent exertion, he was attacked with severe hæmatemesis, which returned at intervals during a year; he also passed blood in his stools. His appetite and strength failed him; but being in good circumstances, and able to take great care of himself, he gradually regained his strength, and enjoyed perfect health till very lately. Having failed, he came to Paris, became a marchand-de-vins, and drank considerably. He was again attacked with vomiting, and purging of blood, and giddiness, for which he entered the hospital. He was then very weak; his face was pale, and had the hue of a person affected with cancer; the pulse was not quick; there was no pain; the respiration was natural. Rest and a strict regime were enjoined, and acidulated drinks prescribed. Under this treatment the stools became natural, and his appetite returned. He was gradually regaining his strength when he was attacked with shivering, sore throat, and dyspnoea, and a few days after with acute pain in the chest; his pulse became very much accelerated, and auscultation de-

tected the existence of pneumonia on the right side, and on the following day on the left.

He was bled, but died at the expiration of 36 hours.

Autopsy.—The brain was quite healthy; the ventricles contained a little serum. The inferior lobe of the right lung was infiltrated with pus, and the middle lobe was hepatized. The inferior lobe of the left lung was in a similar state. There were remains of an ancient pleurisy, and a recent exudation of lymph on the left pleura. The stomach was of its normal size, containing a yellow fluid; the mucous membrane was quite healthy, but veins, considerably enlarged, and full of coagulated blood, were seen under it. On the right of the cardiac orifice there was a small tumor, which, upon being cut into, proved to be coagulated blood. The submucous cellular tissue was slightly thickened. The intestines were natural, and their veins of the usual size. The spleen was hypertrophied, and its texture softened.

REMARKS.—The seat of the hæmorrhage, in this case, was evidently the stomach; and M. Louis attributed it to cancerous ulceration of the stomach; for continued hæmorrhage, except those from the nose, are almost caused by an organic lesion. It is impossible to say whether the hæmorrhage depended upon the varicose state of the veins. M. Louis has seen three cases of a similar state of the veins in the stomach; but though they were more numerous, they were less dilated. In none of these cases was there any hæmatemesis during life. The hæmorrhage, in the present case, was, in the first instance, preceded by violent exertion. The veins were also thickened, following the general law, that "when a tissue is preternaturally distended for any length of time, it becomes hypertrophied."

CASE V.—*Enteritis—Recovery.*

A female, aged 30, of a weak constitution, was admitted into La Pitié, under M. Louis, April 12th. She had been attacked on the 2d instant, about noon, with a rigor, which lasted two hours, followed by increased heat. She had likewise vomited, but had neither headache nor giddiness. These symptoms were soon succeeded by pain in the abdomen and diarrhœa, which, with the fever, have continued to the present time. The pain has been confined to no particular spot. On the second day of her illness she had considerable nausea and vomiting, attended with slight pain in the epigastrium, to which she had never been subject. On the third or fourth day, cough was added to these symptoms. For the six or seven days preceding her illness she had a little

tinnitus aurium, but it has since ceased, except when she coughs. She has one child; and menstruated about three days ago.

April 12th.—Pulse 94, full; yesterday evening 100; skin hot; eyes hollow; the inferior lip covered with a white crust; countenance expressive of weakness, but there is no appearance of stupor; memory exact; the circumference of the tongue red, white, and villous in the centre; nausea increased by drinking; complete anorexia; four stools yesterday; no pain in the abdomen. A little below, and to the left of the umbilicus, there is a swelling about two inches in width. It is hard, fixed, and gives no sound, and is not at all painful; respiration quite natural.

Diet. Ptisan, Olij. Extract. Opii, gr. i.

15th.—Feels better, and is in good spirits; has eaten some bouillon without being followed by nausea; pulse 100, regular; heat natural; no pain in the epigastrium; the tumor is more moveable, and is very close to the neck of the uterus, which is not, however, displaced; tongue white and villous; has had one stool.

All these symptoms gradually subsided, with the exception of the diarrhoea, which persisted to the 22d inst. A few days after, she left the hospital convalescent.

CASE VI.—*Enteritis—Recovery.*

A man, aged 53, of a moderately strong constitution, was admitted under M. Louis March 22d, having been ill seven days. His illness commenced with pain in the abdomen and diarrhoea, which have continued to the present time. He has had as many as thirty stools in one day, but no tenesmus. The matter voided was often compared by the patient to white of eggs, and was frequently mixed with blood. He has had considerable fever, attended with perspiration and headache. He ceased working from the commencement, but has not kept his bed. He has never had a similar complaint.

March 23d.—Countenance natural; tongue moist; thirst moderate; has vomited twice; six stools yesterday; abdomen well formed, sensible to pressure; the pain does not follow a transverse direction. Pulse 52; no fever; subcrepitation posteriorly and inferiorly on both sides, but especially on the right.

Tr. Opii, 5j. in injectione administ.

The diarrhoea ceased the second day after the administration of the injection, and in three or four days he was quite well.

[To be continued.]

CAUSE OF THE VARIATIONS OF THE PULSE IN DIFFERENT POSITIONS.

To the Editor of the Medical Gazette.

SIR,

HAVING experienced the difficulty of coinciding with certain views offered to the Physical Society of Guy's Hospital, which are recorded in the MEDICAL GAZETTE for the 28th of October, and finding the subject again recurred to in the valuable pages of the same periodical, at a subsequent period, I am induced to solicit your attention again to the subject, in the hope of eliciting further information.

I was among the number of those present, when, as the MEDICAL GAZETTE announces, Dr. Guy "vindicated to himself his claim to originality, and stated that others had only surmised, while he had proved, that muscular contraction was the cause of the difference of pulses in different positions." Now I scarcely think he will find those who are willing to deprive him of the honour of this discovery.

The pulse is under the influence of a variety of agents, both as to retardation and acceleration; in running, wrestling, leaping, and such like vigorous exercises, accelerated pulsation clearly ensues; but in mere change of posture, the appreciation of such muscular influence appears to me wholly absurd.

In a treatise on the Effect of Posture on the Pulse—one, too, not hastily produced, but preceded by numerous "experiments," and followed by tabular statistic deductions—we may reasonably anticipate an enumeration, at least, of all the more obvious appearances ensuing upon change of posture. The attention of the observer, however, appears to have been so restricted upon one point, to the prejudice of the rest, as to induce error and obscure the true origin of the phenomena. A person suddenly jumping out of bed, experiences a sense of giddiness, with fluttering palpitation, succeeded by a bounding, energetic action of the heart, &c. which oblige him to assume a sitting posture for a few seconds; after which the untoward sensations retire, and he rises with comfort; the heart, however, continuing to beat considerably quicker, and with greater force than when the circulating system occupied a horizontal position. Here we have change of posture succeeded by a disturbed action of the pulsating organs; and, according to Dr. Guy, the muscles which the individual made use of in getting up, by compressing the blood-vessels, caused all this commotion. Then, although certain effects of posture on pulsation are so obvious and severe as often to oblige the individual to be seated, and not only so, but in some

cases even to prove instantly fatal, Dr. Guy, in his observations, passes by all this, and contents himself with fingering the radial artery after the principal action is over; for he does not begin his counting till the main results of change of posture have subsided! It may be observed, also, that after rising, the heart acts, not only with greater frequency, but also with greater force; it is felt throbbing vigorously, with more and more energy up to a certain point, then gradually subsiding to comparative calmness; but still continuing to act with a permanent equable energy, beyond the force required in a horizontal position. This additional energy, however, cannot be estimated by fifths and sixths; and, therefore, in certain "valuable" statistics, would have spoilt the value of their integrity; consequently it was more than convenient to pass it by without appreciation.

Seeing, then, that I regard Dr. Guy's notion concerning the agency of the voluntary muscles as utterly erroneous (not to say ridiculous, for courtesy's sake), I shall be expected to supply an efficient explanation; and this I find close at hand in the physical effects of fluid gravitation. In the change from recumbency to an erect position, we necessarily have from gravity a diminished pressure upon the brain. "When the heart has to send blood upwards, it requires to act more strongly than when the body is horizontal, and the pulse increases five or six beats in a minute." Increased frequency, in conjunction with additional force, is surely, then, an effort to restore and to maintain the equilibrium. This view of the subject is so perfectly obvious on all sides, that I need not endeavour to trespass on your valuable pages any further concerning it.

With regard to Mr. Gorham's paper, whatever he may think with regard to the "relevancy" of its object with that of Dr. Guy's, they are essentially distinct; the one considering pulsation in different postures, the other pulsation in different ages. Mr. Gorham himself even insists upon it, that in many of his observations, relative posture neither was nor could be considered; which alone was Dr. Guy's main object: yet Mr. Gorham's distinctive powers perceive no irrelevancy between these two objects. Allow me to assure Mr. Gorham, that although an infant cannot be made to stand for the purpose of experiment, yet, if it be held erect, increased pulsation will ensue; but infants are so constituted as to bear an unusual position with greater impunity than the adult, and consequently the effect is comparatively less.

A GUY'S STUDENT.

December 8, 1837.

METHOD OF PUFFING QUACK MEDICINES.

To the Editor of the Medical Gazette.

SIR,

I THINK you would be doing a public service were you to expose the system pursued by proprietors of quack medicines, in giving out to the world by their hand-bills, opinions purporting to emanate from the leading journals of the metropolis in reference to their nostrums, but which are, in point of fact, gross deceptions on the public.

It was only the other day that, travelling in the country, I met with a person who, in testimony of the high reputation which quack medicine was held, pulled out of his pocket a hand-bill whereon the opinions of at least twenty of the leading London journals were given, of course highly laudatory of the nostrum in question.

Their system is as follows:—An advertisement is sent to—say twenty different newspapers, all containing the PUFF intended to be re-conveyed on the hand-bills of the advertiser, and accordingly in a few days afterwards such puff is translated into the hand-bills, and the journals in question put as the authors thereof.

I need not say one word more upon a subject so likely to mislead unwary persons, and hope that your insertion of this will be the means of correcting the evil.

I am, sir, yours, &c.

A CONSTANT READER.

London, Dec. 6, 1837.

GANGRENE OF THE LUNGS.

To the Editor of the Medical Gazette.

SIR,

IN Mr. Laycock's cases of Pulmonary Affection, published in your journal last week, I find allusion made to a case of recovery from gangrene of the lungs, which occurred under my care in October 1832, and which was inserted in the MEDICAL GAZETTE shortly after (vol. xii. p. 134.) As it might be doubted whether the convalescence became complete, I trouble you with these few lines to state, that the man is still alive, and in excellent health, which fact I ascertained by having happened accidentally to meet him in Regent-street about a month back.

I am, sir,

Your obedient servant,
J. W. CRANE, M.D.

Dec. 14, 1837.

OF

DRUGS ON SALE IN THE ENGLISH MARKET,

With their Prices and several Duties.

(From the Official Returns up to Tuesday, December 22, 1837.)

	PRICE.		DUTY.	DUTY PAID	
	£	s. d.		In 1837. 1st Week	Same time last year.
Aloes, Barbadoes, D.P. c	12	0 0 to 30 0 0	} B.P. lb 0 2 F. lb 0 8	110,911	108,539
Hepatic (dry) BD. c	5	0 0			
Cape, BD. c	2	0 0	} E. lb 1 4 E. I. 1 4	251	2,007
Aniseed, Oil of, German, D.P. lb	0	9 6			
E. I. lb	0	7 0	} lb 0 1 c 4 0	2,574	2,699
Assafoetida, B.D. c	0	2 10			
Balsam, Canada, D.P. lb	0	1 3	} lb 0 1 c 4 0	2,491	6,779
Copaiba, BD. lb	0	4 0			
Peru, BD. lb	0	4 3	} lb 1 0 c 4 0	1,531	1,880
Benzoin (best) BD. c	25	0 0			
Camphor, unrefined, BD. c	9	0 0	} lb 1 0 c 1 0	496	487
Cantharides, D.P. lb	0	5 2			
Caraway, Oil of, D.P. lb	0	9 0	} lb 4 0 lb 0 1	1,693	1,533
Cascarilla or Eleutheria Bark, D.P. lb	1	15 0			
Cassia, Oil of, BD. lb	0	7 0	} lb 1 4 c 1 3	3,483	3,549
Castor Oil, East India, BD. lb	0	0 6			
West I. (bottle) D.P. 1 1/2 lb	0	2 3	} lb 0 6	6,388	6,827
Castoreum, American, BD. lb	1	15 0			
D.P. Hudson's Bay lb	1	0 0	} c 1 0 lb 0 1	467	1,331
Russian lb	1	0 0			
Catechu, BD. c	1	0 0	} lb 0 2	32,215	10,316
Cinchona Bark, Pale (Crown) lb	0	2 0			
BD. Red lb	0	3 0	} lb 0 2	141,071	116,184
Yellow lb	0	2 0			
Colocynth, Turkey lb	0	2 6	} lb 0 2	13,511	14,349
D.P. Mozadore lb	0	3 0			
Calumba Root, BD. c	1	4 0	} lb 0 6	15,840	19,622
Cubels, BD. c	3	0 0			
Gamboge, BD. c	5	0 0	} c 4 0 c 4 0	35,995	28,052
Gentian, D.P. c	1	4 0			
Guaicum, D.P. lb	0	1 0	} c 6 0	461	329
Gun Arabic, Turkey, fine, D.P. c	8	0 0			
Do. seconds, D.P. c	5	0 0	} c 6 0	53	9
Barbary, brown, BD. c	3	0 0			
Do. white, D.P. c	4	15 0	} c 6 0	5,146	10,216
E. I. fine yellow, BD. c	3	0 0			
Do. dark brown, B.D. c	1	15 0	} c 6 0	2,705	3,080
Senegal garblings, D.P. c	4	15 0			
Tragacanth, D.P. c	8	0 0	} c 6 0	4,198	3,311
Iceland Moss (Lichen), D.P. lb	0	0 2 1/2			
Ipecacuanha Root, B.D. lb	0	1 9	} lb 1 0	417	278
Jalap, BD. lb	0	1 10			
Manna, flaky, BD. lb	0	4 0	} lb 0 6	12,845	20,599
Sicilian, BD. lb	0	1 7			
Musk, China, BD. oz	1	0 0	} lb 1 0	11,352	11,437
Myrrh, East India, BD. c	5	0 0			
Turkey, BD. c	2	0 0	} lb 0 6	56,355	45,182
Nux Vomica, BD. lb	0	8 0			
Opium, Turkey, BD. lb	0	12 6	} lb 0 3	20,504	20,781
Peppermint, Oil of, F. BD. lb	1	1 0			
Quicksilver, BD. lb	0	3 6	} lb 2 6	2,234	1,431
Rhubarb, East India, BD. lb	0	2 6			
Dutch, trimmed, D.P. lb	0	3 6	} lb 1 0	151	212
Russian, BD. lb	0	8 3			
Saffron, French, BD. lb	0	18 0	} lb 2 6	1,273	1,884
Spanish lb	0	19 0			
Sarsaparilla, Honduras, BD. lb	0	1 0	} lb 1 0	36,453	38,143
Lisbon, BD. lb	0	2 0			
Scammony, Smyrna, D.P. lb	0	12 0	} lb 4 0	1,099	413
Aleppo lb	0	0 3			
Senna, East India, BD. lb	0	0 3	} lb 0 1	310,631	292,118
Alexandria, D.P. lb	0	1 6			
Smyrna, D.P. lb	0	1 0	} lb 1 0	44,420	36,701
Tripoli, D.P. lb	0	1 0			
	0	1 0	} F. lb 1 0	5,946	7,752
	0	1 0			
	0	18 0	} lb 1 0	5,092	5,154
	0	19 0			
	0	1 0	} lb 0 6	109,626	125,140
	0	2 0			
	0	12 0	} lb 2 6	8,130	6,725
	0	0 3			
	0	1 6	} E.I. lb 0 6	96,937	90,167
	0	1 0			
	0	1 0	} Other sorts	56,392	58,973
	0	1 0			

‡‡‡ BD. In Bond. — c. Cwt. — B. P. British Possessions. — F. Foreign. — D. P. Duty paid.

EPIDEMIC SMALL-POX AT PARIS.

AN epidemic small-pox has prevailed for about two months, particularly in the hospitals; most of the physicians of the Hôtel-Dieu have had a considerable number of patients. The vaccinated have not been spared more than the unvaccinated, only in the former the small-pox has generally been distinct, and in the few cases where it was confluent the pustules were fine, with a good central depression, and the patient recovered.

The number of persons affected of either sex was nearly equal; the mean age of the patients was between 20 and 30.

The majority of the unvaccinated patients had the confluent small-pox, which broke out irregularly, the pustules being of different sizes, and appearing at the same time; they were often flattened, and rarely had a central depression: some pustules did not appear till the others were completely suppurating; and then continued very small, and suppurated almost immediately.

When the pustules were well developed, the disease terminated favourably, though of the confluent type.

Serious symptoms, or death, occurred in those cases where there had been no swelling of the eyelids, nor hands, and where the fever continued after the eruption; the pustules then remained flat, and of a silvery white.

We are unable to give an accurate account of the patients admitted, and even of the number of deaths, as the returns have not yet been made, the epidemic not having entirely ceased. A case of the confluent form, under the care of M. Honoré terminated by the complete and sudden disappearance of the eruption. M. Magendie pointed out to us that all the patients under his care, who had been vaccinated, had been so more than ten years ago.

There was nothing unusual in the treatment, excepting that M. Legroux (who had the care of M. Petit's patients) found the chlorides useful when administered internally during the period of suppuration; he gave the patient food at the same time, and these means conjoined seemed to him useful in preventing reabsorption. He employed tonics at the end of the treatment.

M. Legroux is now trying vaccination on all parts of the body, and even the face, at the period when the pustules begin to appear.—a method which, it seems, has succeeded in other hands. We have no result as yet to announce.

This small-pox has been attended by a varioloid epidemic, which has not offered any thing remarkable.—*Gaz. des Hôp.*

UNIVERSITY OF LONDON.

DIFFERENCES IN THE SENATE.

THE resolution of the Senate of the University of London to institute an examination on certain points connected with religion before granting any degree (as stated by us the week before last), has excited such dissatisfaction on the part of the minority, that they have laid a case before the Attorney-General, to know whether such decision be not incompatible with the Charter. We hear, also, that the Home Office has been beset by gentlemen of the Jewish persuasion during the last fortnight, and that a great effort is making by certain individuals, to accomplish by pressure from without, what it was unsuccessfully attempted to effect by any influence within.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, Dec. 26, 1837.

Age and Debility	27	Hooping Cough	12
Apoplexy	3	Inflammation	16
Asthma	9	Bowels & Stomach	3
Cancer	1	Brain	3
Childbirth	1	Lungs and Pleura	8
Consumption	45	Influenza	1
Convulsions	17	Insanity	1
Dentition or Teething	3	Liver, diseased	1
Dropsy	3	Measles	6
Dropsy in the Brain	4	Mortification	1
Dropsy in the Chest	1	Paralysis	1
Erysipelas	1	Rheumatism	1
Fever	8	Small-pox	6
Fever, Scarlet	5	Spasms	1
Fever, Typhus	2	Tumor	1
Hæmorrhage	2	Unknown Causes	25
Heart, diseased	1		
Hernia	1	Casualties	2

Increase of Burials, as compared with } 42
the preceding week

METEOROLOGICAL JOURNAL.

Not come to hand.

ERRATA.

In Mr. Sweeting's paper on the Treatment of Fractures, in our number for December 16th, page 152, col. 2, line 10, for "compatible," read "compatibile;" page 453, col. 1, line 9, add "mended;" and for "tract," read "track;" page 454, col. 1, line 49, for "first board," read "foot-board."

WILSON & SON, Printers, 57, Skinner-st., London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JANUARY 6, 1838.

LECTURES

ON

FORENSIC MEDICINE;

Delivered at the Aldersgate School of Medicine.

By SOUTHWOOD SMITH, M.D.

Physician to the London Fever Hospital.

LECTURE V.

Application of the general doctrine of Evidence to the special subject of Forensic Medicine—Practical Rules thence deducible for the Collection of Facts and Induction of Inferences—No article of Circumstantial Evidence to be neglected because it is weak—No article to be neglected because it is not in itself conclusive—The Aggregate Mass not insufficient because of the insufficiency of the Elementary Parts—The aggregate mass not insufficient because an article of one particular description be wanting—Necessity of care that no intermediate link be wanting—Circumstantial Evidence not insufficient on account of the absence of Direct Evidence—Direct Evidence not insufficient on account of the absence of Circumstantial Evidence—No mass of Circumstantial Evidence conclusive in all cases; still less against Counter-Evidence—Strength of a Confirmative Fact to be tested by its opposing Infirmative Supposition—Circumstantial Evidence not useless on account of the abundance of Direct Evidence—Preservation and Identification of real Evidence—Why the Conduct and Language of all Persons about the Patient should be attentively observed—Record immediately to be made of all that is observed—Why the Investigation should be minute—Mode of Deposition—Conjoint Investigation—Language appropriate to the Medical Witness—Duty of the Medical Witness to state fully every thing known to him—Conduct towards an opposing

Medical Witness—Exception in the case of Secrets communicated in Professional Confidence.

1. No article of circumstantial evidence to be neglected because it is weak.—Whenever you are called to take a part in a judicial investigation, neglect to observe and record no article of circumstantial evidence, because it may appear at the moment to be weak. You do not know how important a link in the chain of evidence a very trifling circumstance may hereafter become, nor how closely connected with an essential fact an apparently irrelative circumstance may turn out to be.

2. No article to be neglected because it is not in itself conclusive.—Much less neglect to observe and record any article of circumstantial evidence, on the ground that it is not in itself conclusive. You know that, from the nature of circumstantial evidence, no single fact can be conclusive; that the force of persuasion produced by circumstantial evidence arises from the accumulation of a number of facts, each independent, but all related; each, taken by itself, inconclusive, but the whole forming a body of evidence which produces irresistible conviction.

3. The aggregate mass not insufficient because of the insufficiency of the elementary parts.—Hold not the aggregate mass insufficient for the separate insufficiency of the elementary articles. There is not a single fact in the chain of evidence attaching guilt to Donnellan which, taken by itself, is sufficient to prove scarcely any thing; and yet, viewing all the facts in combination and in relation, the aggregate mass produces an irresistible persuasion of his guilt.

4. The aggregate mass not insufficient because an article of a particular description be wanting.—Hold not the aggregate mass insufficient, from the mere want of an article of this or that one description. If

the evidentiary facts are sufficiently numerous, and if they stand related to each other with sufficient closeness, their probative force is not to be disregarded because a fact of this or that particular description is wanting.

5. *Necessity of care that no intermediate link be wanting.*—Nevertheless, in every chain of evidentiary facts, let it be your special care that no intermediate link be wanting. The detection of the poison of laurel-water by its smell, in the so-called purging draught taken by Sir Theodosius Boughton, was an intermediate link, connecting the fabrication of the poison by secret distillation on the part of Donnellan, with the anxiety manifested by him to destroy the odour of the poison in the phial from which the pretended physic had been taken. In like manner the effects produced by the contents of this phial, the precise effects known to be produced by laurel water, formed a connecting link between the fabrication of the poison, and the advice to the young baronet to keep his physic bottles in the outer apartment. Take away either of these links—still more, take away both of them—how greatly is the probative force of the evidence diminished.

6. *Circumstantial evidence not insufficient on account of the absence of direct evidence.*—Hold not circumstantial evidence insufficient, as such, from the want of direct evidence. It is seldom, as you have seen, that direct evidence can be obtained; and the perpetration of a crime could hardly ever be brought home to an evil-doer, if credence were refused to evidence wholly circumstantial.

7. *Direct evidence not insufficient on account of the absence of circumstantial evidence.*—Hold not direct evidence insufficient merely for the want of circumstantial evidence. With regard to whatever fact you are really a percipient witness, your testimony as to that fact will of course be decisive; but you can never, after what has been stated, fall into the error of confounding a fact witnessed with an inference deduced from the fact.

8. *No mass of circumstantial evidence conclusive in all cases.*—Set down no article, nor any aggregate mass of circumstantial evidence, as conclusive—not even so much as provisionally conclusive—in all cases.

9. *Still less against counter evidence.*—Much less set down any article or mass as conclusive against, or to the exclusion of all counter evidence.

10. *Strength of a confirmative fact to be tested by its opposing infirmative supposition.*—Whenever you meet with any strong confirmative fact, neglect not to search for every infirmative fact which can by possibility exist. The use of infirmative sup-

positions is to afford a test of conclusiveness, and in some sort even of probative force. A given principal fact appears to you to be supported by a given evidentiary fact. In such a case never fail to look around you to see if there can be found no fact, the tendency of which is to lessen its probative force. If any such fact be supposable, and if, on investigation, its existence be established, the existence of the principal fact may be positively disproved by it; because the existence of the former may be incompatible with that of the latter; or if the existence of the principal fact be not positively disproved by it, it may be rendered less probable. Whenever any such infirmative supposition is established, the probative force of the evidentiary fact can never be so great as to be conclusive. But if after your utmost endeavour you find yourself unable to discover any such infirmative supposition, then in that particular instance, and in as far as regards your own persuasion, the probative force may be conclusive.

A man was found shot by a ball; the wadding of the pistol stuck in the wound; on examination it was found to be part of a ballad, called Sweet Poll of Plymouth; this corresponded precisely with another part found in the pocket of the prisoner.

A man was found lifeless; in the head of the deceased there stuck a chip or splinter which exactly fitted the cavity in a bludgeon from which a piece had been lately broken; this bludgeon the prisoner carried in his hand when he was apprehended.

These are evidentiary facts strongly tending to produce the persuasion of the principal fact, that the prisoner in each of these cases was the perpetrator of the murder. In the absence of all other evidentiary facts—standing alone—they prove nothing; because the probative force of each is reduced to nothing by the infirmative supposition that the ballad and the bludgeon had been thrown away by the real murderers; that these objects had attracted the attention of innocent men passing by that way, and that the one had naturally put the ballad in his pocket, and the other had carried the bludgeon in his hand.

11. *Circumstantial evidence not useless on account of the abundance of direct evidence.*—Reject not circumstantial evidence as useless, on account of the abundance of direct evidence. Direct evidence may be deceptive; direct testimony may be mendacious; but the peculiar advantage of circumstantial evidence is, that the greater the number of facts of which the mass consists, the greater is the chance that every false allegation will be disproved by some notoriously true fact with which the

falsehood is incompatible. And the greater the number of false facts, the greater is the probability that some one or more of them will clash with some positively known fact.

In like manner, the greater the number of deposing witnesses, the more seldom it can happen that any such successful concert can take place between them, as is necessary to give effect to a plan of mendacious testimony.

12. *Preservation and identification of real evidence.*—Take care to preserve every object which is capable of affording real evidence, in such a state that it may exhibit precisely the same evidence hereafter. Take instant and effectual precaution to prevent it from passing of itself, or from being purposely or accidentally brought by others, into a condition in which it can no longer afford evidence; or in which the evidence it indicates may prove fallacious. In a case recorded by Dr. Alison, a Scotch farmer is represented as having acted in a manner which affords a model for the conduct of a medical practitioner.

In 1812, a man of the name of Muir was tried and condemned at Glasgow for poisoning his wife. In the course of the day on which she became ill she was visited by a farmer of the neighbourhood, who had studied physic a little in his youth. The farmer learnt from his friend that she had breakfasted on porridge a short time before she felt herself ill; and that she suspected the porridge to have been poisoned. The farmer instantly procured the wooden bowl in which the cottagers of Scotland keep the portion of meal, used each time for making the porridge. Observing in it some meal, with shining particles interspersed, he wrapped a sample in paper, and took the proper means for preserving its identity. At the same time he secured a sample taken from the family store in a barrel. The two parcels were produced by him on the trial; and from experiments made in court, the late Dr. Cleghorn was enabled to declare, that the meal from the bowl contained arsenic, and that the meal from the barrel did not. These facts, besides proving that the woman had next to a certainty taken arsenic in the porridge, conducted, in conjunction with other circumstances, to establish the further facts, that the poison had been mixed with the meal in the house; that it was mixed with the meal on the morning when the deceased was attacked with illness; and that all this happened before any stranger entered the house.

In like manner, whenever you are called to a patient attacked with sudden disease, the symptoms of which are suspicious, the moment you have given directions for the treatment your thoughts should turn towards the collecting and

securing of evidence. Ascertain in what articles it is possible for poison to have been administered; endeavour to secure at once the remains of that portion in particular of which your patient has partaken; or if this cannot be done, then look to the ingredients of which the dish is stated to have been made; do not neglect the salt, nor any article with which the dish may have been seasoned; and take the necessary precautions to verify the identity of every thing of which you get possession.

13. *Why the conduct and language of all persons about the patient should be attentively observed.*—Keep a watchful eye upon the conduct of every person with whom you come in contact. Mark well the answers they return to any questions that may be put to them by yourself or others. Be not inattentive to the language which may spontaneously fall from them. There is not a word spoken by the patient, or the persons about him, not a thing done or suggested, of which you should not be observant. Conscious guilt is apt to betray itself. It is seldom that it can maintain the indifference requisite to its own security. By saying or doing too much or too little, it is apt to show that it is in possession of too much or too little knowledge, and this suspicion is often excited by the very efforts directly or indirectly made to produce the impression, that it knows absolutely nothing. All the words and actions of all persons in the first moments of alarm, the community has a deep interest in having noted. In this, as in so many other instances, your position and your office combine to make you the guardians of the interests of the community.

The observation of all that passes at the moments of which I am speaking, may be of the utmost importance to the vindication of the innocent, no less than to the punishment of the guilty. Long before suspicion has told her tale to justice, long before the servants of justice have been put upon the search for evidence, long before any steps have been taken to fix guilt upon any individual, you may be in possession of decisive evidence. Lady Boughton had obtained damning proofs of the guilt of her companion, before the conception of his guilt had so much as entered into her mind. A surgeon one day examined a woman who swore that she had been violated; he obtained from her an acknowledgment that she did not scream at the time; the woman died in the interval between her deposition and the trial; she had sworn positively that the accused had consummated his crime. In this declaration she persisted to the hour of her death. On the trial her evidence

could not be sifted; could not be exposed to the ordeal of cross examination; nevertheless, in this case, by the rule of English law, it must be taken absolutely as given, because it is conceived that the solemnity of the occasion, death being supposed to be immediately in view, the ordinary security may be safely dispensed with. What, then, in a case like this, could save the life of a person falsely accused? Precisely the fact of which this medical witness, by his promptitude and sagacity, had obtained possession.

14. *Record immediately to be made of all that is observed.*—Of all that you observe, make an accurate record at the time. The minute circumstances that surround the main facts are apt to be forgotten, and the facts themselves to receive a colouring from subsequent events. Dates especially, the intervals of time between events and the proportionate quantities of substances, are exceedingly apt to be forgotten; but some fact of this kind may possibly become of the utmost importance in the subsequent inquiry. By noting down every thing of this sort on the spot, or as soon as your examination is finished, all possibility of error is avoided.

15. *Why the investigation should be minute.*—Do not satisfy yourself with a general and superficial inspection; let your examination of every circumstance in every case that comes before you, be most minute. The correctness and completeness of the evidence you obtain, and consequently the soundness of the decision to which you come, will commonly be in proportion to the minuteness of your investigation; and minute investigation may possibly open to you new sources of knowledge as valuable as novel,—a splendid example of which I lately brought before your view in the discovery of the different appearances exhibited by burns in the living and the dead body, the characteristic marks of which were observed and described by the late Dr. Duncan, by which it can now be determined with absolute certainty whether the burning took place during life or after death.

16. *Made of deposition.*—I have laboured to the utmost of my power to impress upon your minds an adequate sense of your responsibility, arising out of the circumstance that, on all subjects connected with your profession, the facts you state, and the inferences you deduce, are alike received as evidence, and to show you how sacred the duty consequently becomes, that you should take all possible pains first to give to your facts the qualities of correctness and completeness, and secondly, to draw from them only strictly logical conclusions.

But having done this—having taken all

needful precaution to verify the correctness of your observation, and to ensure the soundness of your induction—you are bound to state the result with decision, and to abide by it with firmness. You have made yourself master of the subject in hand. You go into court to state what you know; results which you have ascertained. What is there in this which a man of sense and integrity should shrink from or fear? Temerity is out of the question; there will be nothing of it, there can be no indication of it, in any one who is in possession of competent knowledge, and who is conscious of the purpose to which it is to be applied; but timidity is equally misplaced, and you must not allow the cause of justice to sustain injury, nor your own reputation to suffer, from a want of firmness equally unreasonable and undignified. Suppose you had examined with due care the wound inflicted on the head with the snuffers, to which I directed your attention in the first lecture, could you have spoken with any degree of doubt or hesitation as to whether this wound were or were not inflicted by a body capable of penetrating the brain to a certain depth? Such a wound must have presented such and such characters; and such characters presenting themselves, the inference to be deduced is inevitable. Not to deduce it implies either an ignorance of the nature of wounds, or an ignorance of the nature of evidence.

17. *Conjoint investigation.*—But there may be some points on which you may be called to give an opinion, on which, with the degree of knowledge you possess, you may hesitate to give a positive opinion. The facts may appear to you, upon the whole, to warrant such and such an inference; but still you may not be quite sure of it. This may very reasonably be the case with some appearances in the stomach, intestines, and other organs, that may seem to exhibit the characters of disease—disease of a specific nature—disease produced by a specific cause. Are these appearances really the result of inflammation, and are they produced by an irritant poison? Now your eye may not be sufficiently experienced in the structural changes produced by disease to give you entire confidence in your own judgment. In like manner, if you are not a well-practised chemist, you may doubt as to the perfect accuracy of some result which you obtain from your chemical investigation.

In all cases of this kind, what ought you to do? Join some one else with you in the inquiry. Discuss the matter with other medical men. Obtain the advantage of their eyes if you can, but at all events get the benefit of their judgment. Before you form your opinion, or give

your testimony, counsel with the best-informed and the soundest-minded among your brethren to whom you can gain access, both as to the correctness and completeness of the facts, and as to the logical soundness of the inferences.

"It is of the greatest importance, says Beck, that two or more professional persons should be united together. They will assist each other not merely mechanically, but by suggesting various points of inquiry. While he who is most skilled in anatomy is pursuing his dissection, the other may note the appearances as they successively present themselves. And the same course may be adopted while performing chemical experiments. The advantage will be thus obtained of having a complete statement of facts, prepared at the moment of observation, which may be afterwards reviewed both in coming to a decision on the case, and in giving evidence before a jury."

18. *Language appropriate to the medical witness.*—In the answers you return to the questions put to you by counsel, let your language be clear, concise, terse, and free from technicality. Never make use of a technical term, unless you introduce it for the express purpose of defining it, and in order to give to your statement greater clearness as well as greater correctness. Do not, for example, speak of *vascularity* when you might say redness, of *syncope* when you might say fainting, or of *cephalalgia*, when there is such a plain word as headache. There is scarcely a single fact or event on which you can be called to speak in a court of justice, however peculiarly it may relate to your own profession, which you may not communicate in the language of common parlance. Follow in this respect the excellent advice which Sir William Blizard was accustomed to give his pupils. "Be," said he, "the plainest men in the world in a court of justice. Never harbour a thought that if you do not appear positive you must appear little and mean; many old practitioners have erred in this respect. Give your evidence in as concise, plain, and yet clear a manner as possible; be intelligent, candid, exact, and just, yet do not aim at appearing unnecessarily scientific. State all the sources from which you have gained your information. If you can make your evidence a self-evident truth; thus, though the court may at the time have too good or too mean an opinion of your judgment, yet they must deem you an honest man. Never, then, be dogmatic, or set yourselves up for jury and judge. Take no side whatever; be strictly impartial; you will frequently hear the counsellors complain when a surgeon gives his opinion with any the

least degree of doubt, that he does not speak clearly; but if he is loud and positive, if he is technical and dogmatic, then he is allowed to be clear and right."

Let not the bad example so justly reproached by this experienced teacher mislead you. The counsellor is an advocate; it is his object to obtain a decision on the side on which he is engaged, whether that be the side of truth or falsehood, of innocence or guilt. But it is your object to establish the truth, and nothing but the truth; and you deviate grievously from your duty when you speak confidently and absolutely in a case in which there is ground for reservation and doubt, and hesitatingly in a case in which the facts are positive and demonstrable.

19. *Conduct towards an opposing medical witness.*—If ever you should happen to be placed in collision with a fellow medical man, forget not the respect which is due from one person of education and science to another. Let your manner be courteous, and the opinion you express be given with diffidence. If you really differ in opinion, state plainly and simply in what the difference consists, and the ground on which you have arrived at your own conclusion: if the difference be merely a matter of opinion, show that it is a point on which persons of knowledge and judgment may be allowed to differ without reproach. Even if, according to the best judgment you can form, you think an error has been committed, it will reflect disgrace upon yourself, and justly beget doubt whether your intellectual and moral qualifications fit you for forming a sound and trustworthy judgment, if on such an occasion you deliver your opinion with an air of triumph, with self-confidence and self-satisfaction. There is no reason why you should conceal your opinion; there may be grave reason why you should avow it; but your feeling should be sorrow that you are under the necessity of doing so, and your manner that which real sorrow will produce.

On a trial some time ago, Mr. Tickner, a surgeon, was asked—"Supposing a person to have retchings and purgings for several hours, and that you found these attended with frequent and fluttering pulse; in that state of the illness what should you have prescribed?" His answer was—"I should have prescribed diametrically opposite to the prescription of Dr. Edwards. I should consider that prescribed by Dr. Edwards as adding weight to a porter's back."

The course and vulgar mind indicated in this reply, received merited reproof from the court. Lord Tenterden said to the witness, "Don't speak metaphorically: you were speaking just now of a gentleman

of experience and respectability. I don't wish you to conceal your opinion, but only to speak it in different language."

Contrast the feeling in which this language and deportment originated, with that which dictated the language and deportment of a man of genius and science. The story is related by Mr. Abernethy, in reference to a case in which he himself took a part.

"A medical man," he says, "was prosecuted for killing a child by giving it opium; at least that was said. I happened to be in the country at the time, and was strongly solicited by him to attend and give my opinion. I considered that the character of the profession was at stake, and although rather against my inclination, I went. After waiting in a crowded court the greatest part of the day, I was called upon and placed in the witness-box. The lawyers had taken it into their heads that the child had died from mal-treatment on the part of the surgeon; the child had been scalded severely, and he had given opium, and they thought they should be able to make the jury think as they did. The first question put to me was, 'Mr. Abernethy, will you inform us what is considered the proper treatment for scalds?' This was a question broad enough, to be sure; I was puzzled a little how to answer it: I did not know but they would require a lecture on burns and scalds. I considered a minute, and then said, 'That which was adopted in the present case.' Oh! that was what they did not expect; it was giving a turn to the case which they did not like. 'You have heard the evidence, have you not?' 'Yes, but it is contradictory.' 'But judging from the evidence?' 'I have no right to judge; you may judge, if you please, or the jury may judge, but I shall not.' 'But I ask you, for the information of the jury, your opinion respecting the opium; whether you do not consider it too large a quantity for a child?' 'The statements respecting the opium have been contradictory; but admitting that the child had, as was said, eight drops immediately after the accident, and ten drops two hours after, I should say that the child had not taken one drop too much.' 'But are you aware that the child had no pain?' 'Yes, perfectly. When the skin, or any other part of the body, is severely injured, the nervous system of the part is so affected that the peculiar actions of the nerves on the brain, or spinal marrow, by which alone pain can be felt, do not take place. A man may have a serious injury inflicted by a mechanical cause—may have a leg smashed, and nearly torn off by machinery—and yet feel little or no pain; and

we are in the habit of giving, in such cases, wine and opium, not to act as a narcotic, but to arouse the energies of the vital powers, and call them again into exercise: the nervous system has sustained a great shock, from which it requires to be roused.' The judge said, 'that he understood it; he saw the principle on which the treatment was founded, and had no doubt but it was correct.' 'But, my lord,' said the counsel, 'the child slept to death.' 'So he may have appeared to sleep, but he would have done so if no opium had been given; it was the torpid state into which the nervous system had fallen, which caused that appearance, and from which the child could not be roused.' Here the business rested; the treatment was admitted to be correct, and the character of the gentleman exculpated."

20. *Duty of the medical witness to state fully every thing known to him.*—Bear in mind that the counsel and the judge are frequently placed under peculiar difficulties, difficulties arising from their want of information on the subject in question—difficulties which you are called upon for the express purpose of obviating. They may not know, and often they do not know, how to frame their questions so as to bring out the whole of the evidence of which you are in possession. There cannot be the slightest doubt as to your duty whenever you observe this. All that you know bearing upon the point in question, and the knowledge of which is necessary to form the ground of a just decision, you would be guilty of a flagrant violation of duty if you did not state, whether the questions that are put to you lead to the statement or not. In the language which I have so often repeated, you are bound, as far as in you lies, to give to your evidence, not only the property of correctness, but that of completeness also; by the terms of your oath, you are bound to speak, not only the truth, but the whole truth.

21. *Exception in the case of secrets communicated in professional confidence.*—Occasionally, indeed, you may be placed in a difficult and painful situation. By virtue of your office as professional men, secrets may have been confided to you by your patients, or you may have obtained possession of them in consequence of the implied confidence which they have reposed in you as medical men, which it is of the last importance to your patients to keep secret. It is in the power of a court of justice to require you to disclose facts even of this nature. However peremptorily commanded, I can conceive of cases in which a disclosure thus extorted must be extremely painful; and so much is it for

the interests of the community that every communication between the physician and his patient should be sacred, that I would myself never reveal any fact, to the knowledge of which I had come by means of this sort, unless required to do so in the most express manner by the court; and even then there may, I think, be cases in which strict integrity on the part of the medical man would justify, and even require, disobedience to the mandate.

CLINICAL LECTURE

ON A

CASE OF PUNCTURED WOUND OF THE ABDOMEN,

Delivered at the London Hospital, Dec. 19, 1837,

BY T. B. CURLING, ESQ.

A CASE of punctured wound of the abdomen, with protrusion and injury of the intestine, having been recently admitted into the hospital, I am desirous, whilst the particulars of it are fresh in your recollection, to call your attention to some of the principal points in the pathology and treatment of wounds of this kind which this case is calculated to illustrate. Injuries of this nature in these days of peace, and in the present civilized state of society, are of rare occurrence, although, by a singular coincidence, this is the third case of wounded abdomen which has been admitted into the hospitals of the metropolis within the last six weeks; the first having occurred at St. George's, and the second at the University Hospital.

The patient, Thomas Hawe, a stout young man, aged 25, was brought to the hospital on a shutter, about half past 11 at night, Dec. 12th, having been stabbed in the abdomen with a large blunt clasp-knife, about half an hour previously, in a quarrel at a neighbouring public-house, where he had been regaling. I was immediately sent for, and arrived within a few minutes after his admission. I found the man with a pallid countenance, lying on the floor bleeding, and his intestines protruding at a wound in the abdomen, ineffectual attempts having been made to return them. I immediately had the man placed on a bed in the ward, and proceeded to secure three large sized vessels in the mesentery, which had been divided and were bleeding copiously. The arteries were tied with fine silk ligatures, which were cut closely off. I then observed that about eighteen inches of the small intestines, with the mesentery attached, were protruding at a wound about an inch and

three-quarters in extent, on the right side of the abdomen, a little below the level of the umbilicus, in the direction of the linea semilunaris, but nearer the median line. The intestines were rather darker in colour than natural, and not collapsed. There were two wounds on opposite sides, and corresponding, shewing that the gut had been transfixed. The larger was transverse, and about eight lines in length; the other was about a line less in size, and at both the mucous membrane was protruding. I grasped the coats of the bowel around each of the openings between the blades of a broad pair of elastic forceps, and caused fine silk ligatures to be applied, and the ends cut shortly off. The protruded intestine was then gently pushed back into the cavity of the abdomen, but not without some little difficulty, and during the operation the man complained greatly of pain, was very restless, and required the aid of assistants to keep him quiet. The wound in the skin was dressed with a compress, and gentle pressure was directed to be kept up, in order to prevent reprotrusion. A long incision, skin-deep only, at the back part of the right thigh, was likewise dressed. When I first saw him the patient had vomited, and was in such a state of collapse, that, after securing the bleeding vessels, I immediately directed some warm brandy and water, and forty drops of laudanum, to be given him, but it was very soon rejected from the stomach.

I saw the man again in five hours. He was still very low, and in great pain, and his pulse was quick and small. He had not vomited during the last hour, and I ordered him to take forty more drops of laudanum, and fomentations to be applied to the abdomen.

About half-past 9 in the morning the pulse had risen considerably, the tongue was dry, and the man complained of thirst, and was still in much pain. The nurse had given him a little tea, which had been retained on the stomach, but it was directed to be discontinued, and the mouth only allowed to be moistened. Two grains of calomel, with half a grain of opium, every fourth hour, with leeches to the abdomen, and fomentations, were prescribed. About 12 at noon sixteen ounces of blood were taken from the arm by one of the house pupils.

2 P.M. — I saw the patient with Mr. Andrews and Mr. Adams. His pulse was weaker than in the morning, but equally quick; there was great tenderness of the abdomen, and constant pain, with frequent vomiting, and short and hurried respiration. In consequence of the irritable state of the stomach, it was considered inadvisable to continue the administration of

remedies by the mouth. A clyster, therefore, of an ounce of gruel, containing a drachm of laudanum, was ordered to be given immediately, and a drachm of the ung. hydrarg. fort. to be well rubbed in on the inner part of the thighs every hour. From this time the man sank gradually, and died at a little before ten at night, having survived the wound only twenty-three hours.

The body was examined sixteen hours after death. It was muscular and well-formed, and the surface exsanguineous. In the vicinity of the external wound the subcutaneous cellular tissue was infiltrated with blood and serum. The lower belly of the rectus muscle had been divided in the direction of its fibres, and on reflecting it from the posterior part of the sheath, the wound in the fascia and peritoneum was seen to be about half an inch on the outer side of the epigastric artery, in size corresponding to the external wound. The peritoneal coat of the small intestines appeared much injected, especially on the right side towards the iliac fossa, in which situation there was a small quantity of faecal matter. A small part of the canal was slightly coated with lymph. The larger of the two wounds was still open, and presented the hernia of the mucous membrane characteristic of incised wounds of the intestine, the ligature having slipped off. It was discovered that the ileum, about an inch from its termination, had also been transixed; for two longitudinal incised wounds, corresponding to each other, and five lines in extent, were likewise found at this part of the bowel. Immediately beneath was observed a wound in the peritoneum and fascia, which extended half through the *psoas magnus* muscle. It was situated about half an inch to the outside of the external iliac artery. There was ecchymosis in the cellular tissue around the muscle and beneath the cæcum, and a small quantity of faecal matter, mixed with purulent serum, in the pelvis. About eight inches from the first-mentioned wound in the intestine there was a small spot of sanguineous extravasation in the mesentery, at the point where it joins the intestine, the peritoneum being slightly lacerated. The heart, lungs, and liver, were healthy.

From the particulars which have just been related, you will readily perceive that these cases are of a formidable and embarrassing nature, and require prompt treatment on the part of the surgeon. The source of immediate and most imminent danger was the free hæmorrhage from the branches of the superior mesenteric artery; vessels large in size, near the centre of the circulation, and so situated

as not easily to admit of the bleeding being spontaneously arrested by their retracting within a cellular sheath. These vessels were, however, readily secured, and all danger of this kind soon removed. I had next to determine what was to be done with the ends of the ligatures. Should they be cut closely off, or should they be left attached, when the gut was returned, with the extremities hanging out at the wound? Had they been applied to vessels in the omentum, I should have had no hesitation in cutting off one end of each, and confining the other at the external wound; so that if the patient survived, there might be no difficulty in bringing it away after separation had taken place, the omentum being placed in front of the intestines. But there are several objections to this proceeding, when the ligatures are applied on vessels in the mesentery. Thus, when the parts are returned, as we cannot well confine the mesentery near the wound, situated as it is behind the intestines, if the ligatures are suffered to remain, they must necessarily trail amongst the bowels, and thereby tend to excite and keep up peritoneal inflammation. If, too, the ligatures were attached, or at all confined at the external wound, and did not yield, they would be liable to girt or even produce stricture of the canal; whereas, if loose, they might be drawn, by the motions of the viscera, completely into the peritoneal cavity, and be involved amongst the convolutions; an occurrence which I have known to happen once, in the case of ligature applied to a vessel in the omentum, in the operation for inguinal hernia. The disadvantage of cutting both ends closely off before reducing the protrusion, arises from the circumstance that, after the ligatures have separated, there being no ready mode of escape, they must remain as extraneous bodies, and continue to keep up irritation in the cavity. This would certainly appear to be an inconvenience attending this plan; nevertheless I adopted it as the least objectionable of the two. Besides, the ligatures constituting but a small mass, would, if the patient recovered, be so surrounded with lymph as to be confined to the situation in which they were placed, and, like foreign bodies, as bullets, or some of the entozoa which have frequently been found in the centre or in the vicinity of the most important organs, they might remain encysted for years without causing any inconvenience.

Having disposed, then, of the question as to the means to be pursued in reference to the divided mesenteric vessels, the next point to decide was the treatment to be adopted in regard to the wounded intestine; for if returned into the peritoneal

cavity in the condition in which it was found, the escape of faecal matter from one, if not from both, of the openings, was certainly inevitable. This opens an inquiry of much interest and importance, which has at various times occupied the attention of medical men, though the practice founded upon it cannot yet be said to be firmly established, or finally settled. Formerly, when swords were worn and duelling was much in fashion, punctured wounds of the abdomen were of common occurrence; accordingly we find, on consulting the works of the older writers on surgery, especially the excellent memoirs of the French Academy of Surgery, that many of the surgeons of those days exhausted their ingenuity in devising plans to effect the union of the injured bowel by different kinds of sutures and mechanical contrivances, which they considered to be essential to the healing of the wound and to the patient's recovery. The best and most experienced modern surgeons are of opinion that sutures were employed formerly much too freely, and that too little was left to nature. Scarpa even has gone so far as to assert, that in penetrating wounds of the abdomen, accompanied with an injury to the intestine, whether the canal be divided longitudinally or transversely, a suture is always not only useless, but even dangerous and fatal. I would say, that experience does not altogether sustain Scarpa in this sweeping conclusion, although the observations of that eminent surgeon, as well as those of many other modern pathologists, clearly show that a safer and more simple plan of treatment may frequently be adopted with advantage and success. The course to be pursued in these cases must evidently depend, in a great measure, upon the nature and extent of the wound. It is here necessary that you should bear in mind that an opening in the intestine, from an incised wound, differs from an aperture consequent upon ulceration, in one respect, which is well worthy of notice. It is this: that when the opening is produced by a wound, there is always more or less protrusion or hernia of the mucous membrane; so that when the wound is small, the edges appear everted, and the mucous membrane is seen of its natural rosy hue, protruding in folds and blocking up the wound. Now this kind of hernial protrusion was remarked in the case just related, and may even be observed in the portion of the wounded bowel removed from the deceased, that I now show you. When the opening is consequent upon ulceration, as was seen in the examination of the case of hernia operated on last week, the mucous membrane being destroyed by the ulcerative

process to a greater extent than the peritoneal, the edges are thin, and altogether different, and offer no bar to faecal effusion. If, then, there is only a slight puncture, the hernial protrusion is sufficient to prevent the escape of the faeculent contents of the canal, and the application of a suture, therefore, is both an unnecessary and injurious interference. The surgeon has only to return the intestine into the abdomen, and then leave it to chance; in which case it will, in all probability, soon contract an adhesion to a neighbouring fold, or become coated with lymph, and thus be placed in a condition favourable for ultimate recovery. In confirmation of the accuracy of this observation, I may recal to the recollection of some present the last case of wounded abdomen admitted into this hospital. It was the case of a man who accidentally fell backwards against a pitch-fork. One prong wounded only the muscles at the back of the pelvis; the other entered the right lumbar region, penetrated the peritoneum close to the ascending colon, which, however, escaped injury, and then transfixed a fold of the ileum. The man died two days afterwards of peritoneal inflammation, when the nature of the mischief was ascertained. There was an abundant effusion of lymph, by which the openings were concealed, and the wounded intestine connected to the neighbouring folds on each side, but no feculent matter could be found in the cavity. In this case, the escape of the faeces was prevented, in the first instance, by the hernial protrusion of the mucous membrane, and subsequently by the effusion of lymph. A slight puncture, therefore, in the coats of the intestine, is the least dangerous kind of wound, and one requiring the simplest treatment.

The wound may, however, as in the case of Thomas Hawe, be more extensive, so that the protruded mucous membrane would be insufficient to close it effectually and prevent faecal effusion. In a case of this kind, some surgeons recommend that a ligature should be passed through the mesentery and around the intestine, and that the bowel should, by this means, be confined at the external wound, where it would very soon become adherent, and through which the contents of the canal would be discharged, the patient recovering with an artificial anus, which might subsequently close spontaneously or admit of being cured by operation. Scarpa regarded the use of the ligature even in this case as unnecessary, and was of opinion that the wounded intestine would readily unite at the external wound without it. But notwithstanding the rapidity with which adhesions form, there must necessarily be a considerable risk, both from the

motions occurring in respiration and from the peristaltic action of the canal itself, of the injured intestine quitting the outer wound. At any rate, in the case now under our notice, where no measures were taken to confine the intestine, the wounded part was found some way from the opening in the peritonæum, and concealed amongst the convolutions. In a case of hernia, where an opening in the gut is consequent upon ulceration or mortification produced by the stricture, the circumstances are altogether different, for here peritoneal inflammation precedes the lesion in the coats of the bowel, so that there is scarcely any risk of its moving away. In an injury, therefore, of the intestine of this kind, I should be disposed, if I applied no suture for the purpose of closing the opening in the intestine, to pass a ligature through the edges of the aperture, and attach it to the sides of the external wound, and in this way secure the gut from quitting the part. In the case, however, of this patient, there were greater difficulties to contend with. The intestine had been transfixed; there were two wounds, one on each side of the canal, so that this proceeding could only be applied to one of the openings. I therefore adopted a plan once employed by Sir A. Cooper, in operating in a case of strangulated hernia. An aperture giving issue to the intestinal contents was discovered in a portion of the sound bowel just when the part was about to be reduced. Sir Astley, including the aperture in his forceps, caused a fine silk ligature to be carried beneath the point of the instrument, firmly tied upon the gut and the ends cut off close to the intestine. The part was then replaced, and the patient did well. Ligatures were then applied to both of the wounds in the intestine. The one applied to the larger of the two, either not having been tied with sufficient tightness by my assistant, or having been displaced by the long manipulation necessary, in order to return the protruded parts, or the wound being too large to admit of being safely secured in this way, slipped from its hold; and had the patient survived, would not have accomplished the object that I had in view. The other aperture, you will perceive, is completely closed, without much diminution in the calibre of the gut. You might suppose, that when a ligature is applied to a wound of this kind in the way mentioned, that the result is the union of the parts thus kept in apposition, as happens in the case of a ligature upon an artery. This, however, is not the case, for the surfaces immediately in contact being mucous, are by no means disposed to unite, and the ligature only serves to close the wound and prevent fecal effusion until the injured

bowel has contracted a firm adhesion to the neighbouring parts. When a ligature is applied to any part of the alimentary canal, it first cuts through the inner, the mucous and muscular coats, leaving the exterior, the peritoneal, in tact. The part having been encased in lymph, the process of separation soon commences, ulceration is set up, and the ligature finds its way into the bowel, from whence it is ultimately discharged by the anus. In many cases the wound is too large to admit of the application of a ligature in this way; it becomes necessary, therefore, to apply either the interrupted or the continued suture. In expectation that the wound would unite more readily if two serous surfaces could be kept in contact, Jobert, a French surgeon, proposed to invert the edges of the wound before the application of the sutures, in order to obtain union between the peritoneal coats. I do not regard this as a point of much importance, when the gut is only partially divided, since whatever kind of suture is employed, direct union of the parts brought together is by no means essential to the subsequent integrity of the bowel; and provided that the first object, the escape of fecal matter, is obtained, there is afterwards so free an effusion of plastic lymph, that the continuity of the canal is ultimately effected in the way I have described, without any future risk.

When there is a complete division of the canal and protrusion, it is necessary either to leave the divided ends of the intestine at the external wound, thus giving the patient the chance of recovery with an artificial anus, or to bring the two extremities together, and confine them with sutures. I should certainly prefer the former practice, which succeeded in a case treated in this way by Baron Larrey, and is, I think, attended with the least risk. Mr. Travers states, in his *Inquiry into the Process of Nature in repairing Injuries of the Intestines*, a work which will well repay you for an attentive perusal, that "wounds amounting to a direct division of the canal are irreparable, and, therefore, invariably fatal." There are cases, however, on record, in which recovery has taken place after an injury even of this severe description. Mr. Travers endeavours to show, that in transverse wounds of the canal the longitudinal fibres constantly tend to retract the edges, and consequently he recommends the employment of the uninterrupted or Glover's suture. But it does not appear that the absolute contact of every part of the divided bowel is absolutely essential to cure, for Sir A. Cooper, and Dr. John Thomson of Edinburgh, in experimenting upon dogs, succeeded in obtaining union of a

divided intestine by using the interrupted suture. The suture which I believe to be best adapted for the purpose is passed as follows:—Introduce from without, inwards, at about four or five lines from the solution of continuity, a small curved needle, armed with a fine silk ligature, through all the coats of the intestine, except the mucous, and bring it out externally at about a line from the wound. Pass it then in the same manner through the other end of the gut, commencing at a line from the divided part, and bringing it out at the distance of four lines. Having passed the number required, which in a complete division of the canal need not exceed four, the ends of the bowel are to be brought together and the ligatures tied separately, taking care at the same time to invert the edges of the wound so as to bring the serous surfaces in contact. Having made a double knot, the ends are to be cut off close. The direct union of the peritoneal surfaces in complete division of the intestine is to be desired, since, in order to perfect the union, the whole circumference of the canal must become adherent or coated with lymph, which, if not tolerably firm, might, after the separation of the ligatures, yield to the action of the longitudinal fibres of the intestine. Jules Cloquet adopted this plan, with success, in a case of divided intestine, and applied only two sutures. Other modes of effecting the same object have been proposed. Thus it has been suggested to introduce the upper portion of the wounded bowel into the lower, over a cylinder of isinglass, or the trachea of some animal, and then secure them with ligatures. But as the propriety of most of these plans is very questionable, I think it needless to dwell upon the subject.

But whatever practice may be followed, our chief reliance for success is on the early setting in of active peritoneal inflammation and the rapid and abundant effusion of lymph. I would say that a copious hæmorrhage, such as occurred in the case of this patient, was a circumstance highly unfavourable in an injury of this description, since it tends to retard the excitement of inflammatory action, upon the early development of which the probability of recovery mainly depends. This poor fellow, when first admitted, was so sunk from loss of blood, that I scarcely expected that he would live throughout the night, and I considered it necessary to give brandy to restore him. He recovered slowly, was restless, and could scarcely be restrained from moving about, so that every opportunity was afforded for the escape of fecal matter with which the alimentary canal had evidently been distended. The post-mortem examination

exhibited merely a thin coating of lymph, effused over a small part of the canal, none having been found around the ligature and wounded parts, although the patient had survived more than twenty-three hours, a period which must be considered fully sufficient for effusion to take place, when it is recollected that in an instance adduced by Sir E. Home, lymph was effused and organized on an inflamed intestine in less than twenty-four hours.

Of course it was not discovered until after the patient's death that the case was really more serious than it appeared when he was admitted. It was impossible to ascertain during life that a second fold of the ileum had been transixed, and that the knife had penetrated even the *psaos magnus* muscle. It would appear that the blow must have been struck with great force, and that the knife had taken rather an oblique direction. The external iliac artery narrowly escaped being wounded.

Having now considered the more important points connected with this case, I have but little further to add. There was some difficulty experienced in returning the protruded parts back into the abdomen. It has been suggested that when there is no wound, and much difficulty exists, it would be proper to puncture the coats of the intestine with a needle, in order to allow the escape of the air contained within. It is better, however, to dilate the wound in the peritoneum, in performing which care must be taken to avoid the epigastric artery. The wound in the integuments was closed with adhesive plaster, and an assistant was directed to keep up gentle pressure with the hand, in order to prevent reprotrusion. Formerly surgeons were so much afraid of the intestine appearing again at the wound, that they stitched up the aperture in the peritoneum and applied the quill suture externally. But these are proceedings which are abandoned in the present day.

The after-treatment in injuries of this nature chiefly consists in subduing the peritoneal inflammation, which, though desirable in the first instance, if not checked, soon becomes the principal source of danger. It must be combated by local and general bleeding, mercury, fomentations, low diet, and perfect rest. Purgatives are objectionable, as they tend to disturb the intestines, which should be kept as quiet as possible; but clysters may often be given with advantage, unless the large intestines should happen to be wounded. Opiates are admissible, especially if there is much suffering.

In conclusion, let me remark, that formidable and dangerous as is the character of punctured wounds of the abdomen, attended with protrusion and injury of the

intestinal canal, these cases are not necessarily and invariably fatal; and the efforts of nature are sometimes so extraordinary, that it is always the duty of the surgeon to call in aid all the resources of his art, and to adopt whatever his skill and judgment can suggest. The records of our profession, and the experience of military surgeons, furnish many instances of almost miraculous recoveries after wounds of this description. Let, then, the recollection of these cases serve to encourage us in the execution of our dispiriting task.

RECOLLECTIONS OF CHOLERA;

ITS NATURE AND TREATMENT.

BY WILLIAM GRIFFIN, M.D.

Limerick.

Introductory.

AFTER the passing away of a pestilence which spread terror and death throughout these countries for so long a period, and on whose treatment, unsuccessful as it has been, information was so liberally supplied by medical men, it may seem a very hopeless attempt to engage public interest to the subject again. One naturally gets tired of the endless conjectures and experiments of the profession, always multiplied just in the proportion they are erroneous or fruitless; and with a great many, the mere probability that we shall not be visited by the dreadful epidemic any more, at least in our own times, is fully sufficient to lay all spirit of inquiry. Those, however, who really desire the improvement of medical science must not write for an age or a country; for as the value of a fact or a truth can never be increased by any interest it obtains, so it can never be diminished by being for a time neglected. For my own part, convinced that the observations I shall offer in the subsequent pages are of some practical importance, I can have no anxiety about their immediate influence. They may, perhaps, have little interest at present for any, except for those few zealous gleaners in the fields of science, who are yet lingering to pick up some ears after a wasted harvest; but even through them they may reach those parts of Europe where the cholera still prevails, and where any suggestions thrown out must admit of immediate application.

I am not singular in having seen a great deal of this unfortunate malady, but my opportunities of observation have, I believe, exceeded those of any gentleman who has written upon the subject in this country. The city in which I reside has been more severely visited than almost any other in Ireland. I have myself treated many hundred cases, and been attached successively to the three hospitals in which the disease shewed itself in its greatest malignity. I have seen the disease both in town and country, and watched the modifications which might have resulted from situation. I have seen it totally abandoned to nature; and in the anxious effort to save life I have sometimes seen so much done, that nature was left no opportunity of exerting herself. Finally, I have received much useful information from my professional friends, which assisted me in arriving at just conclusions on many points of extreme doubt and difficulty.

I do not profess, in these papers, to bring any new discovery under consideration, or to throw any clearer light on the pathological condition which constitutes cholera; that seems to be still as mysterious as is the seat or essential condition of fever, and, I believe, is as likely to continue so. My chief object has been to give statements of facts and results, in such a manner as must make the connexion of the latter with the former obvious, and so lead to unquestionable inferences; and I have endeavoured to determine the application and comparative merits of a particular treatment, not unsuccessfully adopted in other countries, but carried to a greater extent, and given a fairer trial, in Limerick than any where else. If, on reviewing the merits of various other modes of cure, tested as this has been by the results of numerous cases accurately classified, it should appear to advantage, as I believe it will, it may only require a little experience to qualify the disagreeable consequences by which it is sometimes followed. But, on the other hand, should it be ascertained that other remedies are of equal or greater success, and less objectionable, I shall yet feel that I have effected much good, in having been instrumental in placing absolute inductions, with respect to their value, on record, and preventing new adventurers from seducing the profession by accounts of as-

sumed specifics, whose only claim to reputation is that fictitious one which they have acquired in the imagination of the discoverer.

I have, however, also aimed at the attainment of what may prove hereafter of far more importance than any thing I could offer on the subject of treatment—a mode of fairly comparing one line of practice with another, without reference to mere medical opinion. This, a simple matter in most other diseases, and certainly not impossible in cholera, has been rendered inconceivably difficult, partly from the varying nature of the disease itself, and our ignorance of the resources of nature; partly from a disingenuous and wicked disposition in a portion of the profession to magnify the success of particular remedies, for the sake of a temporary celebrity or gain; but perhaps most of all from the unphilosophical precipitation with which most medical men pass to general conclusions on contemplating a few isolated cases.

The consequence of this last and most fatal error is just what might have been anticipated; that we are now, after certainly a very sufficient intimacy with the disease, almost as much in doubt about every thing connected with it, as when it commenced at Sunderland. If one asks what is its nature? some will answer that it is a simple local inflammation; others, that it is a species of epidemic fever; a third party, a corruption of the circulating fluids. If one inquires where is its seat, or what texture it affects? we are told, the brain—the spinal cord—the solar ganglia—the alimentary canal—or the whole mass of the blood; and it would fain be proved, even by pathological evidence, that it is each, or any of these. If one demands whether it be contagious or not? half the profession rise up on either side of the question. I do not, of course, mean to deny that some important negative advantages have been derived from our experience—that physicians in general have become more familiar with the character of the complaint, or that they are not individually better satisfied with the efficacy of certain remedial measures. But we have sufficient evidence in the periodical publications, that, as a body, they are little agreed about any—nay, that they are actually opposed to one another as to the value of these

which are most influential, and which must of necessity effect either extraordinary good, or incalculable mischief. The calomel and the saline treatment, the brandy and cold water, stimulants and antiphlogistics, vomits and sedatives, purgatives and astringents, have all to this hour steadfast and respectable advocates and opponents; and what can be more mortifying than the conflicting sentiments expressed regarding blood-letting? One authority is for drawing no blood at all, early or late, cholera being a disease of debility which will not admit of such treatment. Another advises to take it at an early period of the complaint, but never in the stage of collapse. A third (Dr. Thackrah) observes, that we should bleed in the collapse, but not abstract too much, since a small bleeding is fully sufficient to relieve the venous congestion. On the other hand Mr. Bell asserts (and he is supported by most of the Indian practitioners), that small bleedings do harm, but free venesection, where practicable, has scarcely ever failed. Dr. Brown regards a considerable degree of collapse, indicated by feebleness or arrest of the circulation, as an imperative reason for abstaining from drawing blood. If the temperature, he says, be in any considerable degree below the healthy standard, with the hands cooling rapidly on exposure to air, the pulse at the wrist either very feeble, or totally suspended, the breath and tongue cool, the surface shrunk and pallid, or in certain parts livid, the vomiting and spasms diminishing in their intensity, or totally ceased; at whatever period of the disease this state of things may exist, bleeding is inadmissible. Mr. Spence, who saw much of the disease, also says, to think of bleeding a person in such circumstances, is against every principle of common sense. Mr. Bell, on the contrary, recommends “that no time should be lost in endeavouring to bleed the patient, whenever there is a sunk countenance, with blue lips and nails;” and as to the extent to which depletion is to be carried, he tells us “it has no reference to the prostration of strength or constitutional stamina.” “When the blood flows *stillatim*, black and tarry, stimulants,” he says, “sometimes appear to do good, and to increase the strength of the pulse, upon which renewed attempts should be immediate-

ly made to remove blood, *nor should these be desisted from while life remains.*"

Such is medical opinion, and such are the foundations on which inexperienced practitioners are expected to build their practice! If in their stead we are furnished with a simple report of only twenty cases of collapse, in which bleeding had been resorted to, and as many others, *ceteris paribus*, in which it had not been employed, with the results in both instances, what opinion could either add to or take away from the value of such a document? Accurate and candid reports are all we can want to remove the clouds which at present obscure our inquiries; not such as are daily issued by Boards of Health, or even by hospital physicians, from which little could be gathered beyond the amount of the dead, but statements of the admissions, classified as the patients may happen to be, in the premonitory, or primary, or collapsed stage of cholera, with the general plan of treatment, and the results in each. Almost all the controversy with regard to the treatment of cholera has arisen from the remedies having been employed at different periods of the disease, and afterwards extolled as available in any stage of it. If, in reported cases, we have no distinction or classification; if they are, however unlike, jumbled together by a common cypher, all inquiry is baffled. There can be no comparison between the treatment in one hospital and another, nor is it possible for one man to reason on another man's practice.

The two points to which the public chiefly direct their attention in estimating the success of any particular treatment, are, the numerical amounts of deaths and recoveries, or their proportions to one another. How superficial and erroneous inferences formed in this way have proved, is manifest from the fact now well ascertained, that the mortality has varied considerably under the same treatment, and has at other times rated at exactly the same under very different treatment. None, indeed, but medical men, can fully comprehend the extreme difficulty which attends this subject, a difficulty which can only be surmounted by a thorough knowledge of the modes or habits of the complaint—the circumstances which may influence its phenomena—or the

effects of remedies in controlling or altering them.

The rate of mortality or of recovery always has an infinitely closer relation to states of the atmosphere than it has to any treatment whatsoever; the poisonous miasmata with which it is loaded, either varying in quantity or intensity, or losing or acquiring activity from certain changes which escape our observation. In the early stage of the disease this is chiefly remarkable in the rapidity with which patients fall into collapse at one time, the tardiness at another, and the facility or difficulty of arresting it by remedies. In the latter stage of the disease or collapse it is not only observable in its intensity, or the contrary, but in the varying proportion in which the admissions into hospital in that state occur, and as a consequence in which patients recover or die.

On considering the rate of mortality in cholera throughout Europe, one is necessarily struck with the fact, that under every possible variety of management and medicine, the average has, in each country, been very much the same. It has been hence hastily inferred by general reasoners, that the influence of medicine in this disease really amounts to little or nothing. The circumstance, however, is easy of explanation to those whose information extends a little into details. In violent incursions of cholera, those who are attacked fall into collapse, and die every where before remedies can be efficiently applied, so that the difference of mortality between one country and another must be inappreciable, or relate only to the virulence of the epidemic. On the other hand, when it sets in under a milder form, when there is time for the application of remedies, and even when they are in a great proportion of cases efficacious, there may still be no essential difference in the average recoveries; as in a complaint about the treatment of which there exists such a variety of opinions, even among the best informed of the medical profession, we may infer fairly that the average amount of judicious and injudicious treatment is in every country pretty much the same. Deductions, as to the influence of remedies, from large averages of the mortality, are in fact, too loose and liable to error, to be worth a moment's consideration in medical sci-

ence. The comparisons which are to influence our opinions of medical remedies must be much more close and accurate, not only as to the conditions of the patients when they first fell under treatment, but as to the conditions of the atmosphere, and locality in which the disease originated. If the inquiry be entered into in this spirit of fairness and accuracy, it will not be difficult to show that in the vast number of cholera cases in which there is the least time for the influence of medicine, that influence in skilful hands is more evident, and, to use so proud an expression, more triumphant, than in any other disease whatsoever.

There is one of those circumstances to which I have referred, that must make the comparative influence of different remedies in the treatment of cholera a matter of great moment: I mean the variable nature of the disease itself. Those of second-rate power, which sometimes obtain great celebrity whenever it begins to assume its milder form, are clearly not only useless but mischievous, in cases of a severer type, taking place, as they do, of medicines on whose timely exhibition the lives of the patients depend. The occasional success or efficacy of remedies of inferior power is, indeed, far from advantageous to the sick, in most dangerous diseases, and is unquestionably an evil, as it relates to such as are eminently destructive, since it tempts the inexperienced practitioner to trust to them in cases where their administration leads to utter failure. I have often questioned whether, as regards cholera, it would not be better for the community that medicines of that class had never been discovered or applied. The practice of those who are too indolent or indifferent to make the respective merits of remedies a subject of important study, would then be confined to such as are influential under the worst states, compatible with recovery.

In concluding what may, perhaps, be considered a somewhat tedious introduction to my recollections of cholera, and to those impressions respecting its treatment to which they led, it will be seen that I have not sought to attach value to any views I entertain which are founded on mere opinions; such I have already deprecated; and that nothing is confidently advanced which is not a fair induction from an extensive collection

of facts. My object has, indeed, been not so much to win public attention by new suggestions, new remedies, or new modes of management, as to point out the proper application and merits of some already before the world, and prove the necessity of more accurate hospital reporting, if we would have our labours become in any way useful to the future scientific inquirer, or advantageous to mankind.

Before entering into discussions on these subjects, however, I shall venture to give a short sketch of the incursion and progress of the disease, from its commencement to its close, in Limerick.

[To be continued.]

NEW STETHOSCOPE.

To the Editor of the Medical Gazette.

SIR,

I HAVE read with much interest the opinions of Drs. Budd and Williams on the acoustic principle of the stethoscope, with the latter of whom I fully agree that the sounds are conveyed from the parietes of the chest to the ear, *principally* by the air contained within the tube, and that they are rendered more intense by their reflection from the internal surface of the instrument, and by their consequent convergence.

This being the case, I cannot but think that, by a little more attention to the mathematical principles of acoustics in the construction of the stethoscope, its powers and utility may be much increased. As it is now made, the inner surface is generally left rough and unequal, which must in a considerable degree diminish the intensity of the sounds; and the excavation at the lower end is made without any fixed principle, being bell-shaped or conical according to the fancy of the maker.

Now if the body of the instrument could be hollowed out in the form of a very eccentric ellipsis, and the ends cut off by planes passing through the foci, the rays of sound which enter one end would, after one reflection, all converge at the other. This portion is for examining the sounds caused by the voice, as pectoriloquy, &c. For the purpose of examining the respiratory sounds, another part must be added, excavated in

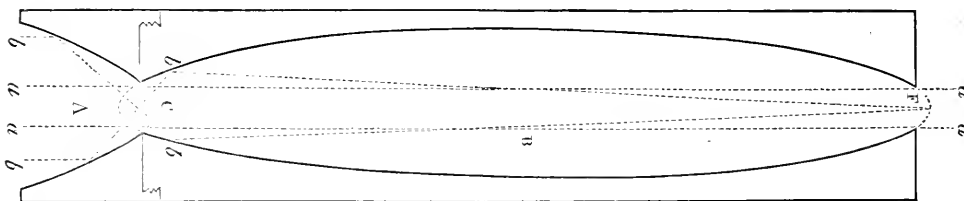
the form of a parabola, having its latus rectum equal to that of the ellipsis, and its upper portion cut off by a plane passing through its focus. This part of the instrument should be so adapted to the other, that the focus of the parabola may coincide with one of the foci of the ellipsis.

It is unnecessary for me to add, that all rays of sound entering the lower end of this stethoscope parallel to its axis, will, after two reflections, converge at its upper end. In regard to the oblique rays, this instrument possesses the same advantages or disadvantages as the one now in use.

The murmuring sound which is heard when hollow bodies are applied to the ear, may be diminished by covering the outside with some bad conductor of sound.

There will be some difficulty in turning the curves above described, so that they shall be perfectly accurate. I believe, however, that even an approximation to this form would render the instrument more serviceable than the one now in use.

I subjoin a diagram, and the dimensions which appear to me most convenient.



A, the parabola. B, the ellipsis. C, focus common to the two curves. The rays, $a a$, and all included between them, will pass directly to the superior opening; $b b$, which fall on the curve, will be reflected, and pass through the focus C, and ultimately converge at the focus F.

Major axis of ellipsis, 8 inches.

Minor ditto, 1.4 inches.

Latus rectum, or aperture, .25 inches.

Length of this part of the stethoscope, 7.9 inches, nearly.

Abscissa of the parabola, 1.2 inches.

Diameter of the lower end, 1.5 inches.

Latus rectum, .25 inches.

Length of the whole instrument, 9 inches, nearly.

The equation to the latter curve, $7^2 = 4ax$, becomes (since $4a = .25$) $2y = \sqrt{x}$, or the diameter of the lower opening = the square root of the length of this portion.

The diameter of the small apertures ($\frac{1}{4}$ of an inch) being smaller than that usually employed, it may be lengthened by rounding off the sharp edges.

Yours respectfully,

HENRY BARRETT,
Surgeon.

Watlington, Oxon.
Dec. 14, 1837.

FATAL DISEASE OF THE STOMACH.

To the Editor of the Medical Gazette.

SIR,

SHOULD you think the following case worthy of being recorded in your Gazette, you will oblige me by giving it a place there.

Margaret B—, aged 18, had consulted me some months ago for occasional gastralgia, which was removed by mild aperients, a system of diet, and

the subnitrate of bismuth. Since that period she has appeared to be in perfect health, with the exception of habitual flatulence, and has been active in her duties as lady's maid. Yesterday, Dec. 22nd, whilst engaged in some domestic avocation, she uttered a violent scream, leaned forward over a table, and complained of acute pain in the umbilical region. It being the day on which the menstrual period should have occurred, I at first regarded the attack as arising from this cause, but inflammatory symptoms soon presented them-

selves, the pulse became extremely quick, small, and depressed, the abdomen tender, the extremities colder than natural, the countenance expressive of hopeless anxiety, and the stomach occasionally, but not distressingly, rejected its contents. Bleeding, instead of being followed by the satisfactory rise of the pulse, which we look for in ordinary cases of membranous inflammation, caused only greater rapidity and further exhaustion. The bowels were moderately moved, but from the moment of the seizure to its fatal termination, no remedy gave relief, and in twenty-four hours the poor girl was released from intense suffering.

Post-mortem examination six hours after death.—The body (which inclined to “*embonpoint*” rather than emaciation,) presented no mark of disease externally, excepting a tympanitic state of the abdomen, which had commenced slightly before death.

The abdomen contained some air, and from three to four pints of a turbid fluid. Recent and slight inflammation was evident on the peritoneal coat of the transverse colon: at its junction with the ileum, the inflammatory appearances were more decided, and at one or two parts had gone on to deposit of lymph; there were several recent adhesions in the vicinity of the duodenum, but the cause of death was apparent in an ulcer at the smaller arch of the stomach, near the *œsophagus*. At this spot the stomach was slightly adherent to the under surface of the liver, to the extent of a shilling, the corresponding portions of both organs being, as it were, shaded by ulceration. Here a circular aperture, about the size of a duck-shot, and having a well-marked gangrened margin, had allowed the contents of the stomach to escape into the abdominal cavity. The internal surface of the stomach presented a funnel-shaped ulcer through its coats, which would admit the end of the forefinger. I may here state that, in answer to my question, “have you any other pain than that in the bowels,” she said, “Yes, between my shoulders and here,” (pointing to the sternum), “but this is nothing compared to the other.”

I regard this case as illustrative of three points, namely,—1st, To what an extent disease of the cardiac extremity of the stomach may proceed without

apparent loss of health.—2ndly, How beautifully Nature makes her attempt to repair an injury, as in this instance, by the long-standing adhesion of the liver to the attenuated stomach: and, 3rdly, How completely and instantaneously she gives up the contest, when that injury becomes irreparable. Dr. Abercrombie, in his excellent work, cites a remarkably similar instance: and I have been the more induced to trouble you with this communication, in the hope that this similarity of symptoms may be considered practically diagnostic, although I need not say of a hopeless case.

I am, sir, with much respect,

Your obedient humble servant,

JOHN PRICHARD.

Leamington, Dec. 23, 1837.

MEDICAL GAZETTE.

Saturday, January 6, 1838.

“*Licet omnibus, licet etiam mihi, dignitatem Artis Medicæ tueri; potestas modo vendendi in publicum sit, dicendi periculum non recuso.*”

CICERO.

POISONOUS CANDLES.

It has often been asserted that London is the healthiest of the great cities of Europe. Unfortunately, however, this boast is founded upon the Bills of Mortality; and the incorrectness of the Bills is but too evident to every one who has inquired into the subject. If we give up the position founded upon the “numerical method,” as untenable, and attempt to balance the destructive influences at work in London against those which harass other cities, the problem becomes too complicated to be solved.

It is clear that almost all the agents of death and disease are in full activity around us; spoiled provisions are the habitual food of large classes of the population; the air is deteriorated by noisome exhalations, from the decomposition of animal and vegetable matter; and the more indigent inhabitants of

London are crowded together in a manner incompatible with health, and almost with life. It may perhaps be urged that many, or all of these noxious agents, are to be found in still greater force in foreign cities; and that if the Londoner drinks opiated beer, the Parisian is obliged to swallow leaded wine. A new source of death, however, unknown to our fathers, has been introduced among us, to which this argument will not apply—we speak of the arsenical candles, by which one of the most fatal of poisons is spread in a gaseous form over the dwellings of thousands of families. The same deadly manufacture was, indeed, tried in Paris, but was put a stop to by the vigilance of the *Conseil de Salubrité* and the police.

The merit of discovering the presence of arsenic in candles, in London, is due to Mr. Everitt, who mentioned it about six months ago, in a lecture delivered before the Medico-Botanical Society. The subject has since been frequently discussed at the Westminster Medical Society, who appointed a Committee to investigate it. Their report, drawn up by Dr. Granville in a clear and judicious manner, is now before us*.

It appears that at the meeting of the 28th of October, a member informed the Society that a hypochondriac had supposed himself injuriously affected by the use of the candles supplied to him at an hotel; and had in consequence sent a piece of one to a professor of chemistry, who, on analysis, found that it contained a sufficient quantity of arsenic to account for the morbid symptoms described by the patient. After a protracted discussion, the Society, on the 4th of November, 1837, came to the resolution of appointing a Committee to inquire into the presence of the poison in candles, and to ascertain in what

proportion it was contained in them, as well as to consider the probable effect the arsenic might have on animal life during combustion.

The reporter observes that Chevreul, one of the most distinguished French chemists of the present day, in the course of an investigation into the nature and elementary composition of animal fats which lasted ten years, discovered that common tallow consisted of two distinct substances. The one was solid, and the other was of the consistency of olive oil: he called the former *stearine*, and the latter *elaine*.

Stearine possesses two great advantages over tallow; in the first place, it is almost free from smell, and, secondly, its melting point is much higher. The manufacture of stearine candles, however, presented some difficulties; these were overcome by a process which was kept a secret at Paris (where the candles were first made); and which was afterwards sold to a London manufacturer, who immediately introduced it into this country.

A man, moreover, who knew the secret, went about to the candle-makers in London, and sold them not only the mode, but the actual material by which stearine could be converted into marketable candles. This material was soon ascertained to be white arsenic; and the chandlers having now emancipated themselves from the tax which they had been obliged to pay for the purchase of the secret, the manufacture of arsenical candles soon became almost general. The stearine candles were sold cheaper than any others, except those of common tallow, and became so popular, that some of the most respectable manufacturers of wax and spermaceti candles were obliged to make the new stearine ones, in order to retain their customers, and indemnify themselves for the great diminution in the sale of candles of the superior kinds.

* See page 585 of our present No.

The manufacturers admitted that the candles in question contained arsenic, but the Committee resolved to ascertain this fact by chemical analysis, and also to determine the quantity contained in each candle; as this was of importance with reference to the probable effects of such candles on the human constitution. The chemical experiments were conducted by Mr. Everitt, Mr. Golding Bird, and Mr. Richard Phillips; the first of whom repeated some of the experiments at one of the meetings of the society. The water in which the suspected stearine had been boiled was tested with re-agents, which showed the presence of the white oxide of arsenic; and metallic arsenic was reduced from the precipitate obtained by sulphuretted hydrogen gas.

Mr. Everitt satisfied the committee that the quantity of arsenic varied in different samples, from ten to eighteen grains in the pound of four candles; and that the largest proportion, namely, four grains and a half in each candle, was found in the cheapest sample.

It was also found that the arsenic was only mechanically mixed with the stearine, and not dissolved in it; and hence a larger quantity was contained in the top of some of the candles than at the bottom, because in moulding the candles are reversed. Such a candle, therefore, when first lighted gives out a greater quantity of arsenical vapour, than when it is nearly burnt out. When glass vessels were placed over the candles, arsenious acid was deposited on their inner surface. Mr. Bird carefully examined the products of the combustion of arsenicated animal fats, and found "that according as the combustion is impeded or free, that is, according as more or less oxygen has access to the flame, metallic arsenic, or the so-called black oxide of arsenic (?)—or arsenious acid, is given out and deposited under their respective character-

istic forms. According to Mr. Golding Bird's experiments, there might be a point of such low combustion in the burning of arsenicated fats, as to give rise to that most deleterious and fatal gas, called arsenuretted hydrogen gas."

In the course of their experiments, the Committee received candles for examination from various sources, some of which were found to be arsenical, and others not. In wax, spermaceti, and composition candles, the poison was never detected.* Experiments were also tried on birds, with arsenicated and with ordinary candles. Several linnets and greenfinches were shut up in wooden chambers which were well ventilated, and contained food and water. In two of these chambers arsenical candles were kept burning, and the birds died; in another spermaceti candles were burning, and the birds remained perfectly healthy. The same experiment was tried with rabbits and guinea-pigs, and although the arsenical vapours did not kill them, they evinced signs of distress, particularly the rabbit. It became dull, "was constantly lying on its side, its flanks drawn in, and its breathing quick, accompanied with a tremulous motion." The rabbit in whose chamber spermaceti candles only were burned, did not exhibit these symptoms.

For the last thirty-six hours of the experiment, an earthen dish was fixed over one of the arsenical candles, at the height of two feet and a half, while shallow basins holding distilled water were placed on the floor near each

* Our readers must not suppose that stearine candles are always sold under that name. They are sometimes called German wax—imperial wax candles—French candles—pressed tallow—tropical candles—moulded wax—and Venetian wax.

We observe that one advertiser asserts that he makes them with what he calls "that harmless metal, bismuth."—Query, is it so?

They may also be made with one-thirtieth part of wax, which must be safer, we should imagine, than the most "harmless metal."

candle; and Mr. Everitt discovered ample traces of arsenic, both on the dish and in the water. Hence it appears, that when arsenical candles are burning, the vapours fall as well as rise.

The Committee then proceed to mention some recorded facts, showing the deleterious effects of arsenical vapours. There are two cases in which arsenuretted hydrogen gas has been fatal. One is narrated in the *Annales de Chimie* (vol. xcv. p. 110) in an extract of a letter from M. Ruhland to M. de Guyton Morveau.

Mr. Phillips told the Committee of a similar case which occurred in 1836, at Falmouth. Mr. Beard, who was delivering a lecture on the gases, at the Mechanics' Institute, inhaled a portion of arsenuretted hydrogen gas, and died some days afterwards, in spite of medical assistance*.

The second form of arsenical vapours is that in which they contain the black oxide; and in France, where the black oxide is employed under the name of *poudre à mouches*, to destroy flies, it is found to be fatal to them, if they but approach the atmosphere around the vessel containing its solution.

The third form of the vapours is that in which they consist of arsenious acid, and the pernicious influence of this gas is well known. The reporter cites several instances to this effect. Our readers will find two of them, and another case besides, in Dr. Christison's work on Poisons (p. 236.) In the mining districts of Cornwall, too, arsenical vapours are found to be injurious to animal as well as vegetable life.

To many it will seem a work of supererogation to prove the unwhole-

someness of arsenical candles; but such objectors are fully answered in the report before us.

The unwholesomeness of the stearine candles was, of course, quite clear from the moment that it was known that they contained arsenic; yet the perpetrators of these candles will, no doubt, complain of the Committee for shedding "intolerable day" upon the subject, and thus diminishing their gainful craft. Yet, what with the *auri sacra fames* on the one hand, and the desire to make a show at small cost on the other, we fear that even this unsparing exposure will fail of its due effect with some.

Whether it would be desirable to have a Board of Health in this country to which such points might be referred, is too weighty a question to be mooted at the end of an article; perhaps we may take some other opportunity of discussing it. Meantime the thanks of the community are due to the Society for the pains which they have taken in investigating a subject of such importance to the public welfare.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abrégé."—D'ALEMBERT.

The London Dispensatory. By ANTHONY TODD THOMSON, M.D., F.L.S. G.S. Professor of Materia Medica, Therapeutics, and Medical Jurisprudence, in University College, London, &c. London, 1837. 8vo. pp. 1164.

DR. A. T. THOMSON'S position, as professor of materia medica in one of our largest schools, as well as the popularity of his Dispensatory, are two manifest reasons why the accuracy of his works should be watched over with the most scrupulous care. Unfortunately, this care is not bestowed; and as on a former occasion it was our duty to point out some of the numerous errors in his *Elements of Therapeutics*, so it is now our disagreeable office to show how

* The account of this case, which is said in the report to be taken from a Cornish paper, states, that the lecture on the gases was delivered December 5th, 1836; that the experiments in illustration of that lecture were performed on the 19th, when the fatal inhalation took place; and that Mr. Beard "died on the 29th, that is, twenty-four days after the accident." It would seem rather to have been ten days after the accident.

frequently the author has slumbered in revising the ninth edition of his Dispensatory.

Before we enter upon the long catalogue of faults, let us preface our censures by declaring that we are by no means blind to the merits of the book. These merits consist in a generally good selection of matter, a happy amalgamation of facts drawn from a hundred sources, and a fluent, natural style. But as our author's faults must not make us unjust towards his brighter qualities, so these latter will hardly prove a sufficient answer to the numerous counts of our indictment, in which we accuse him of being one of the most careless of all writers; and that upon a subject in which accuracy is imperiously required. Let us proceed to substantiate what we have said.

Taking the articles in the order in which we have made our notes, we find that under *Ammonia liquor fortior*, our author says it "is chiefly employed by the retail druggists, for preparing the liquor ammoniac of a strength equal to that of the Pharmacopœia; for which purpose, one fluid ounce of it is mixed with three fluid ounces of distilled water." (p. 188.) Instead of *three*, he should have written *two*. The error unfortunately exists in both the original editions of the Pharmacopœia: Mr. Phillips, however, has given it in his table of errata.

Under the head of *Ipecacuanha*, he has omitted, among its official preparations, not fewer than three; namely, the Pulvis Ipecacuanhe et Opii of the Edinburgh Pharmacopœia, as well as the Pil. Conii Comp. and the Pil. Ipecac. Comp. of the London Pharmacopœia.

He says of *Digitalis*, that "by a proper exhibition of it, the frequency of the pulse may be diminished any number of pulsations, and regulated at the pleasure of the practitioner," (p. 380); an assertion in which we fear few physicians will agree with him. A few lines before, an instance occurs of almost miraculous carelessness, in connexion with the same drug. Our author had formerly said, "*Digitalis* is directly sedative," (4th edit. p. 321); and on the word *sedative* was the foot-note, "this term implies, as I understand it, any substance which diminishes the action of the heart and arteries, *without first increasing it*." The text has now, however, been altered to, "*Digitalis* is

directly stimulant, indirectly sedative," (p. 380); but the foot-note remains the same as before.

We fear that it is not yet known what doses of *digitalis* are effectual, without being poisonous, and that consequently it still remains the most dangerous of remedies. From two papers by M. Joret, in the Archives Générales for January and March, 1831, it would seem to be a common thing, in the hospitals of Paris, to give a patient ten or fifteen grains of powdered foxglove in a day. One of the physicians to the Hôtel-Dieu gave much larger doses to three patients under his care, and in two of them the treatment was followed by sudden death. In one case the patient drank an infusion made with four drachms of the dried leaves; and though this caused frequent vomiting, he was made, on a subsequent day, to take an infusion made with five drachms. He vomited again, but less than before, and the pulse sank from 120 to 45. The potion was discontinued, but two days afterwards he dropped down dead.

On the other hand, some very interesting remarks on *digitalis* were published, some years ago, by Dr. John Davy, in which he showed that it might be taken by persons of broken-down constitution, in much larger doses than was generally supposed. He gave it to soldiers who returned from tropical climates with diseased livers and dropsy, to the extent of 3ss., ʒj., and even grs. 105, of the pulv. *digitalis*, in 24 hours. To guard against being misled, he sent to Apothecaries' Hall for a fresh supply, but still found the same doses borne. He then gave the same *digitalis* to other patients not placed under similar circumstances, and found that it produced its usual effects in the usual doses. Of these cases Dr. Thomson appears to be ignorant, for no mention is made of them; and coming from such an authority as Dr. Davy, they are too important to be passed over in silence.

He says, that the strength of the infusion of *digitalis* is just one-half of that in the last Pharmacopœia (p. 897); but it is considerably less; for in the former one a drachm of the leaves was ordered to eight ounces and a half of fluid; and in the present one, twenty-one ounces of fluid are used with the same quantity of leaves as before.

At page 705, he ascribes the term *Vinum album Hispanicum* to the London College; the term they really use is *Vinum Xerium*.

At page 738, he says, that one fluid drachm of diluted hydrochloric acid contains about 32 grains of real acid; yet the diluted acid is but one-fourth of the strength of the strong acid, and the latter has a specific gravity of only 1.160!

At page 776, he says, in the note 2, "The title of this section in the London Pharmacopœia is, *Alkalies and their Salts*;" not so; the title *was*, but *is* no longer.

The London College order the Cataplasma Conii to be made with the extract; our author with *dried hemlock*, (p. 840)

The London College order the infusion of cinchona to be macerated for *six* hours; our author translates it *two*, (p. 895)

In the Inf. Rosæ Comp. the time of maceration is *six hours* according to the College, but *half an hour* in our author's translation, (p. 901.)

In the Inf. Scoparii, the time of maceration is *four* hours according to the College, and *two* hours in our author's translation, (p. 902;) and in the Inf. Valerianæ, *half an hour* according to the College, and *two hours* in Dr. Thomson's translation, (p. 904.)

He directs the liniment of opium to be made with *ten* fluid ounces of tincture of opium to six of liniment of soap (p. 908;) the quantity in the London Pharmacopœia is only *two* ounces of laudanum to six of the soap liniment.

The Linimentum Terebinthinæ of the London College is composed as follows:—

"R Saponis mollis uncias duas,
Camphoræ unciam,
Terebinthinæ Olei fluiduncias sedecim. Agita simul, donec miscantur."—8vo. edit. p. 168.

Dr. Thomson's translation (?) is as follows:—

"Take of cerate of resin, a pound; oil of turpentine, half a pint. Melt the cerate; then add to it the oil of turpentine, and mix them," (p. 909.) This is, as our readers may guess, the formula of the former Pharmacopœia retained by the most careless of men.

In the Liquor Aluminis Comp. our author has diminished the quantity both

of the alum and of the zinc, by one half.

The London College direct the Spirit of Rosemary to be made with two drachms* of oil of rosemary, a gallon of rectified spirit, and a pint of water: our translator orders two pounds of rosemary, a gallon of rectified spirit, and a gallon of water (p. 1044;) he afterwards says, "oil of rosemary is sufficiently volatile to rise in distillation with rectified spirit, which the Edinburgh College has, therefore, ordered to be used," just as if the London College did not order it likewise; and he then gives as the official preparations of Spir. Rosmarini in the London Pharmacopœia, the Linimentum Saponis Comp., and the Spiritus Lavandulæ Comp.; though the former is now called Linim. Saponis, and the latter Tinctura Lavandulæ Comp. These faults in nomenclature, indeed, are, singly, trifling; but when they occur frequently, they greatly diminish the value of a work of this kind.

In the Tinctura Camphoræ Comp. the London College have restored the oil of aniseed: Dr. Thomson omits it (p. 1062.)

In the formula for the tincture of black hellebore, the London College say—

R Hellebori contusi uncias quinque, &c.

Dr. Thomson, however, first mis-translates it *sliced*, instead of *bruised*; and then says, "the smallness of the fibres of the root of black hellebore renders it almost impossible to follow the direction of the London formula," &c. (p. 1070), whereas there is no difficulty whatever in following the directions actually given, but only in complying with the provisions of his false translation.

The *Mellita* and *Troches* are wholly omitted in the body of the work in their proper place, and appear in a supplement at the end of the volume, between the appendix and the index.

Our author continues to say of the Unguentum Piperis nigri of the Dublin Pharmacopœia, "We are ignorant of the purpose for which this irritating ointment is designed," (P. 1096.) Yet its use is mentioned in a book which Dr. Thomson *ought to have read*, as he *has edited it*; we mean Bateman on Diseases of the Skin. When speaking of Porrigo scutulata, Bateman says,

* In the octavo edition, *decem* has been misprinted for *duas*.

"There is an Unguentum Piperis nigri in the Dublin Pharmacopœia, of the efficacy of which Dr. Tuomy speaks highly. See his Essay on the Diseases of Dublin.* Moreover, Drs. Barker and Montgomery say of this preparation:—"This ointment was formerly in vogue as a cure for tinea capitis; and the powder of black pepper still remains an ingredient in the French *onguent pour la gale*†."

At page 1100 *et seq.* Dr. Thomson gives a "table shewing the proportion in which opium and certain preparations of iron, antimony, arsenic, iodine, and mercury, are contained in some compound medicines;" but it unfortunately contains several errors. *Pilula Saponis cum Opio*, Lond., stand instead of *Pilule Saponis Comp.*; *Pilula Syracis Compositæ*, Lond. are omitted; and the *Pulvis Cornu insti cum Opio* remains here, and also in the Index, though no longer in the Pharmacopœia. The *Liquor Potassæ Arsenitis* is stated to contain four grains of arsenic of potassa in the fluid ounce, though, in fact, each ounce contains this quantity of arsenious acid in combination with potass. But the most important error is in the statement that two fluid ounces of the *Liquor Hydrargyri Bichloridi* contain half a grain of bichloride of mercury: the fact is, that *one* fluid ounce contains this quantity.

In the posological table (p. 1143), the dose of aconitina is said to be from gr. $\frac{1}{4}$ to gr. ss.; at p. 780 the only dose mentioned is 1-16th of a grain.

The dose of tartar emetic is stated to be from gr. j. to 3ss. (p. 1143); any dose, however, beyond three grains, is hardly known in this country, the fl-lowers of Rasori in England being few and far between‡.

The dose of arsenic is said in the table (p. 1143) to be from gr. $\frac{1}{3}$ to gr. j. Surely this last is a monstrous and unheard-of quantity. In the account of the arsenical solution at page 529, the dose is

stated to be miv., gradually increased to mxxx. twice a day, while in the table (p. 1145) it is stated to be from mv. to f. 3j. Now mxxx. are only equal to a quarter of a grain of solid arsenic, and f. 3j. of course to half a grain. Thus the maximum dose at page 1143 is made double what it is at page 1145; while this, again, is just double the maximum dose given at page 929; and thus the largest quantity of arsenic which may be given is stated to be four times greater at page 1143 than it is at page 929.

The dose of the *Pil. Cambogiæ* [Comp.] is stated, at page 1145, to be from two to three grains; at page 1024 it was said to be from ten to twenty grains; *utri creditis Quirites?*

The dose of iodine is asserted to be from gr. $\frac{1}{4}$ to gr. iv. (p. 1144). We rather agree with those who think that the benefits produced by iodine are due to the long continuance of very small doses, and that large ones are inexpedient as well as unsafe. We would never give more than a grain in the course of twenty-four hours.

The dose of the *Spiritus Ammoniacæ succinatus* is said to be from mxx. to f. 5j. This medicine is now called *Tinctura Ammoniacæ Composita*; and as it has not only changed its name*, but trebled its strength, being now made with the *Liquor Ammoniacæ fortior* instead of *Liquor Ammoniacæ*, mxx. are a full dose, and f. 5j. an unbearable one. In the body of the work, the dose of the *Tinctura Ammoniacæ Comp.* is stated to be from mv. to mxx. (p. 1060). So that here, also, the quantities mentioned in different parts of the work as the proper doses vary exceedingly.

Strychnia is directed to be given in doses varying from $\frac{1}{3}$ of a grain to a grain (p. 1149). We more than doubt the propriety of ever giving grain, or even half-grain doses. In a very instructive case narrated in the second volume of the Transactions of the Provincial Medical and Surgical Association, the patient, after having taken half a grain of strychnia twice a day for five days without any sensible effect, was ordered to take a grain twice a day. He soon became apparently accustomed to this quantity, and was then ordered

* Bateman's Practical Synopsis of Cutaneous Diseases. Sixth edition, p. 173, note.

† Observations Chemical and Practical on the Dublin Pharmacopœia, &c. Dublin, 1839; p. 37c.

‡ At page 919 Dr. Thomson says, "Rasori gives at first grs. xij. during the day, and the same quantity at night, dissolved in a pint of barley-water, to be taken in divided doses, and carries it to doses of grs. ii. and a half." The same error occurs in the fourth edition, page 780, excepting that "ij. grs." stands instead of "grs. ij." Perhaps grs. stands for *gros*, the account having been taken from the Archives Générales.

* The old name is retained in Dr. Thomson's Index, which refers us to page 1041, where, however, it does not occur either under its old or new name.

to take a grain and a half night and morning; but the first dose produced fatal opisthotonos, the patient dying in less than four hours after taking the medicine.

The dose of *Uva Ursi* is stated to be from gr. x. to ʒj. , the last being, of course, a misprint for ʒj.

Lastly, the dose of *veratria* is said to be from gr. x to gr. ss! (p. 1146). The gr. x. is, no doubt, a misprint for gr. $\frac{1}{10}$; but even these quantities are too great; in proof of which we appeal from Dr. Thomson in his posological table, to Dr. Thomson in the body of the work, where he says, "If it be internally employed, the dose should not exceed one-twelfth of a grain; and the action of even this minute dose should be watched." (p. 821.)

Again, the dose of *vinum veratri* is said to be from m℥ to grs. xx; grs. being, we presume, a misprint for m℥ —but the constant repetition of such errors, in regard to the most powerful agents, evinces, to say the least, a most unpardonable degree of carelessness.

Among other tables is one setting forth the new names, which are placed opposite those of the former edition of the Pharmacopœia. Now as a list of this kind is given in the original work, published by the College, we should not have thought it very difficult for any one to avoid important errors. Alas! not so: we have observed not fewer than nine or ten in our author's list of "former names," which were not in the last edition of the Pharmacopœia; and what is more absurd, the names into which they are said to be changed are not always in the new one! We are told that *Aqua Calcis Muriatis* is now changed into *Liquor Calcis chloridi*, and so, likewise, four other *Aque*, which did not exist, are said to have been changed into *liqueurs*. We also have a set of *Spirits* which did not exist till Dr. Thomson made his list, and which he tells us are changed into *Tinctures*. Among others we find *Spirit. Rhei Comp.* given by Dr. Thomson, as converted into *Tinctura Rhei*; whereas the only Tincture of Rhubarb in the present edition of the Pharmacopœia is the *T. R. Comp.* Thus, the thing said to be changed did not exist in the former edition of the Pharmacopœia, and that said to have been substituted for it is not to be found in the present!

The number of articles omitted in this same Posological Table is rather considerable. Among the decoctions, for instance, we miss those of pomegranate, and *uva ursi*. Among the infusions, those of *diosma*, (*buchu*) *krameria* (*rhatany*), *hop*, and *parcira*; among the tinctures, those of *colchicum*, *eubebis*, *galls*, and the compound tincture of iodine.

Again, of the articles of the *Materia Medica*, Dr. Thomson has altogether omitted two—viz. *Brominium* and *Ergota*; while some which occur in the body of the work are excluded from the Index.

As to the misprints and orthographical errors with which the work before us teems, we hardly know how to manage them; to give six or eight is nothing, and to give all, we should require to have a supplemental sheet entirely devoted to them. We must, therefore, steer a middle course, and content ourselves with a dozen or so, by way of specimens.

Among the foreign terms for arsenic acid, we find (at p. 215) *Naturlicoer Arsenickhalk* given as the German one; the first word is meant for *natürlicher*; but what *halk* signifies, at the tail of the second, we know not.

The German words, or rather what are intended as such, are certainly extremely curious: we had made a list of *five and twenty* of the most remarkable, but omit them in mercy to our readers.

The Italian term for the same substance is said to be *Arsenico iuxneo*. There's a portent for you—a word to crack an Italian jaw—*IUXNEO*. It is not a little curious that these imaginary German and Italian words are precisely the same as in the 4th edition, p. 186; so that they seem to have been copied from edition to edition, with a constancy worthy of a better cause.

At p. 202, among the synonymes of sulphuret of antimony, we have, as the French one, *L'antimoine sulfure*, instead of *Sulphure d'antimoine*; and at p. 272, the French for cloves is said to be *Clousele Girofles* (it is just the same in the fourth edition, p. 327, excepting that there is no accent on the first word;) the real term being *Clous de girofle*.

Then we find, *ipecacuanne* for *ipecacuanha*, at p. 284; *leveure* for *levain* (yeast,) at p. 293; *Beaume* for *Baume*,

at p. 345; *semen* for *semence*, at p. 367; *digitalis* for *digitale*, at p. 378; and so on, without end.

The Greek and Latin words appear, we think, in rather better trim than our author generally allows them to do; yet even here the errors are far from being few: thus we have *Βωμός* for *Βράμος*—*χρεας* for *κρεας*, &c. &c.; and again, *sextus* for *sextis*; *ulceres* for *ulcera*; *spergatur* for *spar-gatur*; *caloric* latinized by *caloricus*; and *utitur* employed to signify *used*, &c. &c. &c.

As we would wish to part with Dr. Thomson on friendly terms, we will remind him, that had we not thought his Dispensatory a work of merit, and one which will be consulted as an authority both by the practitioner and student, we should not have taken so much pains to point out its errors; errors, many of which he must himself acknowledge are fraught with the utmost danger, and all of which might have been avoided by a very moderate share of attention.

WESTMINSTER MEDICAL SOCIETY.

EXTRACTS FROM THE REPORT ON ARSENICATED CANDLES.

ALTHOUGH stearine was prepared in London in considerable quantity, candles manufactured with it were not, at first, so numerous as they have become since; owing to the secret process employed, to render them fit for sale. Your Committee have been informed, by a very intelligent manufacturer of candles, who made use of, but has since given up, the process in question, that an individual, in possession of the secret, went about to the candle-makers in London, to sell, for a consideration, not only the mode, but the very material, with which stearine was to be made fit to be converted into showy and attractive candles for the market.

The material in question was very soon ascertained by some of the candle-makers to be powdered white arsenic; and thus having emancipated themselves from the tax which they formerly paid for what had now ceased to be a secret process, those persons were enabled to extend the field of their operations, whereby the practice of manufacturing stearine candles with arsenic became very soon almost

general. Nay, such was the effect produced on the market, by the appearance of this novel and extensive branch of trade (at a price lower than that of any other candle except those of common tallow), that some of the most respectable manufacturers of wax and spermaceti candles were compelled to resort to the making of the new stearine candles (according to the then well-known process, which, your Committee have learned, consisted in putting one pound of white arsenic in every hundred weight of stearine), in order to retain their customers, and, in some measure, indemnify themselves for the losses sustained in consequence of the great diminution that had necessarily followed in the sale of every superior sort of candles.

As is generally the case in all matters of this kind, notwithstanding the notoriety of the practice among the trade, the public, whose welfare was likely to be affected by it, remained in ignorance of the fact, that in using the new stearine candles they were burning arsenicated candles; until Mr. Everitt mentioned the subject in a lecture delivered in June last, before the Medico-Botanical Society—and again until Dr. Scott, in October, as stated in the introduction to this Report, brought it to the notice of the Westminster Medical Society, and led them to the present investigation.

The admission made by the parties themselves who employed it, that arsenic was contained in the candles in question, might have been deemed sufficient for the purpose of the present investigation: but your Committee could not rest satisfied without verifying the fact by chemical analysis, and still less without ascertaining the quantity present in each candle: as that point was held to be of great importance in determining the probable injurious effects of such candles on the human constitution.

Accordingly, a great many specimens of the candles in question were procured from several shops, under various denominations, and were submitted to accurate analysis. This was confided principally to Mr. Everitt, who repeated his experiments before several of the members of the committee, and whose results were afterwards corroborated by some fresh experiments made by Mr. Golding Bird, supported by the testimony of Mr. Richard Phillips.

The Society, through the kindness of the first of these three chemists, had an opportunity of witnessing, at one of their ordinary meetings, the repetition of some of the experiments in question, which consisted not only in testing the water (with which the suspected stearine had been boiled for some time) by proper re-agents

denoting the presence of the white oxide of arsenic, but also in reproducing the metallic arsenic from the precipitate that had been obtained in the liquid by means of sulphuretted hydrogen gas.

Through the various experiments which he made, and often repeated, Mr. Everitt satisfied your Committee that the quantity of white oxide of arsenic, or arsenious acid, contained in the candles submitted to analysis, varied, in different samples, from ten to eighteen grains in the pound of four candles, and that the largest proportion of it—namely, four grains and a half in each candle—was found in the specimen which bore the lowest price of sale.

By another set of very ingenious experiments, conducted with the greatest precision, it was ascertained that this quantity of arsenious acid is only mechanically mixed with the stearine, and not dissolved in it (the saponified stearine appearing to be scarcely capable of holding any portion of it in solution:) and it is worthy of remark, that a larger quantity was found at the top of some of the candles, which in the act of moulding forms the lower end in the mould, than at the bottom. The difference between the two ends amounted to nearly one-third of the whole; so that when such a candle is first lighted it must emit a larger quantity of arsenious acid, than when it is nearly burnt out. These several quantities of the poisonous substance are given out during the combustion of the candle, in the form of subtle vapours of arsenious acid, a fact which was proved by the deposits obtained on the inner surface of glass vessels placed over the lighted candle, and which deposits were carefully examined.

But in order to leave no vestige of doubt on this point, Mr. Everitt contrived a little apparatus, by means of which the vapours emitted by a suspected candle in a state of ignition, were obtained, partly in a solid form, adhering to the inside of the body of a retort, and partly dissolved in the condensed steam deposited in the horizontal tube of the same retort, which was kept, for that purpose, in a constant state of refrigeration. Under both those forms arsenious acid was detected.

It is to the interesting question of what are the productions of the combustion of arsenicated animal fats, that one of the members of your Committee, Mr. Golding Bird, chiefly directed his attention. He first experimented on arseniferous gases, and afterwards instituted some direct trials with a mass of fat, in which arsenious acid was mixed, and which by means of a wick was set on fire. In watching the operation in both cases, Mr. Bird convinced himself of the fact, that according as the combustion is impeded or free—

that is, according as more or less oxygen has access to the flame—metallic arsenic—or the so-called black oxide of arsenic (?)—or arsenious acid, is given out and deposited under their respective characteristic forms. According to Mr. Golding Bird's experiments, there might be a point of such low combustion in the burning of arsenicated fats, as to give rise to that most deleterious and fatal gas called arsenuretted hydrogen gas.

In the course of their analytical inquiries, your Committee received specimens of candles for examination from clubs, institutions, and private families, some of which were found to be arsenical, while others were not so. And in order not to leave any point undetermined, the analysis, in some instances, was extended to wax, spermaceti, and the old-fashioned composition candles, in none of which the noxious material was detected.

It would be superfluous to specify more minutely or technically to the Society, the several operations gone through by your Committee, with a view to settle the chemical question of the presence of arsenic in the above candles. It is sufficient to state, that the fact of its presence in such candles was established beyond all possible doubt, and that the quantity contained is considerable.

Your Committee next directed their attention to the best mode of ascertaining, as far as such an investigation can admit of demonstration, the probable effect which the respiring of the ascertained quantity of arsenical vapour might have on animal life; and after some consideration, it was determined to expose various living animals to an atmosphere in which arsenicated, or stearine candles, were burning, at the same time that an equal number of the same species of animals, and, as nearly as possible, of the same age and strength as the first set, were placed in an atmosphere of similar dimensions, wherein spermaceti candles only were used.

The Committee being anxious to convince the Society that every measure of precaution necessary to ensure accurate results in the physiological experiments about to be undertaken was adopted, have directed the apparatus employed on the occasion to be brought into the Society's apartment, at this meeting, in order that it may be examined precisely under the circumstances in which it was used, when the experiments were performing.

The apparatus represents two sets of two chambers each, made of deal boards, the one set measuring two feet by three and three feet deep, and the second two feet by two and three feet deep. Their interior is ventilated by contrivances simi-

lar to those which are recommended for large assembly rooms, namely, by several openings at the bottom and top; so arranged, that the whole, or only part of them; may be used. The top, or roof, of the chambers takes off to admit of the ready introduction and removal of the animals; and there is a glass-door in front, through which all that passes within can easily be observed. These wooden chambers stood at the height of two and a-half feet from the ground, supported upon wooden uprights, and they were fixed within a spacious and lofty apartment, well lighted and well ventilated. The two largest chambers were marked A and B, the smaller C and D.

Into the letter A two strong and lively linnets (*Fringilla Linaria*) were introduced, confined in a large cage together, raised on a stand, and placed in the centre, with two guinea-pigs and one rabbit in perfect health. In letter B a similar cage was placed with two guinea-pigs and one rabbit. Four arsenicated candles, one in each angle of the chamber, were lighted in A; and an equal number of spermaceti in B. In C two green finches, (*Fringilla Chloris*), and in D two other linnets, strong and lively, (particularly the birds in C,) were confined within a cage properly suspended, with three arsenicated candles lighted in the first chamber, and three spermaceti candles in the second.

The experiments in A and B began at two P. M. Monday, the 27th November, and those in C and D at half-after three P. M. Tuesday, the 28th November; and all of them were continued, from day to day, until Saturday evening, the 2nd December, beginning each day about ten o'clock A. M. and terminating at the same hour in the evening, when the cages were taken out, and all of them suspended in an ante-room, cleaned the next morning, and a fresh supply of food and water for the day administered, before they were again replaced in the experimental boxes.

A thermometer was suspended in each of these boxes, so as to be easily seen by the observer, and a strict watch kept by some member or other of the Committee, but especially by Dr. Scott, at whose house the experiments were made, respecting what was going on within the boxes in reference to the state of the animals, their movements, and power of feeding, the temperature and ventilation of the chambers, and the manner of burning of the candles. All these remarks were instantly committed to writing as soon as made; and your Committee beg to lay before you this curious register, which extends over a week's time, during which observations almost hourly were made for seventy-two hours. In the course of that

period several pounds of arsenicated candles were burnt in A and C. As a general statement it may be remarked that, with the exception of the first day, when it varied from eighty to ninety degrees, the temperature of all the boxes was kept, more or less, at the standard of summer heat, as the most congenial to the animals submitted to the experiment,—that ventilation was maintained as perfect as it could be,—and that in neither food appropriate to each, nor in drink, were the animals stinted in the course of the experiments.

In reporting the result of their observations made on the animals during those experiments, your Committee intend strictly and rigidly to adhere to a statement of the facts and phenomena observed, without venturing an opinion as to any relation which such facts for phenomena might bear to the vapours of arsenic, as a cause. Having once proved the presence of the poison in candles, and its volatilisation during combustion; and having faithfully reported in the register laid before the Society, what several of the members had an opportunity of repeatedly witnessing in respect to the progressive condition of the animals experimented upon, your Committee leave it to the Society, and to those who may peruse the present Report, to draw their own conclusions.

After exposure to the candles containing arsenic, for the space of three or four hours, one of the birds in A became visibly affected, but recovered in the night, on the experiment ceasing. At the termination of the first hour (on the resumption of the experiment on the following day, Tuesday), the same bird became again affected, and in an hour more it died. Its death was followed by that of the second bird half an hour later. These two birds had been in an arsenicated atmosphere for seven hours and a half altogether.

Three more linnets were immediately put into the cage of chamber A, with two arsenicated candles instead of four. In about four hours they became dull and stupefied on their perch, although, at first, they appeared particularly the reverse; they seemed much inconvenienced for the rest of that day. On Wednesday three stearine candles with arsenic were lighted, and the three birds who had recovered in the course of the preceding night, were not long in exhibiting symptoms of uneasiness. They drooped their wings, breathed laboriously, and kept their beaks constantly open; they continued so through that day. On the following day two of them became much more distressed three hours after exposure to the candles; an hour later one of them fell, as if from vertigo, from its perch, and in half an hour

it died. The day after witnessed the death of its two remaining companions, although when replaced in their chamber A, at ten o'clock that morning, they appeared to have recovered their usual state of health.

On that day, at twelve o'clock, one of these latter birds had been seen to gasp for life, unable to remain on the perch, and the other became equally affected by one o'clock P.M.

The bodies of five of these animals were confided for chemical examination to Mr. G. Bird, who reported, that distinct, though minute, traces of arsenic were found in them, under circumstances which led him to believe that the poison had been either inhaled or swallowed.

Your Committee have only to add, in respect to these experiments, that the two linnets, which had only been exposed to the burning of spermaceti candles, under equal circumstances of temperature, ventilation, space, and food, never exhibited the smallest deviation from health, and are now alive and well.

Of the larger animals, your Committee has only to report, according to the daily entries into the register, that those in the arsenicated chamber A evinced signs of distress from the second day; the rabbit in particular, which became dull from that time, was constantly lying on its side, its flanks drawn in, and its breathing quick, accompanied with a tremulous motion. These symptoms, which were not noticed in the rabbit of chamber B, kept increasing towards the end of the week, when the experiments were put an end to; at which time the eyes of the animal had become dull, the ears were drooping, yawning occurred frequently, and the guinea-pigs as well as their companion refused corn. They would take only green food, of which, however, they partook in diminished quantities; while they accepted eagerly of water twice in one day. The same species of animals, on the contrary, confined in chamber B, invariably refused the water; they seemed as lively and playful at the end of the week as when they were first put in; nor did they appear to have lost flesh like those confined in chamber A.

It had been arranged that, with a view to obtain some information as to what became of the arsenical vapours, when once they were dispersed through the chamber, an earthen dish should be fixed over one of the candles at the height of two feet and a half; and, also, that shallow basins, holding distilled water, should be placed on the floor near each candle. This arrangement, however, was not made until the third day of the experiments, and was therefore in force only for thirty-six hours altogether. Notwithstanding the short-

ness of the period, Mr. Everitt discovered both on the surface of the dish in question and in the distilled water, ample traces of arsenic; shewing, that when arsenicated candles are burning, the poisonous particles may fly upwards, or fall on the objects in the apartments near the candles.

CASE OF STRANGULATED HERNIA,

IN WHICH THE SYMPTOM OF VOMITING
WAS ABSENT BEFORE THE OPERATION
WAS PERFORMED—OPERATION—DEATH.

To the Editor of the Medical Gazette.

SIR,

If you think that the following case is of sufficient interest to hold a place in your journal, I shall feel obliged to you by its insertion.—I remain, sir,

Your obedient servant,

THOMAS MORTON.

University College,
Dec. 17, 1837.

Thomas Knight, admitted into the North London Hospital, at half-past two, P.M., January 7, 1837. The history of the case is as follows:—

He has been subject to inguinal hernia for twenty years, which was first induced after violent exertion. Since then the protrusion has much increased in size. For thirteen years after its appearance he wore no truss. Seven years ago the hernia descended more voluminously than before, and he was obliged to send for a surgeon to reduce it, as he could not effect its return himself. The hernia has frequently descended since then, but he has been always able to reduce it himself with ease.

Yesterday night, at twelve o'clock, he retired to bed as usual, laying aside his truss; soon after he was attacked with vomiting, during which the hernia descended. The tumor not yielding to his endeavours to reduce it, he was quickly seized with severe pain in his belly, which compelled him to send for professional assistance.

A surgeon attended, who, after trying the taxis for some time, without avail, recommended the man to come to the hospital, which he did. When admitted, there was a large scrotal hernia on the right side, which is tense and painful when it is squeezed. Colicky pains in the belly; nausea and retching; but *he has not vomited since the descent of the hernia*. The bowels have not been relieved since yesterday morning. The pulse is moderate; countenance is expressive of suffering. The taxis, cuemata, hot bath, and two

grains of tartar emetic, were employed, but without any good effect. Soon after the administration of the tartar emetic the muscular system was much relaxed, but he did not vomit. The operation was now recommended and performed. When the sac was opened a quantity of the small intestine was found in it, of a dark colour, but firm and glistening in its appearance. When the stricture, which existed in the neck of the sac, had been divided, the bowel was readily returned. Some omentum that was adherent to the sac was allowed to remain undisturbed. Not long after the operation had been finished, the man vomited for the first time. He had now several copious evacuations downwards from the bowels.

Jan. 8, 5 P.M.—Twenty hours after the operation, he appeared to be in a dangerous state. Has vomited several times during the last two hours. He died at two o'clock A.M., 9th January, 41 hours from the commencement of his seizure, and 29 hours after the performance of the operation.

Examination of the body, twenty hours after death.—Some vascularity of the small intestines in the pelvic region. Folds of small intestine glued together by recently effused coagulable lymph. The portion of the small intestine that had been included within the constriction was recognized by the constricted appearance which it presented at two places. The bowel was more engorged between these points than elsewhere, though the rest of the bowel for some extent was darker coloured than natural. The omentum was more vascular than is usual; the portion that had been left in the hernial sac was free from all stricture. Under the peritoneal coat of the intestine, where the constricted appearance alluded to was manifested, a narrow circular band of light yellow-coloured substance was deposited, and which was regarded as concrete pus; that had been produced in consequence of the injurious pressure to which the muscular and mucous coat of the intestine had been subjected to.

TREATMENT OF FRACTURES BY MEANS OF PLASTER OF PARIS.

To the Editor of the Medical Gazette.

SIR,

I SHALL feel greatly obliged if you will do me the favour of inserting in your journal the following remarks on the treatment of fractured limbs, by incrusting them in plaster of Paris; which request I make be-

cause I believe that I may fairly claim to be the author of the plan, it having been proposed by me in the inclosed pamphlet between six and seven years ago; and besides which there has been for the same period in the Museum of the College of Surgeons, an apparatus invented by me for extending a fractured leg or forearm to its proper length, and for holding it so extended, and in a position neither everted nor inverted, during the deposition and hardening of the plaster of Paris around it.

Mr. Sweeting, whose communication on this subject appeared in the *GAZETTE* of last Saturday, mentions Messrs. Bond and Gale, of Glastonbury, as the parties to whom we are indebted for the plan; but I have been unable, after considerable inquiry, to ascertain the period at which these gentlemen published this plan of treatment, and should feel glad to learn whether it was before or after the date of my pamphlet, viz. September 1831.

In the *Medico-Chirurgical Review* for April 1834, the plan is spoken of as M. Dieffenbach's; but in the same journal for Oct. 1831, there is a review of my pamphlet on this subject, and the treatment is spoken of as my proposal, without any remark that it had ever before been employed or proposed by any other surgeon.

There was also a notice of my pamphlet in the *London Medical and Surgical Journal* for Oct. 1831, edited by Dr. Ryan, who condemned the plan most unequivocally, and concluded by saying:—"He has as yet tried it only on brutes, and we advise him to halt there." This journal likewise gave me credit for the originality of the plan, and for its presumed absurdity also; but Dr. Ryan, I think, must acknowledge, that he at least committed an error of judgment by condemning in so off-hand a manner a mode of treatment which experience has now shewn to be very successful.

The *London Medical and Physical Journal* for October, 1831, then edited by Mr. North, was the only other journal in which my pamphlet was noticed, so far as I am aware. The editor did me the favour to say, that where the patient is a restless child, or an irritable, or perhaps insane adult, Mr. Beaumont's plan would probably be more successful than any other yet devised. It was also stated in this journal, that though plaster of Paris might never have been employed in this country in the treatment of fractured limbs, yet that by the Arabians it had been so employed, and on inquiry, I found, in Dr. Dunean's *Medical Commentaries*, vol. ix., decade 2, for 1794, a detailed account of a case of severe compound fracture of the bones of the leg and foot so treated by the natives of Benderneck, on the Persian

Gulph. The essential difference between their mode of using plaster of Paris in compound fractures, and that described by me, consists chiefly in their using it only as a means of steadying the limb, whilst I proposed it, also, as a means of perfectly excluding the atmosphere from the injured surfaces of the bones and soft parts. The Arabians, also, employ no apparatus to extend the limb to its proper length, and to hold it so during the time of incrusting it in plaster of Paris, which is, of course, a very important point in this mode of putting up a fractured limb. The patient treated by the Arabs, it is stated, recovered, but with a leg considerably deformed.

May I beg you to do me the favour of looking over the inclosed pamphlet, and of giving your opinion as to the plan of treating fractured limbs therein proposed; and also as to whether I am, or am not, entitled to lay claim to the authorship of this mode of treatment.

I have the honour to be, sir,
Your obedient servant,
WM. BEAUMONT.

47, Berrers Street,
Dec. 19, 1837.

[We have looked over the pamphlet above alluded to. It appears to us fully to bear out the observations of Mr. Beaumont.—ED. GAZ.]

COX'S EDITION OF COOPER'S SURGERY.

To the Editor of the Medical Gazette.

SIR,
SHOULD the following seem to you a just cause of complaint, I should feel obliged by its insertion in your widely-circulated journal.

Some months ago a work was advertised, entitled, "Sir A. Cooper's Lectures on Surgery," published by Cox, of St. Thomas's-street. Now, neither in the advertisement nor in the title-page was there any thing to indicate that this was only the first volume of the work, whereas it is to consist of three; this appears to me to have been a complete trick. I have just purchased the second volume, which, in its title page, is marked as such, and in it are no less than ten superfluous explanations of the plates, so mixed up with the text of the work, that they must necessarily remain, while there are also proper explanations placed beside the plates. The following is a specimen:—

At page 62 is one headed, Explanation of Plate 6, containing a description of the five first figures in plate 3, and the six first in plate 4; also three other descrip-

tions of drawings of trusses, which I cannot find at all in the work.

Now, sir, is there no redress in such a case as this, where an expensive work is got up in such a careless and shameful manner? Surely the publisher should be compelled to give a new title-page with vol. i., and also to rectify the above ten mistakes, occupying twelve pages of the work, or to return their money to those purchasers who choose to demand it.

I am, sir,
Your obedient servant,
A STUDENT.

London, Jan. 3, 1838.

ON THE EMPLOYMENT OF GUNPOWDER AS A MEDICINE.

By ROBERT DICK, M.D., Glasgow,
*In a Letter to the Edinburgh Medical and
Surgical Journal;*

SIR,
I BEG to call, through your journal, the attention of the profession to the employment of gunpowder, as, in some cases, an useful addition to the already existing stock of medicaments.

I was induced, from theoretical considerations, lately to prescribe it, in certain cases, and had reason to be satisfied with the results.

The derangements for which it appears to me to be peculiarly adapted are morbid secretions of the gastro-mucous membrane, depending on a *sub*-inflammatory action, or accompanied by it.

In such cases, gunpowder, given in various doses, and with the occasional interposition of ordinary mild laxatives, has proved, in my hands, eminently serviceable.

Whether it may be appropriate in a greater variety of cases, or whether it may, if given in larger doses, and for a greater length of time, than I prescribed it, be found useful as a constitutional alterative, or as a cutaneous drug, I am unable to state. But I should not be surprised if it should be found so. I have administered from ten grains, indefinitely upwards, several times in the day.

I find the use of spirituous liquors, or of pungent condiments, &c. rather contraindicated during the employment of gunpowder. The gastralgic effects which I have found these produce, when used simultaneously with gunpowder, I ascribe to what I shall venture to designate the *detergent* effects of that substance on the mucous membrane, which, owing perhaps to its charcoal and nitre, it denudes of its attaching albumino-mucous secretion,

clearing, and seemingly attenuating that membrane, in some measure.

One advantage of this drug is, that it exists in commerce, ready for medical use.

The best form of administration of gunpowder is in the dry state. The liquid form does not suit.

No apprehensions need be entertained of the charcoal producing any unpleasant consequences. In *pica*, and in the chlorotic state, large quantities of this substance are eaten with impunity; and further, J. P. Frank [*De Curandis Hominum Morbis*] recommends it as an effectual remedy in flatulence.

The above notices are very general, as the remarks of an individual, who suggests a new drug, ought always to be.

I am, sir, yours, &c.

ROBERT DICK.

Kensington Place, Glasgow,
25th July, 1837.

In speaking of the therapeutic effects of gunpowder in various morbid states of the gastro-enteric mucous membrane and its secretions, it may be not unseasonable to add to the observations now made by Dr. Dick, the remark, that charcoal in its separate and pure state has been long used by various practitioners with similar intentions, and to fulfil similar indications in the treatment of ague with gastric, enteric, or dysenteric complication. As a short notice of the use of this substance is given in the Elements of the Practice of Physic by Dr. Craigie, without entering into all the details of the history of the introduction of this substance into the practice of medicine, the easiest way perhaps is to quote the account given in that work.

"The exhibition of charcoal for the cure of ague, as a substitute for bark, appears to have been practised in 1813 by Calcagno at Palermo, and afterwards by Dr. Calvert, Mr. Mackesy, Mr. Tully, and other English practitioners, in Sicily, (*Med. and Surg. Journ.* x. 15, 403); and it was used with that intention by Dr. Jackson in the West Indies. In simple ague it seems to produce little or no benefit. But in ague with affection of the gastric, or the hepatic, or gastro-enteric circulation, in ague with anguish at stomach, squeamishness, flatulence, or hiccup, and in dysenteric ague, its sanative influence is unequivocal and powerful. It may be given in doses of from ten to twenty grains in rice-water or arrow-root, either alone or with six or eight grains of rhubarb, and two or three grains of powder of ipecacuan. It appears to operate chiefly by rectifying disordered secretions of the stomach and bowels. It is probably by its charcoal, that the snuff of a candle, which has been alleged to be beneficial in curing ague, as mentioned by Lind, operates."—P. 137

NEW GERMAN WORKS.

Entwurf einer Bromatologie und Pomatologie für Kranke. Von Dr. C. F. L. WILDBERG. 8vo. Berlin, 1831; pp. 112. London: A. Schloss.

THIS is a compendium of dietetics, adapted for the guidance of invalids, and the instruction of young medical practitioners.

Zusätze zur Erkenntniss grosser Organization im Kleinen Raume. Von C. G. EHRENBURG. Folio. Berlin, 1836; pp. 30. London: A. Schloss.

Die Akalephen des rothen Meeres und der Organismus der Meduse der Ostsee. Von Ditto. Folio. Berlin, 1836; pp. 82. London: A. Schloss.

The author of these two memoirs is distinguished for his brilliant researches into the minutest structures of animated nature.

The former dissertation contains some novel observations relating to the duplicity of sex in the various Infusoria, the vascular system of the Rotatoria, together with a description of fifteen new kinds of Polygastrica, and one new kind of Rotatoria. The latter is an investigation into the anatomy of certain species of Medusa.

Über die Irren und deren Psychische Behandlung. Von Dr. BRAÜSLICH. 12mo. pp. 167. Meissen, 1837. London: A. Schloss.

This is, in a great measure, a psychological inquiry into the nature, causes, and treatment of insanity. It is addressed to non-professional as well as to professional readers. The author states, in the preface, that the number of insane persons in Germany ranges from 20 to 30,000.

Repertorium für Anatomie und Physiologie. Von G. Valentini. 8vo. Berlin, 1836. Bd. I. H., 1, 2, 3, 4. London: A. Schloss.

This work embodies some of the most interesting researches which have recently appeared in the domain of human, comparative, and vegetable anatomy, physiology and pathology. It moreover professes to furnish from year to year a retrospective glance of all that is being done in these different branches of science.

Medicinischer Almanach für das Jahr, 1838, von Dr. J. J. SACHS. Dritter Jahrgang, 12mo. Berlin, 1838, pp. 626. London: A. Schloss.

This work, which, properly speaking, is rather a medical annual than a medical

almanack, is edited by a physician already known by his writings on the *Materia Medica*. It is replete with useful information. The first portion comprises some original contributions; among the rest, an interesting account of a recent visit to the medical institutions of Erlangen, Nürnberg, Fürth, and Bamberg.

The next part (nearly 300 pages) is occupied with details of the latest discoveries in anatomy and physiology, and of the last improvements in pathology and therapeutics; systematically arranged, according to the plan adopted by the late Dr. Bluff, in his "*Leistungen und Fortschritte der Medicin in Deutschland*."

The work concludes with an elaborate digest of medical statistics, which, although relating chiefly to Germany, includes, nevertheless, other European states; and an obituary of distinguished German physicians.

De Animalculis Infusoriis.—J. A. LORENT Phil. Doct. 4to. pp. 40; Manheim, 1837. London: A. Schloss.

This dissertation contains a succinct account of the *infusoria*, more especially in relation to their mode of development. The subjoined table exhibits the recent advances of microscopical discovery, in this field of research.

"Linnæus described 3 genera 21 species
Müller..... 17378.....
Lamarck..... 23241.....
Ehrenberg.....125410"(p.11.)

Analekten der speziellen Pathologie und Therapie. Herausgegeben von Dr. H. BRESSLER und Dr. J. JACOBSON. 8vo. Berlin, 1837. London, A. Schloss.

In a former number of the *GAZETTE*, we noticed the German *Analekten* for Surgery and for the Diseases of Women. We have now before us the first number of a similar publication, devoted to practical medicine. These contain, *inter alia*, Sir B. Brodie's observations on local hysteria, and Dr. W. Stokes' investigations concerning vesicular emphysema.

British Medical Almanack.
Churchill, 1838.

This very useful little work again claims our attention. It contains almost every kind of information capable of being brought into an almanack, to which the medical practitioner can wish to refer.

While we thus do full justice to the Editor, we must add, that we do not feel much flattered by the total disregard of some hints we ventured to give last year; the imperfections then pointed out remaining unchanged.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, Dec. 28, 1837.

Richard Raymond, Listowel.—Herbert Fitzherbert Jones, Malmesbury.—Geo. Guy, Eastmeon.—Francis Macewan Maury, Leatherhead.—Francis Bellamy, Cheddington, Dorset.

Thursday, Jan. 4, 1838.

James Jenkinson Clarkson, Yealand.—Evan Evans, York.—W. Sankey, Hastingsleigh, Kent.—George Newstead.—James Evans, Tavistock, Devon.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, Jan. 2, 1838.

Abscess	1	Fever, Scarlet	7
Age and Debility	20	Fever, Typhus	5
Apoplexy	4	Heart, diseased	2
Asthma	11	Whooping Cough	14
Cancer	1	Inflammation	16
Childbirth	5	Bowels & Stomach	4
Consumption	50	Lungs and Pleura	6
Convulsions	18	Measles	1
Croup	2	Mortification	5
Dentition or Teething	3	Paralysis	1
Diarrhœa	1	Scrofula	1
Dropsy	14	Small-pox	6
Dropsy in the Brain	3	Unknown Causes	41
Erysipelas	3		
Fever	19	Casualties	6

Increase of Burials, as compared with }
the preceding week } 52

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.

Dec.	Thermometer.	Barometer.
Thursday . 21	from 35 to 44	29.75 to 30.21
Friday . . 22	35 51	30.02 29.87
Saturday . 23	40 51	29.78 29.89
Sunday . . 24	39 53	29.89 29.81
Monday . . 25	42 55	29.69 29.86
Tuesday . . 26	34 50	29.87 29.80
Wednesday 27	39 47	29.73 29.76

Winds, S.W. and S.E.

Except the 25th, generally cloudy, with frequent rain. Remarkably mild on the 22d and four following days.

Rain fallen, .075 of an inch.

Thursday . 28	from 43 to 51	29.77 to 29.82
Friday . . 29	41 51	29.79 29.77
Saturday . 30	43 53	29.85 29.89
Sunday . . 31	40 49	29.80 29.92
Jan. 1838.		
Monday . . 1	35 49	29.90 29.86
Tuesday . . 2	32 49	29.84 29.63
Wednesday 3	34 46	29.55 29.72

Wind, S. by E. and S. by W.

Except the 31st ult., and 1st and 3d inst., generally cloudy, with rain at times. Very mild on the 28th and two following days.

Rain fallen, .0375 of an inch.

CHARLES HENRY ADAMS.

WILSON & SON, Printers, 57, Skinner-st., London.

THE
LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF
Medicine and the Collateral Sciences.

SATURDAY, JANUARY 13, 1838.

LECTURES
ON THE
PHYSIOLOGY AND DISEASES OF
THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL
AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of
1836 and 1837,

At the Anatomical School, Kinnerton-Street,
near St George's Hospital,

By CHARLES J. B. WILLIAMS, M.D. F.R.S.

LECTURE XI.

Examination of the Chest through the vital properties or functions—Analysis of the General Symptoms (concluded)—Symptoms connected with the Circulation—Analysis of the Pulse—Varieties of the Pulse explained—State of the Pulse in Diseases of the Organs of Respiration—Symptoms from the Venous and Capillary Circulation—Symptoms of Fever, or increased heat; Perspiration—Symptoms from the state of the Secretions and other functions—Respective Value of the Physical Signs and General Symptoms.

WE come now to examine shortly the nature and value of the symptoms which diseases of the organs of respiration develop in other functions.

We have noticed, in the physiological part of this course, the close relations which subsist between the organs of respiration and those of circulation; and you will be prepared to expect that disease in the former should disturb the latter, and develop symptoms in the function of circulation. Accordingly we find such symptoms in the state of the *arterial pulse*, and in that of the *superficial capillary and venous* parts of the circulation.

528.—XXI.

You know how much the pulse has for ages been relied on as a guide in the diagnosis and treatment of all diseases; but if you have had much experience, you must know how fallacious it sometimes proves; and if you have had little experience, you will acknowledge that it is very difficult to distinguish the varieties of the pulse from one another. Now I cannot help thinking that some of the fallacies and difficulties connected with the pulse as a sign, arise from our studying it too empirically,—from our not rationally considering those elements on which its varieties depend, and a knowledge of which would enable us to understand and to foresee the circumstances which are capable of producing these varieties. As the subject of the action of the heart will more fully come under our notice in the latter part of this course, I shall now only detain you with a brief analysis of the nature and varieties of the pulse of arteries.

The arterial pulse, you know, is caused by the jets of blood thrown at certain intervals of time into the arteries by the contractions of the ventricles of the heart. The motion originates exclusively in these contractions, although it may be modified by the blood which is moved, and by the tubes which convey it. Now here you have three elements:—1, the heart; 2, the blood; and 3, the arteries;—and variations in the condition or action of each of these cause varieties in the arterial pulse. Let us consider a few of these variations.

1. Without noticing the modifications in the action of the heart resulting from disease of that organ—a subject to be considered hereafter—it is plain enough, that if the other elements be equal, the strength and frequency, or rhythm, of the contractions of the left ventricle of the heart, will determine the *strength* and *frequency* of the arterial pulse. But the contractions may have another quality—that of abrupt-

ness : being rather brisk and short than strong and complete, they communicate to the pulse that character which is called *sharp*. Now what property in the heart gives it this abruptness of contraction? what but an extreme irritability? There is sometimes this irritability in inflammations and fevers; but you may find it also in conditions of mere nervous irritation, of which it is more distinctive. And it is when these co-exist with inflammation or fever, that the pulse presents a sharpness, in addition to other qualities more peculiar to inflammation. In sthenic irritations, or those connected with fulness and tone of the vascular system, which may tend either to acute inflammation or to active hæmorrhage or other discharge, the heart's contractions are strong as well as sharp; and so is the pulse. In these cases, although the original irritation were local, it has now reached the centre of the circulation, and thence, distributed through the whole system, becomes general. But let us see how the other elements modify the pulse.

2. There can be no doubt that the blood in the heart and vessels determines by its *quantity* the character of the arterial pulse: very possibly it does so by its *quality* likewise; but this is not so easy to prove. The fulness and strength of the pulse in the arteries depend materially on the quantity of blood in them; and when the pulse is frequent as well as full, there is the greater proof of plethora, inasmuch as it shows that there is a considerable jet thrown into the arteries at each contraction, notwithstanding that the contractions are so frequently repeated. But you may have a full system of blood-vessels without a large or strong pulse,—as when the heart is acting feebly or faintly; and where its irritability is lowered, such a mode of action may be actually caused by the congestion or distention which for a time oppresses the function until it is roused into reaction. Under these circumstances, blood-letting will often increase the fulness and strength of the pulse. The opposite condition—a defective quantity of blood—will modify the pulse differently according to the state of the other elements, the action of the heart, and the arterial tubes. When the irritability of the heart is reduced, together with the quantity of blood, the pulse will become softer, weaker, and less frequent. But it frequently happens, especially in nervous temperaments, or where the depletion has been carried to excess, that the diminution of the blood is accompanied with an augmented irritability of the heart, and the pulse becomes not only quicker, but sharper than usual; and the effect of the abrupt jets into a small bulk

of blood, contained in imperfectly distended tubes, is to give to the pulse that jerking or bounding character, as if a mere ball of liquid were suddenly shot through the empty tube,—which is so remarkable in the irritation of inanition and chlorosis.

3. But we cannot fully understand the variations of the pulse without attending to the properties of the tubes in which it is felt. If the arteries were tubes of an unyielding or an unvariable character, then the pulse in them would more uniformly represent that of the heart, which would be transmitted through them unmodified. But you know that they are not so: they possess properties of *elasticity* and *tonicity*, which vary according to circumstances, and which modify the pulses from the heart, by changing the size of the tubes, and the yielding or the resisting nature of their walls. Now just think how differently the impulse of a jet of blood must be transmitted by vessels when they are large and yielding, and when they are contracted and tense: in the first case the pulse would be *soft* and *full*, in the latter *hard* and *small*. What are the circumstances which affect the tonicity and elasticity of the arterial coats, and thereby the pulse? We know these but imperfectly; further experiments are wanted to elucidate them: but the following are pretty well ascertained, and they should not be forgotten in estimating the signs of the pulse. Cold causes the arteries to contract, and therefore renders the pulse smaller. You know how a cold lotion will often diminish the fulness and throbbing of the arteries of an inflamed part; and I have seen the same effect of cold more strikingly produced in the large arteries. In the experiments on the sounds of the heart which I carried on in the room below in February 1835, I repeatedly observed, that when the aorta of an ass, recently killed, was plunged into cold water, it contracted, so as not to permit the introduction even of the little finger, and its coats acquired an increased thickness and rigidity; the pulmonary artery did not contract near so much. The circumstance of temperature must therefore be taken into account in judging of the pulse; for cold may render the pulse of an artery small and hard, or if severe, small and weak, when the action of the heart and the condition of the system would give it the reverse qualities. Heat, on the other hand, within certain limits, tends to diminish the tonic contraction of the arteries; so that under its influence they receive more strongly and fully the pulse from the heart. You know how warmth restored to a limb makes it throb with these expanded pulses.

But there is another circumstance that

may modify these actions of heat and cold on the pulse, besides proving by itself a cause of modification—the condition of the capillary circulation. When this is not free, the artery will be more distended, and therefore the pulse harder and stronger than usual; and thus in fevers, where the surface is pale and constricted in the cold stage, and dry and unrelaxed in the hot stage, the pulse often preserves through these changes of temperature a hardness and strength, which would be much more varied were the capillary vessels free and exhaling their usual excretion, and which is actually diminished under the influence of a warm bath or temporary moisture of the skin, although the fever still continue. Again, whatever view we take of the nature of inflammation, we cannot, in the present state of pathological knowledge, doubt that the circulation through the inflamed vessels is to a certain degree obstructed; whilst, either as a consequence of this, or from some co-operating influence, the vessels leading to the part become dilated, and being thus more open than others to the pulse-wave from the heart, they become the seat of that throbbing hard pulse, that has been mistaken for increased action of the vessels themselves. And there are many other variations in the pulse explicable on these principles, but we really have not time to consider them further in detail, as they might occupy two or three lectures.

There is, however, one more cause of variety connected with the arteries, so frequently occurring, that I must mention it;—I mean the difference in the arteries of different individuals. Without any adequate difference in the action of the heart, in the quantity of blood, or in the temperature, you will find a very remarkable variety in the character of the pulse in different healthy individuals, and the same difference extends to the modifications of disease. Some have always a soft large pulse; others a small feeble one; others small and hard; others, again, have habitually a hard strong pulse, which scarcely becomes soft under any circumstances. The first depends on the arteries being large with thin elastic coats. The small feeble pulse may result from their small size and thin coats: this is common in females, and may co-exist with inordinate action of the heart. The hard wiry pulse is connected with small arteries with rigid coats; and the same rigidity or deficiency of elasticity in the coats of arteries of larger size gives that unvarying hardness and strength to the pulse which we so often meet with in old people, and which renders it so uncertain a sign in these cases. You may often, in the radial artery, feel the per-

manent thickening and hardness of its coats, which thus, like a tube of glass or metal, rigidly transmit the heart's pulses, without tempering them by any yielding or spring. With these peculiarities of pulse there are often connected characters of constitution or temperament, and propensities to disease or health, which are of great importance in guiding us in practice.

I cannot attempt to go into many interesting details which this subject presents; but I hope that the elementary view which I have given may prove something of a key or guide, and that you may now see (for instance) why you are to depend more as a sign of inflammation on a pulse which continues quick and strong, or hard, with permanent heat of the skin, or with perspiration, or through varying circumstances of temperature, than on one which, although very quick, sharp, and jerking, yet without fullness or permanency of character, obviously depends on an irritation more mobile and transient than that of a fixed inflammation. What a capital example of the fallacies of the pulse have we witnessed in many cases of the influenza that has been lately infesting this quarter of the globe! Here you would have a pulse as sharp and frequent as at the commencement of an exanthematous fever; and this with the local symptoms betrayed many practitioners into the supposition that a most serious inflammatory or febrile disease was commencing; but, perhaps, the very next morning you would find the pulse weak, unsteady, and with a moderate degree of frequency; and if as often happened, an exacerbation took place again at night, the pulse and circulation exhibited the same excessive disturbance, quite disproportionate to the ephemeral character of the fever, and to the degree of the local affection. You would have been tempted by these signs, to use, at one time of the day, active antiphlogistic treatment; at another, stimulating remedies; so rapidly were the functions overbalanced from one extreme to the other, by the excitement and depression which this morbid influence caused in the system.

Besides the general causes which modify the pulse, which we have now briefly considered, there are some specially connected with diseases of the pectoral organs. Those arising from diseases of the apparatus of the circulation will be treated of hereafter. But severe affections of the respiratory organs also sometimes signally modify the pulse, and that in a manner which may tend to confuse its indications. You know how closely the heart is linked with the lungs; by the circulation even more closely than by mere position; for the lungs may be said to lie between the two

compartments of the heart; and any considerable obstruction in the lungs will derange the usual relations of these compartments. There is then a distention or over-stimulation of the right side of the heart; while the left, receiving a diminished quantity of blood from the lungs, and that not thoroughly aerated, is less excited than usual, and may give to the arterial pulse a character of weakness and smallness that by no means represents the condition of the whole vascular system, and which often is remarkably contrasted by the action of the right side of the heart, as felt or heard under the sternum. These varieties are produced by any of those affections of the chest which infringe far on the respiratory function. These are more commonly those of the bronchial and parenchymatous kind, which have accordingly been described to be accompanied by a softer and weaker pulse than those affecting the serous membranes. But you may have a pleurisy also with a small weak pulse, when the effusion or pain is such as to interfere largely with the function of the lungs. Neither are you to suppose that the pulse in severe pneumonia or bronchitis is always weak, even when these affections infringe considerably on the function of the lungs. Even under asphyxiating influences the left ventricle may sometimes become excited, together with the right, and give a sharpness to the pulse, which, combined with the arterial tension of fever, may be readily mistaken for hardness and strength. But this character is seldom permanent; and you will generally find in all diseases, when the function of respiration is much impaired, that the pulse soon loses its body and strength. These considerations suggest the expediency of examining the state of the circulation not only by the arterial pulse, but also by the pulsations of the heart itself, and by the condition of the veins and capillaries.

Under the circumstances just mentioned, when the indications of the arterial pulse are most variable and deceptive, you may often find useful signs in the condition of the *venous* and *capillary* part of the circulation. The distention of the more superficial venous trunks, especially the jugulars, in which a double pulsation often shews also the retropulsive action of an over-distended right ventricle; the fulness of the capillaries of the lips, tongue, throat, cheeks, eyelids, nails, and other parts, at first having a florid and flushed appearance, but afterwards, as the respiration becomes more injured, assuming a purple or livid hue,—are signs of great practical importance, and of a constancy more approaching to that of the physical signs. They do not, however, present

themselves in the early and more tractable stages of disease; and they are always less distinct in pallid persons with small superficial vessels.

Intimately connected with the state of the circulation is the symptom of *general fever*, or *increased heat*, which attends many diseases of the chest. It depends on increased force and rapidity of the circulation, with diminished perspiration. When the perspiration is restored, the heat always falls. This exhalation of fluid not only lowers the temperature by its physical agency of evaporation, but being in itself a sign of a relaxing of the superficial vessels, it implies an abatement of the vital irritation. In the more transient forms of fever, such as the intermittent and hectic, the profuse perspiration sometimes reduces the animal heat to below the natural standard, just as the circulation is proportionately enfeebled; and you have the same chilling influence illustrated by the cold sweats which succeed to temporary and irregular excitement. There are degrees of vascular irritation in which the increased heat of skin is partial, and determined by the structure of particular parts. Thus in the asthenic excitement of hectic fever, the heat is most felt in the palms of the hands and soles of the feet, because the circulation is not strong enough to drive the perspirable excretion through the thick cuticle of those parts, which become consequently dry and hot. The same thickness of cuticle, on the other hand, when once imbued with perspiration, often keeps these parts soft and moist, when there is no sensible perspiration on other parts. Not unfrequently the unequal state of the circulation is exhibited in febrile and inflammatory disorders by the heat of the abdomen, back, chest, or head, whilst the extremities are cooler than usual; and occasionally the same locally increased action is manifested by partial sweats, which prevent the increase of heat, and tend to reduce the excitement. I have known a patient with pleurisy perspire profusely only from the affected side, for several days; and nothing is more common, in slight abdominal inflammations, than to find the pungent heat of the belly relieved by a perspiration equally confined to that part. But you know how we practise on the same principle, in applying to irritated or inflamed parts poultices, fomentations, and partial baths, which tend to bring the skin and superficial vessels to the same relaxed and expanded state which they have in a perspiring part.

I think, then, that it will be plain to you that heat of skin must be an uncertain symptom; for it depends on a condition of the superficial circulation that is

by no means constantly associated with disease of the internal organs. When present, it may as much result from a general cause—an idiopathic fever—as from a local inflammation; and cases are not uncommon in which severe, and even fatal, visceral inflammations are attended, through a great part of their course, by free perspiration; nay, the same may be said of some fevers which are called idiopathic. Still the heat and condition of the skin become valuable guides, when taken in conjunction with other signs, inasmuch as they indicate the constitutional disturbance, which is an important part of the disease, and which is sometimes as much to be considered in the treatment as the local disease which has excited it. So, likewise, when the presence of a disease has been established by other signs, the condition of the skin may prove a measure of its increase or diminution more delicate and sooner appreciable than can be found in the physical signs. Thus an increased heat of skin, coming on during a bronchial or pulmonary inflammation, either indicates an increase of that inflammation or the addition of some abdominal or other irritation, which tends to aggravate the condition of the patient. So, too, perspiration breaking out in the hitherto dry and hot skin of a pleuritic patient, occasionally does prove *critical*; whether that word be applied to the excretion as a cause or as a sign of the amendment.

Where there is disorder of the circulation, especially of a febrile kind, you may well expect alteration of the *secretions*, which are so intimately connected with it: hence you find the urine is scanty and high coloured, and the secretions of the liver and intestines variously deranged. As a natural consequence, too, there will be disorder of the digestive and nutritive function: the tongue will be furred, or florid; the appetite will fail; the stomach will cease to digest; thirst will torment; the blood, no longer fed with chyle, will not duly nourish the textures, nor support the functions; the strength will fail; absorption continuing active, if time permit, emaciation will ensue; and various complications of these disturbances may differently modify the character of diseases of the chest. The sensorial functions, too, may be deranged, either in consequence of the secondary visceral disturbances, or more rarely by a more direct influence of the imperfect respiration on the brain and nervous system. You need only recollect our views of asphyxia, to perceive also how certain states of the nervous system may tend to develop disease of the lungs. Thus insensibility, or coma, causes imperfect respiration, and

consequently congestion of the lungs; and, as we shall hereafter see, a long-continued congestion of the lungs only requires the addition of vascular reaction to convert it into inflammation. Persons rarely recover from an asphyxiated state, without suffering more or less from the injury which it leaves in the vessels of the lungs; and not a few who have been recovered from suspended animation, have sunk under the pneumonia, or bronchitis, which supervened.

I have been thus rapidly glancing at some of the pathological relations of the organs of respiration to other functions, to give you an opportunity of considering rationally the nature and value of general symptoms. We have said enough of those more nearly related to the chest—dyspnoea, cough, pain, and signs of the circulation, with its concomitant, temperature: although often equivocal, yet when strongly marked, they sometimes assist us in diagnosis. Now it is just the reverse with the symptoms arising out of disorder of the other functions. The altered secretions of the kidneys, the liver, and the intestines, cannot inform us of the nature or presence of a disease of the chest; and still less will gastric derangement or sensorial disturbance. Nay, not only will they not direct, but they tend essentially to blind us to the presence of pectoral disease; for they set up prominent symptoms of a new character, that may take the attention entirely from the real source of disease, and fix it on the brain, the liver, the stomach or intestines, the affections of which are only secondary, and often trivial. How often do we find a peripneumony, or a bronchitis, disguised by delirium or stupor, or by vomiting, a loaded tongue, or diarrhoea. How often a pleurisy, masked by a jaundiced skin, a tender right hypochondrium, and clay-coloured feces; or by a lumbago, or a nephralgia! How often tubercular consumption, obscured by sundry bilious, dyspeptic, or nervous symptoms! It will be happy for us, for our credit at least, if not always for the success of our practice, if we detect the enemy through its false colours, ere it triumph, and before the scalpel shall proclaim the delusion of our unwary minds! The physical signs will enable us to do this, and again I commend them to your most attentive study. We have been occupied for three lectures with the general symptoms, and I trust not unprofitably; but on taking a rational review of them, in comparison with the physical signs, I think you must come to the conclusion to which my reason and experience have long brought me, that as diagnostic means, the general symptoms fall far short of the physical signs.

But do not suppose, that because the

general symptoms are often comparatively of little aid in diagnosis, we are to neglect the study of them. They are almost always of great importance in prognosis and practice. The physical signs more surely show how the pectoral organs suffer; but having discovered this, to the general symptoms we must look for how the system suffers; and as the system often closely sympathizes with the injured organ, we may, through them, often watch the first turns of the disease before the change in the organ becomes physically appreciable. In the general symptoms we seek for those *critical* phenomena, which, although sometimes deceptive, yet generally announce the tendency of the disease to one or other mode of termination. In them we study the vital forces and properties with which nature works, and the signs of what nature can do; and in our methods of treatment these become the standards to which we direct, and by which we modify, our remedies. When we treat a patient with peripneumony or catarrh, we do not apply our remedies merely to the local lesions, inflamed vessels, or a discharging membrane; we study the system at large, we examine other functions through the general symptoms, and we direct our treatment with due reference to indications from all these several sources. You see, then, that the mere stethoscopist is but ill fitted to practise medicine. He may justly boast of his skill in diagnosis: his place of triumph will be the dissecting-room, where he can shew the lesions that he had detected; but his practice at the bed-side will be unsuccessful in proportion as local lesions vary in their general relations, and in the conditions of the constitution, or of other functions that may accompany them. The judicious physician will not omit to study the condition of the vital properties, which are exhibited in the general symptoms, as well as the local physical changes which have been already produced; and whilst he chiefly confides in the physical signs to indicate and measure the present local lesions, he carefully watches in the general symptoms the tendencies of those properties and functions which are capable of increasing or modifying these lesions, and are equally liable to be affected by them. The general symptoms being less intelligible and certain than the physical signs, need more experience to enable us to appreciate them; but I hope that you now think that even these also may be *rationally* studied, and may derive a light from a knowledge of physiology and the physical signs, which experience alone could never throw on them.

LECTURES
ON THE
ENTOZOA,
OR
INTERNAL PARASITES OF THE
HUMAN BODY,

Being Part of a Course on Morbid Anatomy,
delivered at the London Hospital,

By T. B. CURLING, Esq.

LECTURE II.

WE now come to the consideration of the second class of the Entozoa, the Protelmintha. There are two kinds, the *Cercaria Seminis*, and *Trichina Spiralis*.

3. *Cercaria Seminis*, Zoosperms, or Spermatozoa.

These names are applied to minute animals which exist in the secretion of the testicle at puberty. They resemble tadpoles in shape, and when placed in the field of a microscope they are seen moving

Zoosperms.



Man.



Bull.



Rabbit.



Sparrow.



Frog.



Carp.



Silk worm Moth.

about, often with considerable rapidity, apparently by means of a caudal appendage or tail, which varies in length in the spermatozoa of different animals. Mr. Owen classes them with the entozoa; but as they are proper to health, and are usually treated of in connexion with the subject of generation, I shall not dwell further upon them here.

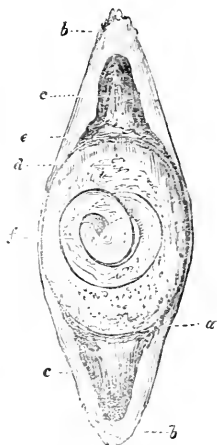
M. Donné, of Paris, who has been much engaged in the examination, both chemically and microscopically, of the various secretions, has very recently announced the discovery of an animalcule which he has detected in muco-purulent discharges from the vagina. It is termed the *Trichomonas vaginale*, and varies from $\frac{1}{150}$ to $\frac{1}{30}$ of a millimetre in diameter. Its body generally appears round, but assumes various forms. At its anterior extremity the animal is provided with a filiform appendix, double the length of the body, which it has the power of moving freely. On one side there are several shorter appendices, which are more difficult to be perceived. These animalcules have never been observed in the simple mucous discharges, and M. Donné is inclined to regard them as characteristic of gonorrhœal matter. M. Donné has likewise noticed a number of animalcules, having the form of the Vibrions, described by Müller under the name of *Vibrio lineola*, which he has observed constantly in the pus of chancres on the glans penis and vulva. He states that they do not exist in non-syphilitic pus, nor in that of buboes, but are only found in the matter of the primary sores. Should the accuracy of these observations be confirmed by further researches, the discovery must be regarded as of an interesting and important nature, and as calculated to suggest many curious speculations and inquiries respecting the development of these diseases.

4. *Trichina Spiralis*.

This is a minute worm, infesting human muscle, which has recently been discovered by Mr. Owen. Anatomists had occasionally observed, in dissection, the muscles presenting a peculiar speckled appearance. The nature, however, of the white specks which are found thickly studded upon and between the fibres of the various voluntary muscles, escaped detection, until a portion of muscle thus beset was transmitted to Mr. Owen for examination, in 1835. The specks were then ascertained to be produced by cysts containing a worm. The cysts vary in distinctness according to their degrees of

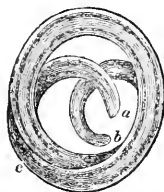
Cysts of the *Trichina*, in situ.

opacity, whiteness, and hardness. They are of an elliptical figure, and the extremities are attenuated, elongated, and more opaque than the intermediate part. They measure generally about $\frac{1}{50}$ th of an inch in length and $\frac{1}{150}$ th of an inch in breadth, and they are adherent to the surrounding cellular substance by the whole of their external surface; being attached loosely at the middle dilated part, but more closely at the extremities. These cysts, like those

Separate Cyst of the *Trichina*.

A separate cyst, with the worm coiled up in its interior, and seen through the transparent walls.

containing the acephalocysts and echinococci, are composed of condensed cellular tissue, and are of a laminated texture. Occasionally they become hardened by the deposition of earthy matter, feeling gritty upon pressure. On tearing open the cyst, which may be done with a cataract needle, in the field of the microscope the worm is discovered within, disposed in two or two and a half coils, and surrounded by a transparent fluid. The trichina measures from 1-25th to 1-30th of an inch in length, and 1-700th of an inch in diameter. It is cylindrical and filiform, termi-



Trichina magnified.

nating obtusely at both extremities, which are of unequal sizes; tapering towards one end, but continuing of uniform diameter to the opposite extremity, which Mr. Owen regards as the head, in consequence of the constant appearance of a transverse linear mouth. The worms appear to consist of a smooth transparent exterior skin, inclosing a fine granular and extremely fragile parenchyma. Dr. A. Farre has ascertained that they possess

an intestinal canal, with distinct parietes; and Mr. Owen has more recently perceived a transverse slit close to the small extremity, which he regards as the anus.

The *trichina* appears to be chiefly, if not entirely, confined to the muscles of volition; and those which are superficial, such as the pectoralis major and platysma myoides, usually contain them in greater abundance than the muscles which are deeply seated. In addition to the larger voluntary muscles, they have been detected in the muscles of the external ear and of the soft palate; also in the constrictors of the pharynx and in the levator and sphincter ani. They have not yet, however, been found in the heart, in the muscular tunic of the alimentary canal, or in the muscular fibres of the bladder. It is an interesting fact, noticed by Mr. Owen, that all the muscles infested by the *trichina* are characterized by the striated appearance of the ultimate fasciculi; while the muscles of organic life, in which they are absent, have, with the exception of the heart, smooth fibres, not grouped into fasciculi, but reticularly united.

The presence of these worms in the system does not appear to be connected with age, sex, or any peculiar form of disease. They have been found in the bodies of persons who have died of tubercles in the lungs, aneurism of the aorta, and hectic fever. No painful or inconvenient symptoms were present, in these cases, to excite any suspicion of a morbid state of the muscular system, and it is probable that, in all instances, the patient himself is unconscious of the presence of the microscopic parasites which are enjoying their vitality at his expense. Although they are so minute in size as scarcely to be perceptible to the naked eye, yet, when present, they exist in such countless numbers, and their distribution throughout the muscular system is so extensive, that it was imagined they must occasion debility, from the quantity of nutriment required for their support. This opinion, however, is not sustained, for I have found the *trichina* in the muscles of two robust men, who were killed while in the apparent enjoyment of good health; one by fracture of the skull, the other by fracture of nearly all the ribs, with laceration of the lungs: the first was 58 years of age, the other 60.

The occurrence of the *trichina* is certainly rare, but they were found during the last winter, once or twice, in most of the dissecting-rooms in London. I have myself observed them three times, whilst conducting post-mortem examinations, and I have met with them once in dissection. It is, however, a curious fact, that my brother, Henry Curling, who was constantly engaged during last season, in

FIG. 2.

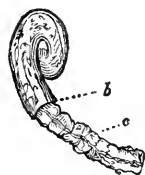


FIG. 1.

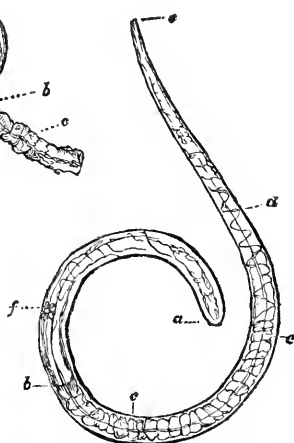


FIG. 1.—Worm extracted from cyst, shewing the alimentary canal. (f) A small body regarded by Dr. Farr as the ovary.

FIG. 2.—Portion of worm shewing the alimentary canal protruding from the cut surface.

Clamart's large dissecting-rooms, at Paris, where the number of bodies admitted exceeds those dissected at all the schools in London, did not hear of any instance in which this worm was discovered; though having been but recently described, it would not have been liable, if present, to have escaped detection. The only specimen seen by Giralddi, one of the most diligent of the prosecutors there, had been brought over from England. The distinguished physiologist, Müller, who visited this country in the autumn, informed me that he had met with it in Germany.

The *Sterelmintha*, or third class of the Human Entozoa, contains five genera. We shall speak first of the

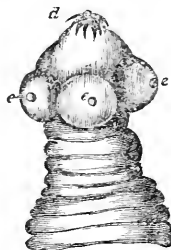
5. *Cysticercus Cellulosæ*,

(from *κυστις*, a bladder, and *κερκος*, a tail). It consists of a head, neck, and dilated cyst, of a spheroidal form. Of the genus *Cysticercus* there are several species, distinguished for the most part by the forms and proportions of the neck, or body, intervening between the head and the cyst. The only species known to infest the human body is the *Cysticercus Cellulosæ*, which



Cysticercus Cellulosæ.

is developed like the trichina, in the cellular tissue between the fibres of the muscles, and is also included in a similar adventitious cyst. The head of this parasite is furnished with a beak, or proboscis, armed with a double circle of hooks, for irritation and adhesion, and four promi-

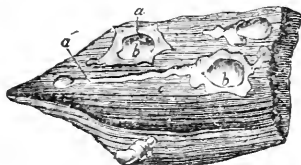


Magnified head of *Cysticercus Cellulosæ*.

nent suckers, for imbibing the surrounding nutriment, the fluid secreted by the adventitious cyst in which they are lodged. The head and neck are usually retracted within the cystic pouch, their situation

being indicated by a white opaque spot in the transparent vesicle.

The occurrence of this entozoon in the human subject appears to be rare, especially in this country. It is less uncommon on the continent. Rudolphi relates, that out of two hundred and fifty bodies dissected annually at the Anatomical School of Berlin, from four to five were found, through nine consecutive years, to be infested more or less abundantly with the *Cysticercus Cellulosæ*. Bremser, however, endeavoured for ten years to procure them at the Great Hospital, and in the Anatomical Amphitheatre of Vienna, but in vain. The development of these animals is not limited, like the *Trichina*, to the voluntary muscles, for they have been discovered in the heart and œsophagus, and also in parts not muscular, as the brain, especially the plexus choroides, and in the eye. The muscular tissue around the cyst is not otherwise diseased.



Portion of Human Muscle with *Cysticercus Cellulosæ*.

Soemmering first observed this parasite in the anterior chamber of the eye, in the case of a young woman aged 18. In the course of seven months it became twice as large as when first observed, and attained the size of a pea. On being extracted through a small incision in the cornea and put into warm water, it continued to move for more than half an hour. It then became gradually opaque and white; and with the microscope the four suckers and the double circle of hooks, forming the head of the animal, were plainly discerned. The *Cysticercus Cellulosæ* has been detected since, in the anterior chamber of the eye of a lively healthy child, seven years of age, after an attack of ophthalmia, by Mr. Logan, of Scotland, in 1833. It floated unattached in the anterior chamber, was about two lines in diameter, and of a spherical form, except that a slender process was seen to be projected or elongated from time to time, and again retracted, so as to be completely hid within the cystic portion. It interfered with vision, more or less, according to its position, and the form which it assumed. As it subsequently enlarged in size, so as to impede the motions of the iris, and excite inflammation in the sclerótica and conjunctiva, it was extracted through the cornea, at the Glasgow Eye Infirmary. The animal was torn in shreds in the operation, but the

result was satisfactory as regards vision. Cysticerci sometimes occur in the brain in considerable numbers. Treutler once found fifteen in one plexus choroides, and two in the other. They are not, however, so common as the simple serous cysts, for which they may be mistaken. In the brain of an epileptic patient, who died in the Bicêtre at Paris, Cruveilhier found at least a hundred of these animals. Some were seated in the sub-arachnoid cellular tissue of the brain and cerebellum; others occupied the substance of the convolutions, and central part of the hemispheres. As many as fifty were observed in the structure of the cerebellum, and others were discovered around the spinal cord.

I know of no symptoms by which the presence of these parasites in the muscles can be recognised, nor any mode of treatment by which they can be expelled the system.

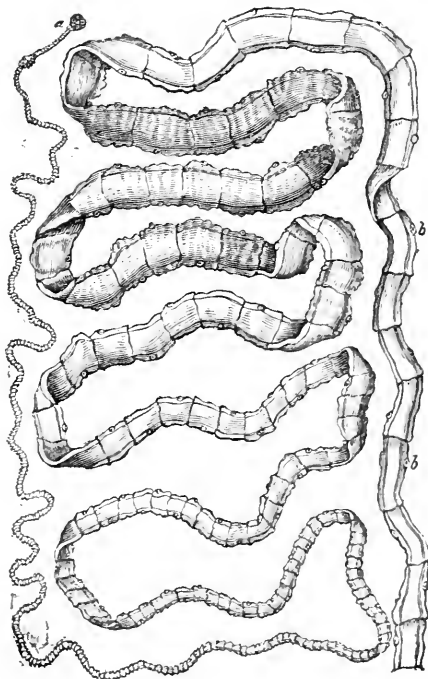
The *Cysticercus Cellulose* likewise occurs in quadrupeds, and is found most commonly, and in greatest abundance, in the hog, occasioning that state of the muscles which is vulgarly termed "measly pork." In two measly pigs dissected by Andral, cysticerci were found in the subcutaneous

and intermuscular cellular tissue, in the different folds of the peritoneum, and in the liver, lungs, and substance of the heart.

The *Cœnurus*, or *polycephalous hydatid*, which is so frequently developed in the brain of the sheep, and is the cause of the affection known under the denomination of the "staggers," does not occur in man.

6. *Tania solium*,

or common tape-worm of this country. The *Tania solium* consists of a flat, elongated, articulated body, varying in breadth from one-fourth of a line at its anterior part, to three or four lines at its widest, from whence it again diminishes to its termination. The head, which is so small that it cannot well be seen without the aid of a strong lens, is somewhat hemispherical, and provided on its anterior surface with a prominence, encircled by a double row of minute recurved processes or hooks, which are surrounded by four suckers or mouths. The circle of minute hooks are not constantly found, and Bremser supposes that they disappear with age. The anterior joints of the neck are very short; those immediately follow-



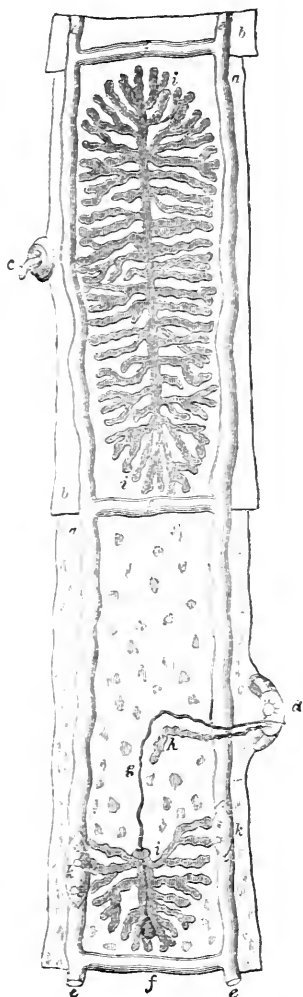
Tania solium, two-thirds the natural size.

ing are nearly square, their length scarcely exceeding their breadth; the remaining ones are long, the last series of the segments being sometimes twice or three times as long as they are broad. The first articulation is received into the base of the head, and each subsequent joint is let into the one in front of it. Both longitudinal and transverse fibres may be observed in the larger specimens of the *Tænia*. Each segment possesses distinct and separate motions, the fibres not being continued from one joint to the other. A *tænia*, when recently expelled, is occasionally contracted to the length of a few inches, the segments appearing as close-set transverse striæ; but after remaining for a few hours in water, it may be found to measure as many feet. Werner states, that a *tænia* which extended from the anus of a patient to the length of three feet, returned itself almost entirely into the intestine. Other and still more extraordinary instances of the powers of motion possessed by *tæniæ* have been noticed; and it is without doubt to this cause that we must attribute the circumstance, that a part of the body of one of these worms is sometimes found to be tied in a complicated knot, a specimen of which is preserved in the museum. From the four



Tænia tied in a knot.

months already described proceed two vessels, which run along the whole length of the animal, near the margins of the joints. These, which are the nutrient vessels, are united by transverse canals at each joint; and they terminate, according to Rudophi, in a common orifice in the last joint. In a large specimen of tape-worm, in which the nutrient vessels were injected with quicksilver, they were found to pursue a regular serpentine course; and as in repeated trials the mercury could only be made to pass in one direction, viz. towards the posterior extremity, I



Portion of a *Tænia* magnified, shewing the Nutrient Canals and Generative Organs.

conclude that they are furnished with valves. All the articulations have marginal foramina which open alternately, that is to say, on the opposite edges of two contiguous joints. Thus, of the two free margins of each articulation, commonly one only possesses a pore; this, however, is by no means invariable, and in some instances there are two pores on each joint. In each joint there is a large branched ovary, from which a duct is continued to the marginal opening. The ova are crowded in the ovary; and in those situated in the posterior segment, of the

body, they generally present a brownish colour, which renders the arborescent form of the ovary sufficiently conspicuous. A duct more slender and opaque than the oviduct, and containing a gummy secretion, may be traced extending from the termination of the oviduct at the lateral opening, to the middle of the segment, where it terminates in a small oval vesicle. This is the male duct and gland, and the ova are impregnated in their passage outwards. The structure, therefore, of the generative apparatus in the *Tænia* is androgynous, each segment of the individual being sufficient for reproduction.

It has been a question whether the nutriment of the tape-worm is imbibed through the pores which I have described at the sides or margins of each joint, or whether the entire body is dependent for its nourishment upon the four mouths from which the lateral canals commence. Rudolphi and Mr. Owen are of opinion that the lateral or marginal orifices of the segments are exclusively the outlets of the generative organs. In some species of tape-worm, in which no ovaria have been detected, there has been a corresponding absence of the marginal pores, while the lateral longitudinal canals have been present and of the ordinary size. In other species the generative pores open upon the middle of one of the surfaces of each segment, and in these it is plain that the lateral nutrient vessels have no communication with the central pores. The orifices of the segments correspond, in short, with the modifications of the generative apparatus, while the nutrient canals undergo no corresponding change. We may conclude, therefore, that the head of the *tænia* is the sole natural instrument by which it imbibes its nutriment, and it is to the expulsion of this part that our attention should be chiefly directed in endeavouring to relieve a patient from these injurious parasites.

The *tænia solium* measures usually from five to ten feet. It has often been found much longer. Tape-worms twenty feet long are by no means rare. Robin relates in one of the French journals that he once found one extending from the pylorus to within seven inches of the anus, which would make its length at least thirty feet. Cases, however, are recorded in which they have attained the length of a hundred feet, and even more. Some of the accounts, such as a case mentioned in the Copenhagen Transactions of a worm measuring eight hundred fells, are not perhaps altogether worthy of credit. The name of *tænia solium*, or *solitary worm*, is not strictly applicable to it, as it is well known that it may co-exist with several others of its own or even different species.

Bremser has frequently known two or three worms to be passed simultaneously by the same individual. As the joints are very readily separated, you must be careful not to mistake several portions of one worm for different parasites. It is, indeed, rather rare to find a *tænia* of any size entire, the larger tape-worms being found almost always torn across, and deprived of the final articulations. Sir A. Carlisle conceived that the detached joints were capable of becoming distinct animals, but later investigations have shewn this opinion to be erroneous.

The *tænia solium* is met with in the principal countries of Europe, but not in all with equal frequency. It is common in England, Holland, and Germany; and travellers relate that it infests the inhabitants of Egypt.

Tapeworms generally reside in the small intestines, where it does not appear that they produce any important lesions. It is not often that we have an opportunity of ascertaining the state of the intestinal canal when they are present. I examined the body of a female twenty-three years of age, who had passed portions of tapeworms by stool for several years, and who had voided part of a very large one by the mouth the day before death, but although a large *tænia* was found in the ileum, the only lesion in the intestinal canal, which was examined carefully throughout, was a very slight hypertrophy of the follicles, chiefly of the *glandulæ solitariae*. There was no preternatural injection of the vessels. They have been found adhering pretty closely to the mucous membrane by means of the circle of hooks, and it has been imagined that they might occasion ulceration and even perforation of the intestine; but I do not know of any well-authenticated case in which they had produced so unfortunate a result.

The statements of various writers respecting the effects of tape-worms upon the œconomy differ considerably, some representing them as exciting scarcely any disturbance, whilst others regard them as the source of very serious mischief. Rudolphi speaks of them as existing in very healthy individuals, and only being detected in consequence of portions being found in the stools. Such may sometimes be the case, but there can be no question that in general they occasion more or less derangement in the system and impairment of the general health. The best account of the symptoms occasioned by these animals is by Louis, and it is drawn up with all the care and accuracy which characterize the productions of that distinguished pathologist. It results from his observations, that the origin of *tænia* cannot be ascribed to bad nourishment, a weak state

of the constitution, previous disease, or a sedentary life, and that they occur at all ages. The greater number of the ten patients who were the subjects of his observations, had evacuated fragments of tape-worms almost daily for several years, and sometimes in the intervals of the stools, portions having been observed in their beds and amongst their clothes. In none of the cases were the functions of the body perfectly healthy, though the degree of derangement varied much in different individuals, being very inconsiderable in some, and yet so distressing in others, that they took every means within their power to rid themselves of the disease. The chief symptoms were colicky and other kinds of pain in the abdomen, itching about the anus, and at the end of the nose, more or less derangement of the appetite and digestion, and pain at the epigastrium. Headache was rare, but pain and lassitude in the limbs frequently existed, and to so considerable a degree, as to prevent the patients from continuing their occupations. Other symptoms less constant and of an anomalous character are described; but he remarks that when there is pain in the abdomen, colic more or less severe and frequent, but unaccompanied with diarrhœa, and itching at the anus and at the extremity of the nose, we may then conclude almost with certainty that there is a worm existing in the intestinal canal.—In the case of the female already alluded to, whose body I examined, there was a remarkable prolapsus of the uterus, an occurrence rare at so early an age as twenty-three, and before the organ had been impregnated. It is questionable, therefore, whether the prolapsus was not in some degree the result of the irritation occasioned by the worm.

I shall leave what I have to say, as to the treatment which should be pursued for the removal of tæniæ, until I have spoken of other worms which infest the intestinal canal.

RECOLLECTIONS OF CHOLERA;

ITS NATURE AND TREATMENT.

BY WILLIAM GRIFFIN, M.D.

Limerick.

The Cholera at Limerick.

No. II.

THE cholera, after spreading terror and death throughout numerous towns in Scotland and England, reached Dublin about the latter end of March, and broke out destructively in Cork before

the middle of the ensuing month. From the miserable condition of the people in this country, their want of sufficient food and clothing, and their habits of living together in dense masses in the narrowest and filthiest streets of large cities, it was long anticipated that a disease, dreadful every where, would assume some new features of malignity, and leave a memory of its desolation amongst us, surpassing any thing that had been known of it in other kingdoms. This sad presentiment was very general, but in Limerick it was felt with peculiar force, from the conviction that the crowded state of the old town, the closeness of the streets and lanes, and the utter destitution of the great majority of its inhabitants, had no parallel within similar limits in any other city.

As Cork, perhaps, approached near to it in this respect, the greatest anxiety was evinced in ascertaining its daily mortality, and when, at the latter end of the month, the reports gave, as the hospital deaths alone, the frightful amount of fifty in one day, a sensation of mingled apprehension and astonishment pervaded all classes. People felt as if it was already at their doors; inquiries were daily and hourly made of the medical practitioners, whether a case had yet occurred, or whether there were any symptoms in the prevailing complaints which could be considered indicative of its approach, and a deep interest was now for the first time observable, in the general preparations to meet the calamity. Within ten or twelve days afterwards two persons died of the disease in Boherbuoy, and on the 26th of May eight more were admitted into St. John's Hospital, of whom seven expired within a few hours.

Very soon after their admission I went to visit them, in company with Dr. Geary, who was senior physician to the hospital. The outer gate was thronged with crowds of people, whose panic-stricken faces were sufficiently expressive of the occurrence of some catastrophe which had never been witnessed in their age before. The relatives of the sick were in the van of the crowd, lamenting loudly, and watching anxiously for the opening of the door in the portal, to make inquiries of those within. As we passed through the yard, silent fear was seen in every countenance; the servants, the nurses, the strangers, who had come in to assure

themselves of the presence of the disease, all looked anxious and depressed, as if some new mystery in human nature had just been unfolded to them.

The habit of meeting disease in almost every form that could awaken feelings of awe and terror in the mind, and a general immunity even in the midst of the worst contagion, protects the physician, in some degree, from those apprehensions of which others are so naturally susceptible. His interest in the study of the science to which he is devoted, his curiosity to witness a train of morbid phenomena without counterpart in any thing that he had yet seen in the diseases of the human frame, and perhaps a faint hope that he might light upon some remedy capable of arresting its progress or diminishing its fatality, tend also to divert his mind from those depressing reflections so universally consequent to the incursion of cholera. With all this, I must confess I felt a shock on entering the ward of St. John's Hospital, for which any thing I had heard of the disease had not prepared me. On the right as I entered were two young men, of robust frames, apparently asphyxiated; they had been, it was said, at their work in the morning, but they were now laid in the bed, cold and pulseless; their breathing oppressed, their faces shrunk and livid, their eyes with a dark areola around them, their hands and arms blue and sodden, and a clammy moisture exuding every where from the skin. To the left was a pale and cadaverous-looking figure, flinging his arms about, complaining of the burning within him, and craving for cold water. This was, indeed, the general cry amongst them when any one approached or passed the beds; and the deep hoarse whisper in which the supplication was uttered, was one of the most startling as well as characteristic symptoms of the malady. There were others vomiting, and some were screaming with the cramps, while the attendants were busily engaged in rubbing them. Altogether, a scene was presented to me of which any thing I had known in the practice of medicine, except from the direct administration of poison, had not the remotest resemblance. My mind was so strongly impressed with this conviction, that I could scarcely repress my surprise at the statements which had appeared from many physicians on the first irruption of the

cholera in England. It was said that it was not a new disease, and that it had prevailed in many places as an epidemic repeatedly before. There is not, perhaps, a single symptom attendant on it which may not have been met with in an isolated form in other maladies; but viewing them all connected with one another, as they were in cholera, their commencement, their progress, the frightful spectacle of disease they present to us in a few hours, and the rapidity of their termination in death, I venture to affirm no medical man has, in these countries, before witnessed their likeness, much less the identical complaint they constitute.

When the bodies were, one by one, taken out for interment the next day, the scene at St. John's gate was beyond description. Many more patients had been admitted during the night, and all the friends and relations of the sick, with crowds of persons attracted by intense curiosity, choked up the street. There was no ingress or egress except through the centre of a dense mob; and whenever the gates were opened, and a death was announced in reply to any inquiry, wild screams were heard along the market-place, mingled with the subdued cries of those who had learned the fate of their friends in the morning.

Towards the close of the day, however, the expression of complaint among the people was gradually altering its tone. Of the first seven who had been admitted, four or five had been already removed to the burial-ground, and but little hope was given to those inquiring respecting the rest. Such an unprecedented and rapid mortality was altogether unaccountable, and strong suspicions began to seize upon the minds of many, as it had done in almost every place which had been visited by cholera, that the physicians were poisoning the patients. At first, exclamations of astonishment were heard in the midst of the wailing as a coffin passed out, "What! no one recovering? all, all dead!" But as the belief grew stronger, little scruple was made of directly charging the medical attendants with the deaths which had occurred. It was not until long afterwards, when the character of the disease became thoroughly known to the poor in their own cabins, and under their own management, that they became convinced of the injustice of these ridiculous suspicions.

Although the two first instances of the complaint appeared in Boherbuoy, parish of St. Michael's, it did not spread in that district for some days. The cases already admitted into hospital were from the parish of St. John's, and seemed to have no connexion either with those that had first taken ill, or with one another. It now, however, burst out almost simultaneously in all quarters of the old town, and was so destructive in the parish of St. Mary, that it was apprehended it would become utterly depopulated. The published reports gave the hospital admissions to the 30th of May at fifteen or twenty a-day. On the 1st of June there were fifty-five new cases, and on the 10th one hundred and twenty-three, up to which period there had been altogether nearly six hundred in hospital, of whom two hundred were already dead.

Commodious houses were at this time opened in the several parishes for the reception of patients. The Christian Brothers or Monks' School was obtained for St. Michael's, a small house on the North Strand for St. Munchin's, and the school of the convent of St. Clare for St. Mary's. The last is one of the poorest parishes in the city; the population exceeds 14,000, and as they were for the most part panic-stricken at the number carried off in the first burst of the disease, and thus generally predisposed to the attack, even the spacious school-rooms of the nunnery became too narrow for the sick.

I had been appointed, with other physicians, to the Strand hospital, but as this was not yet prepared for patients, and the medical men at the nunnery were worn down with fatigue, I was requested by the Board of Health to relieve them. I shall never forget the impressions which that night's attendance left on my mind. Those who have never visited a cholera hospital during the early ravages of the disease, can form but a feeble idea of its horrors from the most vivid description. The field of battle at the close of day presents scenes, perhaps, as shocking; but the constant presence of the relations or friends at the bedside of the cholera patient, gives rise to many touching circumstances which can seldom occur at the lonely death of the soldier. I repaired to the hospital about eight or nine in the evening; the windows were bright with lights as I approached, but

there was a bustle within, a confused din of voices or cries, a trampling, and hurrying to and fro, that gave sufficient announcement of the fearful struggle with death which was going on.

A few miserable wretches hung round the gate; they seemed to have been there for the greater part of the day, and were only sobbing quietly; but on its opening as I entered, they started up, to make inquiries of the porter. The yard was strewn with straw; there was a heap of coffins piled at one side, against the wall, and the cholera cot was at the stair-foot, with a patient waiting to be removed up stairs.

As I ascended, the full horror of all I was to meet came upon me; the moans of the dying, the screams of those in cramps, the cries for assistance, the vomiting, the hiccup, the prayers for a drop of cold water, came mingling with the heavy sobs or unrestrained lamentations of the attending relatives. It was not inaptly said, by a friend who was with me, that it went nearer to realize one's imagination of the place of future torment than any thing he had ever witnessed. The apartment was long and lofty; the beds were arranged in three rows, and so crowded together that there was merely room between them for the attendants to move about. Here and there, from the walls and pillars, dingy lights gleamed across the ward, bringing into indistinct view the upturned ghastly countenances of those near the door. Further on, as the rays fell fainter and more obscured by the smoke and the steam of the water used in filling tins, little could be seen but struggling movements of the patients as they lay, or the figures of persons hurrying backwards and forwards from bed to bed; and yet onward, at the extremity of the room, all was mist and gloom, from the depth of which was repeated back, like distant echoes, the horrid sounds and cries that assailed me on entering. I was particularly struck by the first object I happened to encounter near the door. An old man, perhaps 80 years of age, lay in the nearest of the central beds; his head was unusually large, and his whole frame of colossal make, though gaunt and bare. He was partly bald, but whatever hair remained was snow white, and his thick grey beard, descending from an immense jaw some inches on the chest, gave to a visage,

otherwise sufficiently proportioned to the rest of the person, an appearance of gigantic magnitude. He was supporting himself on his elbow, after a severe fit of vomiting; his features were of a ghastly blue; a glairy fluid was flowing down his chin and beard, and his large, sunk, dilated eyes, were fixed and staring. He was evidently dying. This herculean form, which had resisted the vicissitudes of fortune and climate, and the attacks of all common diseases, for eighty winters, sunk in a few hours under the stroke of the pestilence. Within a few beds of him, as if in horrid contrast, a little infant, of a year old, was lying cold and pulseless. It appeared to be dead, but on my raising its little head, it started up and looked about for cold water, which it drank ravenously from the hands of the nurse. In a few moments it began to vomit; between each fit of which it looked about in piteous wondering, as if to know whether we could conjecture what was the matter with it. There was little time, however, for reflecting on individual portions of a picture which was altogether new and appalling. The physician I was come to relieve was in the middle of the ward, taking his round; the apothecary following him with a tray of medicine. He was now prescribing, now issuing orders, now crossing here and there, as the cries or calls of the sufferers demanded. At times all seemed in utter confusion, some running with sinapisms, others with tins full of hot water, others with liniments, and the doctor was frequently arrested by the friends of the sick, each endeavouring with the most earnest supplications to call his attention wherever they were most interested. "Doctor! my mother, my dear mother! she is gone, if something is not done for her."—"Oh, sir, what will become of my brother, with the cramps?—do look at him; one look, only one look!"—"Doctor, is my child to die; is nothing to be done for him? Heavens bless you for ever, sir, and give me something to stop the vomiting." With these distracting appeals were mingled the ever-during cries for cold water, or the screams of some miserable creature whose cramps had abated a little, to have the sinapism removed.

Occasionally the startling shout of "a case, a case!" reached us, from the door-way; a heavy trampling on the

stairs succeeded, and, in a moment after, a figure tottered into the ward, supported by the porters, with a countenance sunk and death-like, the lips blue, the eyes glassy, the voice hoarse and sepulchral. I saw a stout young man brought in in this state, and struggling violently with cramps, who had but two hours before, on the preceding watch, been refused admittance by the physicians, on the supposition of his not being in cholera: but even scenes like these soon grew common-place, and it was only where one's attention was caught by some extraordinary display of patience or passion in the passing tragedies, that they excited any deep interest. A mother's motionless figure, kneeling at the feet of her dead son, with hands clasped and tearless eyes, uttering, in whispering accents, "The Lord gave him to me, and the Lord has taken away;" or a daughter, with hysterical screams, claspings the limbs of her breathless parent, as the heavy steps of the porters coming to remove the body to the dead-house, were heard passing along the ward.

There had been a great mortality the night before in this hospital, and I believe about 37 patients were still living when I took charge of it. Feeling much indisposed and exhausted, I returned home at four o'clock in the morning, after admitting nearly forty new cases, and witnessing eleven deaths. There had now been 312 admissions since the commencement, and 111 deaths; and as the disease seemed still on the increase, and the sick were accumulating to so frightful an amount in the wards that it was impossible to attend to them, it was found necessary to remove the patients from the General City Hospital (Barrington's), to the County, to make room for the admission of cholera cases.

On the 12th it also began to spread rapidly in St. Michael's parish, where it had hitherto extended itself rather slowly. On the 13th the admissions into the hospital opened in the Monks' School were immense, and were chiefly from Little Anne-street and Market Alley. The cases in the other hospitals were also numerous, and those among the wealthier classes vastly exceeded any which had occurred in one day since the commencement of the pestilence.

At length the mortality seemed to

have attained its maximum, sixty deaths having taken place in and out of hospital within twenty-four hours. Nothing could now exceed the still and forlorn state of the city, or the dismay of its inhabitants. The deserted streets were white with lime, which was abundantly scattered through them, with the hope of decomposing the infectious poison in the atmosphere. Silent and almost deserted, they presented at noon, in the midst of summer, an appearance sometimes seen at day-dawn in the depth of winter after a hard night's snow; in itself sufficiently startling if only for its strangeness, but rendered infinitely more so by the gloomy countenances of the lonely individuals who now and then hurried past, or the solitary hearse moving slowly on, with no other attendant than the driver. St. Munchin's parish, at the opposite side of the river, had been hitherto comparatively free from the disease; but on the night of the 15th and 16th of June, as if it had suddenly crossed the river, a vast number were taken ill at Thomond-gate, and conveyed to the Strand Hospital, to which I was now attached.

For several days the disease spread extensively in this new direction. As the population of the parish, however, was small, and such as were unable to resist the pestilence were soon struck down, it declined almost as suddenly as it commenced, and on the 22d both the North Strand and Nunnery Hospitals were closed.

I was immediately transferred to St. Michael's Hospital, where I found the severity of the disease unabated. The deaths, which had from the beginning exceeded those of any other hospital, were still so numerous, and the supply of coffins so deficient, that the bodies of those who died during the night were often thrown in heaps in a corner near the gate, to await the arrival of the burial cart in the morning, or the claims of friends or relatives, who chose to inter the deceased themselves. It was a shocking sight to witness the business-like indifference with which the cotmen turned over these ghastly remains in search of any particular corpse, when it was demanded for interment, and from the recklessness of these men, and the confusion often incidental to sudden incursions of cholera, or to an unexpected mortality, mistakes no doubt occasionally occurred, and bodies were

consigned to the grave amidst the wailings of those to whom, while living, they were wholly unknown.

It was at this period I first had an opportunity of classifying the patients accurately as they were brought in, and noticing whether in the early or late stage of the malady the treatment adopted was more or less successful. At the termination of a month, by a simple calculation made by the clerk or registrar, I was furnished with inferences worth all the medical opinions which had been broached since the disease had reached these kingdoms, and only not in every respect indisputable, because not founded on more extensive premises. To these inferences I shall have to refer more particularly hereafter.

In the beginning of July there was a very perceptible decrease in the total amount of new cholera cases and deaths in the city; and towards the end of the month the complaint had nearly ceased. It had, however, so universally returned to every place once visited by it, that the people were for some time in no way disposed to feel at ease on the subject; and the result proved their apprehensions were but too well founded. On a beautiful warm evening in the latter end of August, when even the most apprehensive were beginning to forget their fears, the medical officers of Barrington's Hospital (the only one kept open for the disease) were a little alarmed on finding that two or three cases had been just brought into the house; and while they were yet conjecturing as to the probability of a new irruption, the tramp of crowding feet, and the well-known cries and sounds of cholera, were heard in the hall. Within an hour or two the wards were full; and as beds could not be procured with sufficient readiness, the patients were stretched on the floors side by side, with just room enough for the medical attendants to pass between them. Three-fourths of those brought in were pulseless, cold, and livid; and of 73 admitted on this and the succeeding night, 52 expired—a mortality far exceeding that at St. Michael's Hospital, which had previously occasioned very general remark. Nothing, indeed, seems so singular in this disease as the suddenness of its onset, and the apparently inevitable death of those first struck with it. This great mortality was even observable when it moved to a new street

or quarter of the city, as if it always assumed fresh energy when it changed its locality. The attack in this instance, though more violent than even on its first invasion, was happily of very short duration; and we had the happiness, within a few weeks, of declaring the city perfectly freed from the pestilence.

The total number of persons attacked in Limerick from the commencement of the epidemic until its termination, including private cases, at a rough estimate, was over 2500; the number of deaths about 1000. The population of the city, by the census in 1831, was 66,551; but it was supposed to be then very much underrated, and was more generally estimated at from 70,000 to 75,000. The hospital cases reported to the Board of Health to the 18th of July, when the complaint had nearly ceased, were 2125; the deaths 760. There were a considerable number afterwards on its second irruption in August, of which I have the reports only for the three first days, during which the admissions were 73, the deaths 52, as already stated.

[To be continued.]

ON

"REVERBERATION" OF THE VOICE IN DISEASES OF THE CHEST.

To the Editor of the Medical Gazette.

SIR,

I HAVE frequently noticed the fact, that a reverberation of the voice is communicated to the hand if applied to the chest of a healthy person during the time he is speaking; but the application of this circumstance as a diagnosis of disease did not occur to my mind until Dr. Chambers stated, while examining a patient at St. George's Hospital, that no "reverberation" of the voice was felt by the hand in cases of effusion of fluid into the chest. These may not be the precise words employed by Dr. Chambers, but the principle is correct. Since then I have made a few observations on this subject, which, if not very important, will probably be the means of calling the attention of others to a symptom which may often be found of some practical utility.

The word "reverberation" strictly means, a beating back, an echo; nevertheless, I will endeavour to explain, as clearly as possible, the phenomena to which this name has been applied.

If the hands be placed on either side of the chest of a healthy individual in the submaxillary region, while he is speaking, a tremulous motion is felt, as if the sound of the voice was conducted to the walls of the chest, and gave a mechanical stroke or impression to the hand*, just as the waves of air conduct sound up the meatus auditorius, and cause an impression on the auditory nerves; only in one case the sense of feeling, in the other that of hearing, is used. I have remarked, if the tone of voice be rather high, that the impression is not conveyed to the hands; and consequently in women, who generally speak in a higher tone of voice than men, the reverberation of the voice is not so frequently felt. If, however, much effusion of fluid exists in the cavity of the thorax, this impression is not propagated to the hands; or if fluid is contained only in one cavity of the chest, the reverberation is felt on the walls of the healthy and not on those of the affected side. I have also reason to believe, that in cases of partially condensed lung, the reverberation of the voice is much modified; and when one lung is entirely condensed, that no sensation of the voice is produced on the hand applied to the affected side.

I remain, sir,
Your obedient servant,
C. J. B. ALDIS, M.D.

13, Burlington-street,
Dec. 9, 1837.

PERFORATION OF THE STOMACH.

To the Editor of the Medical Gazette.

SIR,

MR. SAMUEL SANDYS has desired me to forward to you the inclosed case, and to request your insertion of it at an early time, as a companion to that of Mr. Prichard, of Leamington, published last week.

It occurred some months since in a patient attending our local dispensary,

* Any person can verify this fact on his own chest.

and is principally copied from notes taken at the time.

I am, sir,
Your obedient servant,
WILLIAM WHITEHOUSE.

4, Francis Terrace, Kentish Town,
Jan. 8, 1834.

A young woman, aged about 22, unmarried, of a pale and somewhat chlorotic appearance, yet naturally plump, and in *embonpoint*, requested my advice for a pain at the region of the stomach, accompanied by a loss of appetite (especially as regarded solid food,) with a sensation of weight and fulness after each meal. This distaste for solid food, and some other dyspeptic symptoms, have existed for some months.

The treatment adopted was cupping and blistering over the region of the stomach, the exhibition of the vegetable tonics with soda,—an occasional aperient,—and lastly, a course of steel. From none of these did she experience any marked benefit.

After persevering in this treatment for some weeks, she went out one morning, having taken a single cup of tea for her breakfast. She had proceeded about a mile, when she was suddenly seized with violent pain in the lower part of the abdomen, recurring in throes like labour pains,—so much so, that a stranger, into whose house she was taken, expected nothing less than an “accouchement” every moment. A chaise was procured, and she was quickly brought home;—my attendance was now requested, and I found her extremities cold, her pulse 120, low and feeble; there was constant urging to make water, a continual retching, with ineffectual efforts to vomit; great tenderness of the abdomen, the pains increasing in violence, in duration, and in frequency, till at length they became incessant, with a most urgent and distressing bearing down of the womb.—The bowels were moderately open.

Bleeding being contra indicated by the extreme lowness of the pulse, and the great prostration of strength, a full dose of *Liq. opii sedat.* was given, and warm fomentations sedulously applied to the abdomen.

From these measures she seemed to experience some benefit, the retching being moderated, and the pain somewhat relieved; but in the evening she

became thirsty, and indulged largely in lemonade, when the previous symptoms returned in all their violence. Her thirst now became incessant, and she drank at least five pints of acid drink during the night. I was summoned at 7 o'clock in the morning, but before I arrived she had expired, about twenty-one hours after the occurrence of the first alarming symptom.

Examination about eleven hours after death.—The body, externally, was plump and well-formed. There was evident fluctuation in the abdominal cavity. On making the first incision through the skin, there appeared a layer of fat at least three-fourths of an inch in thickness over the whole of the abdomen and thorax: the cavity of the abdomen was filled and distended with a mixed and turbid fluid, to the extent of more than a large wash-hand-basin full. —There was a circular perforation of the stomach, of the size of a shilling, in the lesser curvature, about midway between the cardiac orifice and the pylorus; the edges of this were smooth, stiff, and almost cartilaginous; it had evidently been, and was still, partially adherent to a thickened and solidified portion of omentum. There was no appearance of ulceration on the internal surface of the stomach, and no other apparent lesion of its mucous coat, if we except two or three minute points of a vivid scarlet near the cardiac orifice. The intestines, in several parts, were slightly adherent, from the effusion of coagulable lymph.

Such other parts as were examined, presented no unusual appearances.

REMARKS CONNECTED WITH MIDWIFERY.

To the Editor of the Medical Gazette.

SIR,

BE kind enough to give insertion, in your valuable periodical, to the following observations, in answer to the remarks of Mr. H. Coles, which appeared in a late number of your journal.

It is well known to every one acquainted with the principles of midwifery, that women have from time to time been delivered in almost every

position that could be mentioned, and frequently in very odd ones, from their inability to change it on the unexpected occurrence of *spontaneous evolution* supervening. Though women have from necessity or superstitious habits, preferred being delivered in certain positions rather than others, yet that never can be a reason for continuing the practice in opposition to all delicacy and mature reflection, neither can the vague and indefinite remarks of Mr. H. Coles be a motive for changing the position at the period of parturition, from the left side, which is the established, to the right. There are many special reasons of the highest importance for delivering on the left side. First, it is the most becoming and suitable, both for the patient and accoucheur. Secondly, it is the best for making examinations (in all common cases), applying instruments or performing operations, without the patient seeing either the practitioner, or what he is about, which is of the greatest consequence. Thirdly, it is superior to every other for the application of manual aid for the safety and support of the perineum during the second stage of labour, a circumstance which all must admit to be of great importance, frequently calling for our undivided attention. Fourthly, it is the position of all others in which a woman can be delivered in accordance with that high and sensitive feeling of delicacy which at all times, and more particularly at this, pervades the female breast.

I think it would be absurd going on multiplying proofs of the advantages of the left side in preference to every other position, as I imagine if what I have said does not convince, nothing would. The experience of ages, and unanimous consent of all the professors and teachers of the obstetric art, after repeated discussion and mature deliberation, have been in favour of the left side. If there was nothing else, the *relative position* of the attendant and patient condemns and forbids us ever once delivering on the right side, if it can be avoided. Rare cases, no doubt, do occur in which the right side holds out advantages, but these are the exceptions to the rule, and will not happen more than once in five or six hundred cases. There are also forceps cases where the back will be found the best position, particu-

larly where the difficulty arises from extreme rigidity of the external parts in the first labours of women advanced to the age of forty or upwards.

INTRODUCING THE FEMALE CATETER.—This is an operation that ought to be performed with great gentleness and courtesy, neither doing any thing rude, nor making use of any expressions that would wound the most sensitive ear. One attendant is enough to be present. The room ought to be darkened; the curtains of the bed drawn, and the patient's face covered. The back, with the thighs drawn up, is the best position, next to which is the left side. It is the only operation in which you require to make your examination from before: laying your hand gently on the hypogastric region, feel the distended bladder, and make some inquiries about the seat of the pain, to carry off the alarm and attention of the patient: take care you use no pressure on the distended organ, as it gives rise to acute pain: pass the index finger of the left hand down over the pubis, between the nymphæ; carry it down in a perpendicular direction, about an inch below them: at this point, and immediately above the orifice of the vagina, you will feel the meatus of the urethra in the shape of a small protuberance resembling a flattened pea: having placed your finger on the meatus, carry the catheter in your right hand *to it*, your finger will then be able to direct it into the urethra without the slightest difficulty, if you hold it in the proper direction, with the other extremity pointing up over the pubis, towards the umbilicus, having the catheter either armed with a bladder or a vessel applied to receive the urine on the bed, opposite the right groin, which is the most depending and convenient direction for the escape of the urine, without either wetting the female or soiling the bed, which ought never to occur, as it can with ease be avoided. You will be able to tell by the length of the catheter introduced if it has reached the bladder. The urethra is about an inch and a half long; never more than two, except in extraordinary cases, where the bladder has been pushed up, and the urethra stretched, distorted, or forced to one or other side, out of its natural course, by the pressure of the gravid uterus and distended rectum behind it. In some of

these extreme cases great difficulty is experienced, and sometimes all our efforts fail in introducing the catheter. The plan I have found to succeed best in these cases is simply to empty the rectum by the gentlest means, then raise the nates and lower the shoulders of the patient considerably, to allow the pressure of the distended uterus to fall back off the urethra; you will then be able to pass the catheter with comparative ease, and relieve the patient from excruciating torment. If the case is very bad, keep in the catheter for some time, and do not empty the bladder all at once, but by degrees, or very slowly, stopping a little now and again, that it may contract upon itself. If bland, or comparatively mild injections, thrown up, do not empty the rectum, the scoop will be found the most convenient. I have said little of the position of the patient during the operation of introducing the catheter, as in all ordinary cases the accoucheur ought to be able to perform the operation with facility, ease, and delicacy, in any position which he may chance to find her in, as it ought to be done with the least possible preliminary ceremony, as it would multiply her fears, increase her alarm, and make the operation appear doubly terrible to her already excited imagination.

No one who has studied the anatomy and relative position of the parts, and practised sufficiently on the dead subject, will feel any difficulty: practice itself will never teach a man to pass the catheter with grace and gentleness without the above-mentioned preparatory study and practice: certainly no one is justified in exposing the patient, and still less in looking, and then apologising by saying she was not conscious; just as if it was possible, which I maintain it is not, for the accoucheur to tell whether the patient was or was not conscious of being exposed. Women do not make known all the wounds they receive on those occasions from imprudence. Mr. H. Coles, I perceive, has forgot to inform us of the length of his catheter, as well as the direction he makes it assume, whether between the abdomen and thigh, over the acetabulum, or down between the legs and round the nates. I am inclined to think his instrument must be very long, if it reaches from the interior of the bladder

out of the bed into a receiver, which he leads us to believe.

Your obedient servant,

W. H. M'IVOR.

14, Harley-street, Dec. 8th.

MEDICAL GAZETTE.

Saturday, January 13, 1838.

"Licet omnibus, licet etiam mihi, dignitatem
Artis Medicæ tueri potestas modo verendi in
pulchrum sit, dicendi periculum non recuso."

CICERO.

CASE OF SUPPOSED IDIOCY.

THE difficulties which we meet with at every step in forensic medicine do not differ in kind, and perhaps not even in degree, from those which are found in the therapeutic division of our art. The practical difference in the difficulties of the two branches seems to be, that in the one where law and medicine come in contact, each slight distinction is of importance; whereas, in the practice of physic, though the same shades necessarily exist, they are ranked together under a few bold divisions, and the minor varieties are passed over. Thus, to take an instance, scarlet fever may vary through a thousand grades from the most malignant forms of the disease to perfect health; and it would surpass the skill of the most acute physician to decide which are the precise forms where danger begins, and, on the other hand, which are those that can become fatal, as Sydenham expresses it, only by the *nimia medici diligentia*. Nicety of distinction, however, is not here of much importance; the milder cases, in this, as in other diseases, recover under any, or no treatment: and the only inconvenience resulting is, that the medication which allows the lesser degrees of the malady to get well, is often spoken of as having a really curative power. Examples of this error will occur to every one among the thousand-and-one remedies

extolled in the treatment of Asiatic cholera. But let us suppose that the possession of a civil right depended on the claimant's being affected with scarlatina or not; how nice and how disputable would be the line drawn between the slightest case of the disease, and perfect health! How many and how vain would be the attempts to distinguish between some varieties of scarlatina and certain other eruptions! Who would be able to settle whether roseola is scarlatina at a minimum?

The circumstances under which these fine distinctions are every day laid down by one party, and contradicted by another, is unsoundness of mind; for here the boundaries of health and disease are as undefined as in either of the maladies we have mentioned, but with this additional circumstance, that to decide whether the supposed patient is one or not, is often to decide whether he is to enjoy his property at large, or be a prisoner for life.

Hence the repeated but vain attempts which have been made to define unsoundness of mind with precision, and hence the conflicting verdicts of juries in commissions *de lunatico inquirendo*. A remarkable instance of this kind took place in Scotland about a year ago, of which an elaborate report is now before us*.

The circumstances of the case were as follows:—

David Yoolow, the son of a farmer, at Mill of Peattie, in Forfarshire, was struck with paralysis, at the age of nine years, from which he never altogether recovered. From this time to the holding of the inquest, a period of about forty-three years, he had led the life of an invalid. He spent his time in his room, where his chief employment was

reading the Scriptures, though he occasionally had recourse to other religious books, and even glanced at the newspaper, in which, however, he did not seem to take much interest. His extreme debility hindered him from personally superintending the concerns of his farms, and his knowledge of rustic matters was, of course, far from deep. Such as he was, however, he seems to have been a man of plain common sense, not wholly indifferent to what was going on around him; and considering his very limited opportunities of acquiring information, he answered many of the questions put to him with something more like shrewdness than idiocy.

In England, says Blackstone, a man is not an idiot if he has any glimmering of reason—if he can tell his parents, his age, and the like common matters.

Similarly in Scotland, those are idiots, says Erskine, “who are entirely deprived of the faculty of reason, and have an uniform stupidity and inattention in their manner, and childishness in their speech, which distinguishes them from other men; and this distemper of the mind is commonly from the birth, and incurable.”

With this definition by the highest legal authority staring him in the face, how any one could have ventured to assert that David Yoolow was an idiot, is almost inconceivable.

We find in Mr. Colquhoun's book, that a remedy is provided by the Scottish law for those cases where persons are not idiots, but from profuseness, or the too great facility of their tempers, might be too easily induced to make hurtful conveyances. This remedy is called an interdiction, and makes all deeds voidable which are subsequently executed by the party interdicted, affecting his heritable estate, without the consent of the interdictors, “unless it shall appear that such deeds have truly

* Report of the Proceedings under a Brieve of Idiocy, Peter Duncan against David Yoolow, tried at Compar-Angus, 28—30 Jan. 1837. With an appendix of relative documents, and an introduction, by Ludovic Colquhoun, Esq., Advocate. Edinburgh, 1837. 8vo. pp. 116.

been of an onerous and rational character, and that the grantee has suffered no prejudice through them."

Whether such an interdiction would have been thought sufficient for the protection of the property of Yoolow we know not; but it is clear that the evidence would not have borne out the jury in laying even this limited restraint on him; nor does it appear, indeed, that there was the slightest danger of his losing his property in any manner.

This inquiry into the state of mind of David Yoolow was held at Coupar-Angus, in Forfarshire, Jan. 28—30, 1837; the question proposed to the jury being the supposed idiocy of the defendant. The proceedings were opened by speeches from the counsel for the plaintiff and the defendant, after which Yoolow was visited at his dwelling by the Sheriff, accompanied by the jury and the counsel. The answers given by Yoolow to the questions which were put to him might alone have been sufficient to settle the point of his imaginary fatuity. "Being asked whether he read the newspapers? he answered that he did—the *Weekly Journal*. He was asked what it was that he read in the newspaper? he answered that it was the price of grain. Being asked what is the price of wheat? he answered, thirty-eight shillings a quarter. Being asked how much is in a quarter of wheat? he answered, eight bushels. To the question whether he read his bible? he replied that he did. Being asked if he understood what he read? he answered, "I think I do." He was asked whether the soul perished with the body? he answered that it did not. Being asked what became of it? he answered, "If you are good, it goes into happiness; if not, into misery." He was asked how many ploughs he had labouring, and how many horses? which he answered correctly. Being asked who were his

sister's trustees? he named them all. Being asked what he would do with his money at his death? he answered, "I cannot take it with me." He was asked, if he were dying, would he not think it right to leave some part of his money to his relations? he answered, "Part of it." He was asked, if a deed were offered him to sign, giving away his property, would he sign it? He said, "I should like to know what was in it*."

We can only say, that if these are the answers of an idiot—nay, even of one against whom "a brieve of idiocy" could with any decency be levelled, that we know plenty of people who might shake in their shoes; and that the whole population of the country might be divided into juries, and defendants accused of idiocy.

Some of your signers of blank acceptances might take a lesson from David Yoolow's proviso—"I should like to know what was in it."

The first witness on the plaintiff's side was Dr. Christison, the author of the treatise on poisons. He gave an account of the questions which he had put to Yoolow, together with his answers, from which he inferred that he laboured under a great degree of imbecility, and was unable to manage his affairs. Many of the questions put by the learned professor were very strange, if we consider them as addressed to a supposed idiot;—*e. g.* "Although you do not meddle with politics, there are some branches of them which, as a farmer, you should know about; for instance, what is your opinion of the Corn Law Bill?" He answered, "I ken naething about that." I asked "What is it intended for?" he answered, "To sell the corn, I fancy†."

If he had read all the folios that have been put forth by committees on the

* Report, p. 11.

† Ibid. p. 13.

corn-laws, he could hardly have given a more shrewd reply.

When asked "if he knew who was prime minister just now, he answered, "No;" and this we suppose was considered as a proof of fatuity; though even the counsel for the plaintiff acknowledges in his second speech that "it is often difficult to answer who is prime minister. Whether it be Lord Melbourne, or the Duke o' —,* or any other O', may be at times a puzzling inquiry to wiser men than Yoolow†.

Dr. Christison says, "an uncommon degree of memory sometimes accompanies persons of unsound mind, particularly memory of the contents of the Scriptures, which I was informed Mr. Yoolow possessed. It was partly on that account, and partly because I was otherwise satisfied that he was of unsound mind, that I refrained from asking him any questions upon the subject of the Scriptures‡.

In the celebrated case of Miss Bagster, on the contrary, Dr. Monro deposed that "many insane persons have good memories, but I have not known imbecile patients have good memories§."

Almost the only answer in which Yoolow apparently broke down was the following one:—"I asked him how much wheat should be used per acre in sowing?" To which he answered, "a boll." "Then I asked him what would be a good return?" He answered, "Twa (two) bolls;" and this he repeated on being asked the question again, and said "it would be a very good return||."

A return of two for one would certainly not be considered good by any one acquainted with agriculture; but the constant mistake committed by the

witnesses for the plaintiff throughout this inquest was to suppose that Yoolow was a farmer; whereas he was a trembling invalid confined to his room by incurable disease, and though in possession of several farms, he necessarily transacted everything by deputy. Hence he could hardly be expected to have a ready acquaintance with these matters; far less could such an answer be considered a proof of fatuity.

Moreover, it was suggested by his counsel, with great probability, that Dr. Christison had misunderstood the answer, and that Yoolow had really said "twal bolls," *i. e.* twelve bolls, though the Professor fancied he said "twa bolls," *i. e.* two bolls*.

We shall give the remainder of this interesting trial, with some further observations, next week.

CLINICAL LECTURE

ON

THE VENEREAL DISEASE.

Delivered at St. Bartholomew's Hospital,

BY WM. LAWRENCE, ESQ. F.R.S.&c.

THEY who wish to learn the nature of the venereal disease may choose between two modes of proceeding. One is, to read books on the subject; the other, to enter the wards of an hospital, and study the facts there presented to observation, with a mind as free as possible from doctrines and preconceived notions. The two methods ought to coincide; but they differ widely in their results. There cannot be the slightest hesitation in determining that the latter is preferable for the purpose of gaining correct knowledge. If you expect to find what you read confirmed by what you will observe for yourselves in this hospital, you will be disappointed: the two modes of study do not differ more essentially than the conclusions to which they respectively lead. The statements in books are to the effect, that there is a certain virus or poison, which, when applied

* A witness "asked him, at different times, who was prime minister, when he said, 'The Duke o' —,' and then looked to Charles Scott; but on the last visit he told me it was Melbourne."—Report, p. 25.

† Report, p. 56.

‡ Report, p. 15.

§ MED. GAZ. Vol. x. p. 523.

|| Report, p. 13.

* At least three Englishmen out of four, in pronouncing French, confound the sounds of the *eu* and *ou*. Hence, if a Frenchman says *deux sous*, they are apt to mistake it for *douze sous*, or *vice versa*, which produces exactly the same misapprehension as the one in the text.

to the human body, produces effects so well marked in character, and so regular in progress, that we may by them easily distinguish syphilis from all other forms of disease. Thus, we should expect to find the great pox as definite in appearance and course as the small-pox or cow-pox. Again, it is represented that other morbid phenomena, apparently produced in the same manner as this supposed regular syphilitic disease, are not venereal, do not result from the application of a poison, and require a different mode of treatment from true syphilis.

When, on the contrary, we look over the several cases presented to our observation in a large hospital, we see affections very different from each other in appearance, and in other essential characters, yet coming under the common denomination of venereal disorders. Different as these are, they seem all to arise from a common source; they owe their origin to promiscuous sexual intercourse, and are, therefore, justly denominated *venereal*. I know no reason why any one of these varieties should be called *true syphilis* rather than any other. We may, therefore, abandon the attempt to distinguish between syphilis and the diseases resembling it, and discard from our vocabulary the terms *true syphilis* and *pseudo syphilis*, and the others founded on the hypothesis just noticed; terms which have either no clear meaning or an erroneous one, and are only calculated to introduce confusion and obscurity into a subject sufficiently difficult in itself. If, then, you wish to learn the nature of syphilis, dismiss from your minds the speculations of authors, and observe the phenomena, progress, and treatment of the venereal cases in this hospital.

I now direct your attention to the case of William Smith, a tailor, in Lazarus ward, who has already been in the hospital more than once. This is a well-marked example of phagedenic venereal disease, and it shows a correspondence in character between the primary affection and its secondary or constitutional consequences.

He was first admitted into the hospital on the 19th of May, 1836, with a phagedenic ulcer of the prepuce, by which the upper half, or two-thirds of the part, had been destroyed to the basis of the glans. The ulcer had an irregular ragged surface and edge, small portions of the latter being livid; there was no appearance of reproduction. The discharge was thin and ichorous; not abundant nor offensive. There was not much pain. The ulcer had existed eight weeks, during which time he had taken twelve pills, and employed a lotion, which caused considerable pain. A few large brownish-red cutaneous tubercles had appeared recently on the fore-

head and face. He was directed to take two grains of calomel with one-third of a grain of opium, three times daily, and to apply the black wash to the sore. He continued this treatment till the 6th of June, when the mouth had become considerably affected: he then left off the calomel and opium, and took five grains of the blue pill every night. During this time he was kept in bed, and confined to milk diet. Under this plan the ulceration was quickly arrested; and the sore, which had been spreading for eight weeks, healed rapidly: the tubercular eruption disappeared from the face.

The original sore, however, did not heal completely at the middle of the dorsum penis, where the ulceration had extended a little under the integument: it continued to burrow in this direction after it had cicatrised soundly elsewhere, and at last extended so far that it was necessary to lay open the undermined skin by a free incision on the 13th of June. The unhealthy surface thus exposed was dressed successively with black wash and with the balsam of Peru. The sulphate of quinine, and the compound decoction of sarsaparilla, were administered internally. The patient left the hospital in the middle of July, freed from the local complaints, and in excellent health.

The case, thus far, shows the natural progress and the destructive effects of phagedenic ulceration, when unchecked; for, if we suppose the twelve pills taken before admission to have been mercurial, they had produced no sensible operation on the system. It shows that phagedena, although essentially destructive, eating away the affected part by ulcerative absorption, as its name implies, is not necessarily active in its progress. It had here gone on uncontrolled for eight weeks, and merely destroyed a portion of integument. The effect, indeed, would have been more serious, if the disease had been seated in the glans. The present case is an example of what might be called chronic phagedena. It demonstrates further the powerful influence of mercury over the disease, and its unequivocal instrumentality in effecting a cure. Some have believed that the use of mercury is capable of giving the phagedenic character to an ulcer. Here, however, the primary form of the complaint was phagedenic: this character rapidly disappeared under the use of mercury, being succeeded by a healthy restorative process.

W. Smith returned to the hospital on September 10, 1836, with a relapse of ulceration on the penis: I was absent from London at the time, and he consequently came under the care of my colleague, Mr. Lloyd. I understand that the

whole dorsum of the penis was occupied by a foul phagedenic sore of circular figure, which extended a little in the direction of the pubes. The surface was so foul, and it had extended so rapidly, that Mr. Lloyd had thought it proper to apply the strong nitric acid. The sore, when I saw it, was covered by the brown crust which follows the effective application of that escharotic. The separation of the eschar was followed by a clean granulating surface, and cicatrisation was soon completed. On this occasion no mercury was used, either externally or internally, and the patient was discharged perfectly well in October.

On January 6, 1837, he was again admitted with phagedenic ulceration of the fauces and face, and inflammation of the periosteum. The *velum palati*, *uvula*, and tonsils, the upper and back part of the pharynx, were occupied by an irregular ulceration with a lardaceous surface, ragged edge, and bright red margin; deglutition was performed with difficulty and with great pain. There were three or four circular phagedenic sores on the face. There was a soft fluctuating swelling over the left superciliary ridge, and another on the os male of the same side. The carpal extremity of the left ulna presented a large painful swelling of the periosteum. All these affections were of painful character; they had interrupted rest, and impaired appetite, causing emaciation and great weakness. These evils were aggravated by great alarm respecting the nature and progress of the disease, and depression of spirits. I found, on inquiry, that he had never been affected with syphilis before; that he had been of regular habits; and that he had been particularly careful of himself since his first admission into the hospital. The painful nature of the symptoms, and the enfeebled condition of the patient, required narcotics, and strengthening means, both medical and dietetic, and presented a strong contra-indication to the general use of mercury, although the influence of that remedy was required to check the progress of disease, particularly in the throat. He was ordered to fumigate the throat with cinnabar every night and morning; to take the concentrated compound decoction of sarsaparilla three times a day, and 5 gr. of the *pil. saponis c. opio* at night; and to feed on milk and broth, or on meat, according to his powers of swallowing. On the 9th eight ounces of port wine ordered for him daily, and the dose of the *pil. saponis c. opio* was increased to 7½ gr. On the 24th the fumigation was continued once daily, the month having become sore, and linetus was ordered for a cough. Under this treatment, the local symptoms and the health

improved rapidly; the ulceration of the throat lost its phagedenic character, and soon healed; the ulcers of the face cicatrized; the swelling of the ulna disappeared, and those of the frontal bone and os mala broke and discharged, the openings subsequently scabbing over. With the abatement of pain the rest and appetite returned; the flesh and strength were restored; and the patient left the hospital in the middle of February, not only free from disease, but stout and in excellent health.

On this occasion mercury was only used locally, viz. in the form of cinnabar fumigation; but, as it happens not unfrequently under such circumstances, the specific action of the remedy on the constitution was produced, and pytalism ensued. The rapid cicatrization of the ulcers in the fauces and in the face, and the corresponding dispersion of the periosteal swellings, were probably owing in great measure to this operation of the remedy. The soothing and restorative medicine, and the generous diet, which were necessary on account of the pain, emaciation, and weakness, were advantageous in enabling the patient to support the action of mercury.

Smith seemed completely restored when he left the hospital; but the cure was not yet permanent, for he is now again in Lazarus ward, affected with disease of the nose, ulceration of the throat, enlargement and induration of the right testicle. He came in on the 23d of this month (June.) There is a large ulcerated opening in the septum narium, forming a free communication between the two nostrils, both of which are in great measure blocked up with bloody scabs and stinking matter. A most offensive fœtor is diffused to some distance round the patient. The bridge of the nose has partially sunk, and has become turned to one side. The integuments are swelled, of dull red colour, hot, and painful. There can be no doubt that the bone is diseased, from the yielding of the septum, the offensive stench, and the inflamed state of the external coverings. The affection of the mucous membrane is probably secondary; the seat of disease in the pharynx is its upper and back part, so high up that the lower portion only of the ulcer comes into view; it has the same phagedenic character as on the former occasion. This position of the ulcer in the pharynx, if not peculiar to phagedenic syphilis, is very common in that form of the disease; the testicle is moderately enlarged, hard, somewhat irregular and knotted, painful on pressure; the scrotum is red, and rather warm.

The remedies ordered were, solution of corrosive sublimate (half a grain to the

ounce) in lime water, to the nose, previously clearing it of scabs and matter by means of tepid water; five grains of hydriodate of potash in an ounce and a half of compound decoction of sarsaparilla, three times a day; mercurial liniment to be rubbed on the testicle. The patient has now been in the hospital a week under this treatment, and already feels much better.

The foregoing is a striking case of primary phagedenic ulcer, followed by the constitutional effects which are observed in this form of syphilis more particularly, viz. tubercular eruption, and phagedenic ulcers of the skin, fauces, upper and back part of the pharynx; disease of the nasal bones and membrane, and affection of the testicle. It shews the rapid succession of the various secondary symptoms, which is usually observed in this form of the complaint.

Mercury is of great service in the treatment of phagedenic syphilis; but it must not be employed indiscriminately. If we proceed on the old notion of its being the specific and sole remedy for such complaints, employing it freely at first, recurring to its use when the disease appears again, and so on, *toties quoties*, we shall seriously damage the constitution, aggravate the patient's sufferings, and increase the liability to relapse. The disease itself is of painful nature; it causes loss of flesh and general depression: repeated and long courses of mercury act like a poison in this dilapidated state of health. It is necessary to check the destructive progress of the primary affection; and this object sometimes cannot be accomplished without the full action of the remedy on the month. Its local administration is of great service in phagedenic secondary symptoms; but I avoid, if possible, its internal use, especially when the powers of the constitution have been impaired by the painful nature and the duration of the disease.

[Smith remained in the hospital four or five weeks on the last occasion. Before he went out the whole septum narium came away in several pieces. The discharge ceased, and the nostrils became clear, but the deformity of the nose was increased; the throat soon got well, and the swelling of the testis subsided. He showed himself at the hospital from time to time during the autumn, continuing free from disease and in excellent health. It may be concluded that he continues well, as he promised to return in the event of any relapse.]

There are at present other cases in the hospital, illustrating the various effects produced by what appears to me a common cause, promiscuous sexual intercourse;

and clear evidence to the same point may at any time be obtained by looking over the patients in any one venereal ward.

James Wood, admitted June 15, has six or eight small circular sores on the lining of the prepuce, at its orifice. These sores are a little elevated, so that the entire ulcer is in slight relief; they are of florid red. This is a common form and situation of primary syphilis. The sores, which are numerous, present a little ulcerative excavation; they then become raised as you see them in this patient, and of florid colour, and subsequently cicatrize. From four to eight weeks not unfrequently elapse before the healing process is completed. This must be the kind of affection which has been denominated *ulcus elevatum*. Remember that this elevation belongs to one period only in the progress of the affection. These cases shew that excavation and a foul tawny surface are not essential characters of a syphilitic sore; also, that primary sores may be numerous as well as single.

Thomas Haley, in the same ward (Lazarus), has a sore, not excavated, seated on an indurated basis, at the root of the prepuce, just behind the glans. The induration, as large as a horsebean, feels like a lump of cartilage under the skin. In this and the preceding case black wash is applied to the part, and five grains of blue pill are taken twice daily.

Mr. Hunter's description has led to the belief that a venereal sore is circular, excavated, foul or tawny on the surface, and with an indurated base and edge. A little observation will be sufficient to shew that the latter character is not essential, and that it is not seen in the majority of venereal sores. The subcutaneous hardness, which is certainly very peculiar in indurated chancres, depends in some measure on the texture of the affected part; its development requires the existence of loose cellular tissue under the skin. It is not seen in ulcers of the glans, where that tissue exists sparingly: we meet with it in the prepuce, and especially at the angle between the lining of the latter and the body of the penis, where the cellular tissue is abundant. An ulcer sometimes occupies the corona glandis and the basis of the prepuce, in which case the preputial portion of the sore may be indurated, while that on the glans possesses no such character.

Another patient in Lazarus has a sore as large as a sixpence on the integuments of the prepuce. The ulceration is superficial, with a clean smooth surface, affording a thin discharge in small quantity, which concretes into a slight closely-adhering scab. The margin of the sore is a little raised; there is a bubo in the right

groin, while the sore is on the left side of the prepuce. This occurrence of glandular affection on the side opposite to the ulcer is occasionally noticed. This case is treated in the same manner as the two preceding.

Thus these four cases of disease arising from promiscuous sexual intercourse, present to your observation four primary affections essentially distinct; and I know no reason why one of these should be called true syphilis more than the others. Hence syphilis, whatever may be the nature of its unknown cause, and whether that cause be one or many, produces phenomena considerably varied. They who have been endeavouring to find out true venereal disease, in the expectation of finding, as in small-pox or cow-pox, an affection of determinate character, commencing at a regular period, running a certain course, lasting a certain time, and coming to a definite conclusion, have been pursuing a chimera; they have been seeking for what does not exist. The whole phenomena of syphilis, the characters of the primary disease, the constitutional effects, the duration of both, the intervals between the primary and secondary affections, and between the successive manifestations of the latter, present numerous and striking differences.

FURTHER REPORT OF

CASES OCCURRING IN THE CLINIQUE OF M. LOUIS,

At the Hôpital de la Pitié, Paris.

By HENRY CURLING, Esq.

CASE VII.—*Typhoid Fever with Eruption— Recovery.*

A YOUNG man, aged 23, of a strong constitution, with black hair and eyebrows, well developed, came to the hospital March 9th. For six or seven days previous to his illness he had had diarrhoea, but had paid no attention to it. He has been obliged to keep his bed the last fortnight. His illness commenced with shivering, cough, and headache, followed by excessive heat and great prostration of strength. The diarrhoea has continued except during the last two days; the fever has not ceased, but he has had no return of the rigors. Anorexia and tinnitus aurium are to be added to the symptoms. The sight has been troubled, and he has felt giddy; but

he has had neither pain in the abdomen, nor epistaxis.

March 10th.—Lies on his back; face covered with red spots, and has an expression of anxiety; answers questions rapidly and sensibly; sight troubled; eyes injected; nostrils dry; feels giddy; tongue dry in the centre, moist and natural in its circumference; no meteorism; no gargonillement in the right iliac fossa. The chest, axilla, and abdomen, covered with numerous vesicles, half a line in diameter; perspired a little in the night; several rose-coloured spots are seen on the chest and abdomen; pulse 96 yesterday evening; 100 this morning. A bitter taste in the mouth; no nausea; respiration vesicular; much thirst.

Antim. Tartariz. gr. j. in die sumend.

11th.—Countenance less injected; has not vomited; pulse 92 yesterday evening, 88 this morning; tongue dry in the centre, but trembles less; tinnitus; fresh vesicles on the chest and neck; desquamation of the skin; four stools; spleen cannot be distinguished.

Eau de Seidlitz, half a bottle to be taken during the day.

On the 14th the sudamina were still more numerous, and confluent, particularly about the axilla. The tongue began to clean, and to become moist.

On the 17th he felt a little appetite, and from this period he became rapidly convalescent.

REMARKS.—The last three cases afford examples of two diseases, enteritis and typhoid fever, which by inattentive observers are often confounded. Though several symptoms of typhoid fever were absent in the last case, those that were present were sufficient to announce its nature. The diarrhoea and excessive prostration of strength at the commencement, the headache, the giddiness, and tinnitus, the presence of sudamina and rosé-lenticular spots, could, when combined, be attributed to no other disease.

In like manner, in the second case, the nature of the disease was denoted by the very numerous stools, the little fever, the pulse being only 50; the little prostration of strength (for although the patient ceased working the second day, he never kept his bed); the advanced age of the patient (53); the absence of giddiness, tinnitus, and spots. These symptoms are only observed in enteritis. Pain in the abdomen and diarrhoea, though sufficiently frequent in typhoid fever, are still more constant in enteritis. Headache occurs in forty-nine cases out of fifty of fever, but in not more than two of the same number of

cases of enteritis. Among a great number of cases of enteritis observed by M. Louis, only one quitted his work the day of the attack, while the contrary happens in typhoid fever. The rosé-lenticular spots, the sudamina, epistaxis, meteorism, are rare in enteritis; not more than one in twenty-three had epistaxis. Vomiting is also rare in enteritis; but it does not often occur in fever before the eighth or ninth day, the period when most of the complications of fever are developed. Enteritis may occur from infancy, may be repeated several times, and may complicate other diseases. Typhoid fever rarely attacks very young or very old individuals. When M. Louis wrote his work on Fever, 25 was the mean age of the cases he had observed. From a more recent analysis of a still greater number of cases, he finds 23 to be nearer the truth. He has never seen it after 60 years, and very rarely after 50. M. Louis has only seen five fatal cases of enteritis; and, considering its frequency, he does not view it as a dangerous disease.

Though it is impossible to confound well-marked cases of enteritis and typhoid fever, yet as a disease is more denoted by the combination of certain symptoms than by the presence of any one particular sign, so, when some of these symptoms are absent—when they occur in a different order, the diagnosis is often rendered difficult. This occurs in enteritis and typhus fever, slight cases of which are often confounded. And the distinction between them is more important than is generally thought, particularly as regards the prognosis. All the cases of perforation of the intestines which have fallen under M. Louis' notice were at the commencement very mild, and comparatively trifling.

In the first case the diagnosis was doubtful: the disease commenced by a shivering fit, fever, diarrhœa, pain in the abdomen, succeeded on the second day by epigastric pain and vomiting, and on the fourth by a little catarrh.

The diarrhœa, the depression of strength, and the vomiting, would seem to indicate fever, whilst the slight headache, the absence of giddiness, of spots, and of pain confined to the right iliac region, would rather indicate enteritis.

A white exudation on the mucous membrane of the mouth was considered by the ancients as a very fatal sign; but that it is not always so the first case proves. It certainly is only developed when the patient is very weak, whether from the effects of an acute or chronic disease, and depends upon an inflammation of the mucous membrane. In like manner sudamina are often looked upon as a very dangerous symptom, whereas only one-half of the

patients in whom this symptom is present die.

CASE VIII.—*Typhoid Fever—Erysipelas of the Face—Acute Peritonitis from supposed perforation of the Ileum—Recovery.*

A cook, aged 33, had resided in Paris three weeks, when, on attempting one morning (March 22) to get up, she was attacked with vertigo, pain in the head, buzzing in the ears, and chilliness, which obliged her to keep her bed. She arrived in Paris in perfect health, and had not been exposed to privations. These symptoms continued till the 25th, when, being obliged to go out, she fell down from weakness. The next day she vomited for the first time, and had diarrhœa, with pain in the abdomen. On the 31st she entered the hospital, having walked there with difficulty, supported by two persons.

April 1st.—Memory exact; lies on her back; eyes sunk; cheeks of a livid red colour; vision troubled; tinnitus aurium; great prostration of strength, but not complete; tongue dry and red, not pointed; pulse 120, small and feeble; great thirst; abdomen soft; the spleen cannot be distinguished; pain in both iliac fossæ increased by pressure; no pain in the epigastrium; twenty stools yesterday; respiration healthy.

Diet: ptisan. Antimon. Tart. gr. j. in die sumend.

2d.—Has had three stools, and vomited four times. Yesterday afternoon the nose became red and swollen; the left cheek soon became in a similar state, and during the night the right cheek. The erysipelas extends as far as the chin; the occiput is painful when pressed, but is not œdematous; pulse 112; other symptoms continue the same.

Eau de Seidlitz, half a bottle.

6th.—The erysipelas has subsided considerably, and there is abundant desquamation of skin; her menses returned yesterday; no headache; pulse 104; no thirst; feels very weak.

7th.—At half-past six yesterday evening she was attacked with an acute pain in the right iliac fossa, accompanied by nausea and vomiting and rigors. At midnight the abdomen was universally painful, the pulse 116, weak and feeble. The pulse is now 140, and very small. Some laudanum was immediately given her. The tongue is projected with difficulty; the cheeks are of a deep violet tint; 40 respirations in a minute; she cannot bear the least pressure on the abdomen; pulsations of the heart imperceptible; two stools after a lavement; only took three spoonful of soup and a biscuit yesterday.

Three oranges. Acet. Morph. gr. j. in Mist. Mucilagin. sum. Strict diet.

8th.—Pulse 152 yesterday afternoon; now 116. Countenance improved; no nausea; circumvolutions of the intestines can be distinguished; abdomen soft, and without pain, except when pressed; no stool. Tongue slightly moist; red in its circumference, white in the centre.

Three lemons, and a little ice. Rep. Mist.

10th.—Checks red; abundant desquamation; no pain in the abdomen, except in the right iliac fossa; tongue red, not very moist; small white spots on the mucous membrane lining the lips, and a white exudation at the roots of the teeth.

Rep. Mist. Fomentations to the abdomen.

For several days she continued in a very precarious state, the pulse being very quick and small; but by persevering in the administration of opium, and in complete abstinence from all food, she was, towards the end of the month, enabled to get up, although very weak. In the course of another month she left the hospital.

REMARKS.—The symptoms at the commencement bore some resemblance to those of ramollissement of the brain. Indeed, it has twice happened to M. Louis to confound typhoid fever with that disease, which he has seen at the commencement attended with no other symptom than excessive prostration of strength. On the fifth day the nature of the disease was announced by the diarrhœa, abdominal pain, vomiting, &c., although many other important symptoms of fever were absent. It is by no means rare to see erysipelas of the face developed during the course of a typhoid fever; whereas it is very rare to see erysipelas preceded by cerebral and abdominal symptoms for twelve days; so that this complication would rather tend to confirm the diagnosis. On the 7th instant evident symptoms of acute peritonitis were present, which M. Louis believed to be caused by a perforation of the small intestine caused by ulceration, and situated about the junction of the ileum with the cæcum. Peritonitis so rarely complicates acute diseases, that it may almost be laid down as a law, that if in the course of an acute disease there suddenly comes on a sudden pain, if this pain is increased by pressure, and accompanied by a rapid alteration of the features, and more or less promptly by nausea and vomiting, a perforation of the intestine exists. The probability becomes still greater if the disease is typhoid fever, as it is always accompanied by some degree

of ulceration of the intestine. This is the first case of perforation of the intestine, occurring in the course of a typhoid fever, which M. Louis has seen recover. Would not the peritonitis be considered by many physicians, and perhaps justly so, as a metastasis of the erysipelas of the face?

CASE IX.—*Scirrhus of the Pylorus—Death.*

A sempstress, aged 69, previons to the month of November, 1836, enjoyed excellent health; her digestion had always been good; she had never experienced pain in the epigastrium; had always been in easy circumstances, and enjoyed the necessities of life without having committed any excesses in drinking, &c. &c. In that month she was troubled, for the first time, with a pain in the epigastrium, and her appetite at the same time diminished. In December she was attacked with frequent vomitings, and often brought up her food thirty-six hours after eating it. On that account she took hardly any nourishment, and became very thin. The pain in the epigastrium increased during the months of January, February, &c., and came on generally two or three hours after a meal; the vomiting also became more frequent, the food which had been eaten three or four days before being often rejected unaltered. In the month of April she was admitted into La Pitié, under M. Louis, and presented the following appearance:—

April 22.—She is in the last stage of marasmus; cheeks purple, and very hollow; superior limbs of a purple hue; pulse very small; pulsations of the heart well marked; tongue villous and white; no diarrhœa or cough; a depression in the epigastrium; pain upon pressure $1\frac{1}{2}$ inches to the right and a little below the umbilicus; also a little above and below the umbilicus a projection having the form of the little curvature of the stomach. Upon pressure being applied, a species of gargouillement is heard; has vomited four times since yesterday a clear fluid; continual eructations of gas; respiration healthy. She remained in the hospital about a fortnight; at the expiration of which period she died. Extensive scirrhus of the pylorus was found to be the cause of the symptoms, but I unfortunately was not present at the examination.

Pain in the epigastrium and anorexia are by no means always the signs of an organic disease of the stomach. They occur also in chronic gastritis, a disease which has not yet been well described, and of which M. Louis has seen but few examples; that is to say, in healthy persons, and occurring as a primitive disease; for nothing is more common than chronic gastritis in the course of phthisis and other similar diseases. But in the latter disease

there is seldom vomiting of food taken two or three days before, as in this case. The vomiting was, however, by no means pathognomonic of organic disease; it was composed solely of the food, and was never mixed with blood, so that there was no reason to suspect ulceration of the mucous membrane. The depression at the epigastrium, the projection above and below the umbilicus, and the gorgouillement, indicated a distended stomach; the projection being chiefly caused by the lesser *cul de sac*. The situation of the pain seemed to indicate the pylorus as the part diseased, which was the case. M. Louis does not consider scirrhus an inflammatory affection, or to be a sequel of inflammation. Cancer seldom occurs before the age of fifty, whereas gastritis occurs at all ages. Men are less liable to cancer than women, though they more frequently commit excesses. This, however, proves nothing; for, according to the same authority, women are more subject to acute gastritis than men. Cancer has a predilection for certain parts of the stomach. Thus out of 33 cases

In 16 the cancer occupied the epilorus.
 11 small curvature.
 6 the large do.

The cicatrices of ulcerations of the stomach are never observed in the epilorus; they more usually occupy the posterior surface of the stomach. The parts of the body most subject to cancer, are less liable to inflammation. Out of 55 cases,

21 consisted of cancer of the stomach.
 22 uterus.
 10 liver.
 5 lung.
 2 rectum.

No account is here taken of the mamma or testicle. In the lung, which perhaps is the viscus most subject to inflammation, it only occurred in five cases, and in none of these primitively.

CASE X.—*Cancer of the Uterus—Death.*

A woman, aged 40, the mother of six children, was attacked with hæmorrhage from the uterus fourteen months ago, which lasted seven days, and has frequently recurred. She commenced menstruating at 16; her menses were not very abundant, and were always preceded by pains in the loins, which ceased on their appearance. She has had leucorrhœa from the time she first commenced menstruating. Her accouchements have all been natural, and she has generally kept her room five weeks after each. Since the first hæmorrhage, the leucorrhœa has been more abundant. She has continued her work, except during the last two days.

The hæmorrhage was preceded by pain in the loins, extending round the sacrum. She has had a difficulty in making water since her first accouchement, and passes it very slowly and frequently. The pain in the loins and sacrum has augmented during the last week, and the discharge has been red. She has had scarcely any rest during this time, on account of the great pain at the anus. Her bowels are relaxed. She has but little appetite, and is considerably emaciated. Her abdomen is well formed, but pressure in the left lumbar and iliac regions causes pain; pulse 84; chest healthy.

Pulv. Opii, gr.j. nocte sumend.

Inject. Calcis Chlorid. ʒij ad ʒj.

January 23d.—Has suffered much from pain; less discharge, passing her water with facility; no sensation of weight at the uterus; pulse 88; no headache; above the left clavicle are situated several small round spherical tumors which have existed for months; they have caused no pain, and had existed some time before they were discovered; several of the glands in the inguinal region are enlarged.

During the month of February she was examined with the speculum; the neck of the uterus was found indurated and enlarged on the right side, and of a livid tint, presenting a very unequal appearance. She complained greatly of a pain at the arms, which ceased after the application of leeches. Having obtained admission into Salpêtrière she soon after left the hospital.

The symptoms were quite sufficient to denote the nature of the disease. The hæmorrhage occurred at the commencement, was considerable, and was repeated four times. In inflammation of the uterus there is often hæmorrhage, but it is very slight. When fibrous tumors exist in the uterus, there is also hæmorrhage, but by no means so abundant; and it occurs at a later period. Out of twenty-one cases of cancer of the uterus, all of whom died and were examined, in two only was hæmorrhage not present, and in one of these there was a rose-coloured discharge. Out of the nineteen, in which hæmorrhage formed one of the principal symptoms, in 13 it was developed 3, 4, and 5 months after the commencement of the disease. In the remaining six it was the first symptom that was observed, preceding or occurring at the same time as the pain. Age had no influence upon it, for it occurred at the various ages of 23, 60, and 70. The two in whom this symptom was absent, were between 30 and 40. Hæmorrhage accompanies organic diseases of various organs, and in none is it so frequently present as in cancer of the uterus. The mucous

membrane is frequently found quite healthy: the bleeding often precedes the pain. The sensation of weight at the anus, which had lasted a considerable time, and was relieved by leeches, most probably depended upon a swelling of the neighbouring parts. The mean duration of cancer of the uterus is about two years or two years and half. Cancer of the stomach causes death in a much shorter period, in eight or ten months; the great importance of the functions of the organ fully accounts for this.

ACCIDENTS ADMITTED AT THE LONDON HOSPITAL.

*Surgeon of the Week, Mr. ANDREWS.
Assistant Surgeon, Mr. ADAMS.*

Dec.	Sex.	Age.	Case.
12.	M.	18	Injured back.
	M.	25	Punctured wound of the abdomen.
	M.	2	Burn.
	F.	60	Contused ankle.
13.	F.	13	Scald.
	M.	55	Injured ankle.
	M.	19	Lacerated hand.
	M.	66	Contused side.
14.	M.	30	Contus. d knee.
	M.	29	Fractured ribs.
	M.	74	Fractured ribs.
	F.	62	Contused hip.
	M.	46	Fractured humerus (neck.)
15.	M.	45	Lacerated scalp.
	M.	44	Contused ankle.
	M.	30	Contused ankle.
	M.	57	Fracture fibula.
	M.	40	Fractured fibula.
16.	M.	18	Contused knee.
	M.	30	Contused ankle.
	M.	28	Lacerated scalp.
17.	M.	45	Sprained knee.
	F.	46	Fractured tibia and fibula.
18.	M.	16	Sprained ankle.
	F.	48	Fractured ribs.

In-patients 26
Out-patients 39

Total during the week .. 65

*Surgeon of the Week, Mr. SCOTT.
Assistant-Surgeon, Mr. HAMILTON.*

19.	M.	19	Cut head.
	F.	3	Burn, and died the 20th.
	F.	18	Cut face and contusions.
20.	F.	6	Burn.
	M.	37	Cut head.
	M.	55	Punctured hand.
	M.	34	Contused side and abdomen.
	M.	47	Dislocated elbow.
	M.	44	Compound fracture of the humerus into the elbow joint.

M. 28	Strangulated congenital hernia (operated on.)
21. M. 31	Lacerated face.
M. 55	Retention of urine.
M. 37	Fractured os frontis, with depression and compound comminuted fracture of the ilium. Died same day.
M. 48	Fractured scapula.
F. 34	Punctured arm.
22. F. 63	Lacerated leg and contused arm.
F. 44	Sprained ankle
M. 18	Lacerated arm.
M. 30	Punctured hand.
23. M. 60	Fractured tibia and fibula.
M. 14	Fractured jaw.
M. 50	Contusion.
F. 28	Fractured clavicle.
24. M. 32	Cut face.

In patients 24
Out-patients 27

Total 51

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, Jan. 2, 1838.

Age and Debility . . . 25	Hooping Cough . . . 9
Apoplexy . . . 2	Inflammation . . . 14
Asthma . . . 6	Bowels & Stomach . . . 1
Childbirth . . . 3	Brain . . . 4
Consumption . . . 20	Lungs and Pleura . . . 6
Convulsions . . . 13	Insanity . . . 3
Dentition or Teething . . . 2	Measles . . . 9
Dropsy . . . 5	Paralysis . . . 1
Dropsy in the Brain . . . 1	Rheumatism . . . 1
Dropsy in the Chest . . . 4	Small-pox . . . 4
Erysipelas . . . 1	Tumor . . . 1
Fever . . . 9	Unknown Causes . . . 38
Fever, Scarlet . . . 5	
Fever, Typhus . . . 2	Casualties . . . 6
Heart, diseased . . . 1	

Decrease of Burials, as compared with }
the preceding week . . . } 79

METEOROLOGICAL JOURNAL.

*Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.*

Jan.	Thermometer.	Barometer
	from 24 to 45	29.84 to 29.99
Thursday . . . 4	24 33	30.13 30.18
Friday . . . 5	24 38	30.18 30.17
Saturday . . . 6	28 36	30.17 30.22
Sunday . . . 7	23 31	30.28 30.19
Monday . . . 8	17 26	30.17 30.08
Tuesday . . . 9	17 27	29.98 29.87
Wednesday 10		

Winds, N.W. and N.E.

Except the mornings of the 4th and 10th, generally cloudy; rain on the 4th, a dense fog on the 5th, and snow on the 8th and three following days.

Rain fallen, .05 of an inch.

CHARLES HENRY ADAMS.

ERRATUM.

In our last No. p. 588, *for*, "soon after he was attacked with vomiting, during which the hernia descended," *read*, "was attacked with coughing," &c.

WILSON & SON, Printers, 57, Skinner-st., London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JANUARY 20, 1838.

LECTURES

ON

FORENSIC MEDICINE;

Delivered at the Aldersgate School of Medicine.

By SOUTHWOOD SMITH, M.D.

Physician to the London Fever Hospital.

LECTURE VI.

Restrictions on the Deposition of Evidence in the English Courts of Law—The sanction of an Oath necessary in all criminal cases—None but an Original Record admissible—Self-criminative Evidence not demanded—Hearsay Evidence not admissible—When Medical Opinion is admissible—Conditions on which Dying Declarations are admissible—Confession—Penalties impossible on the medical witness for non attendance—Courts of Law in which the medical witness may be called to give Evidence—Constitution of the Court of the Coroner—Object of the Coroner's Inquest—Duties of the Coroner—Distinction between the Verdict of the Jury of the Coroner's Court and the Verdict of the Jury of the High Criminal Court—Recent Act providing for the Attendance and Remuneration of Medical Witnesses at Coroners' Inquests—Special object of the Medical Witness at the Coroner's Court—Effect of Evidence given before the Coroner's Court in case of the sickness or death of the witness—Special object of the Medical Witness at the High Criminal Court.

Restrictions on the deposition of evidence in the English courts of law.—In the preceding lectures I have entered fully into the consideration of the nature of evidence. I now proceed to make you acquainted with the restrictions which the mode of procedure in the English courts of law place upon the deposition of evidence. With

these restrictions it is important that you should be familiar, because if you are not cognizant of them, you may ignorantly do too little, or too much; neglect to do that which is essential to the admission of your deposition as evidence, or do that which will disqualify you, perhaps, on an essential point, for becoming a witness. Not only will ignorance of this subject be disreputable to you as men of education and science, whose peculiar knowledge is continually required in the courts of law, but it may be the means of defeating the ends of justice.

The sanction of an oath necessary in all criminal cases.—With the exception of dying declarations, to which I shall direct your attention immediately, no evidence can be given in any criminal matter unless upon oath; and therefore any one, unless he be a Quaker or a Moravian, who, on any account, scruples to take an oath, should be careful when employed on a case likely to come before the criminal tribunals, to have some person associated with him who may supply his place in court.

None but an original record admissible.—I have shewn how important it is, in every judicial case in which you are called to take a part, that you should immediately place upon record all the material circumstances which come under your observation. The record in which you originally note down the circumstances of the case you must carefully preserve. Of this original record you may avail yourself, for the refreshment of your memory; but you will not be allowed to make use of any copy of it. Any paper of this kind, to be of service, must be the original record.

Self-criminative evidence not demanded.—If you have yourself taken a part in any transaction by which you have become liable to a criminal prosecution, you will not be required to give evidence in that case. If, as a surgeon, you go out with parties about to fight a duel, with a view

of affording your professional assistance, you are liable to be prosecuted criminally if you knew that a duel was to take place. Should one of the parties be killed, and the surviving party be prosecuted for murder, and should you be summoned to give evidence in this case, you are not bound to answer any questions which may tend to criminate yourself.

"Mr. Pettigrew," said Mr. Justice Bailey, "I think it necessary to give you this caution: if you think the evidence you are about to give, likely to expose you to a criminal prosecution, you are not bound to give it.

"My Lord," said Mr. Pettigrew, "I am not competent to form any opinion of my legal guilt. I have not taken the part of principal nor second. The part which I have taken was merely to exercise my professional duty; in that I do not think there was any moral guilt."

"If," replied the judge, "you went, knowing a duel was to take place, for the purpose of giving professional attendance, I apprehend you are liable to a criminal prosecution.

"Then, my Lord," answered Mr. Pettigrew, "I must decline answering any questions.

"I recollect," continued Mr. Justice Bailey, "having seen a surgeon of eminence tried in this court on a similar occasion."

Neither Mr. Pettigrew nor his assistant were examined.

Hearsay evidence not admissible.—You will not be allowed to bear testimony to any thing which you have not yourself seen or observed. Any thing which you have merely heard from others is not evidence as coming from you. Hearsay evidence is peculiarly liable to the imperfections of incorrectness and incompleteness; there is no possibility of bringing it to the test of cross-examination; there are no means of sifting it in any other mode, and there is no responsibility attached to the statement of it. The English courts peremptorily reject it on the twofold ground, that "matters of fact must be tried only by proof of witnesses personally cognizant of such fact, upon oath before the judge, and that the party making the declaration must be subjected to cross-examination."

The exception to this is the account given by a patient to his medical attendant of his own state and feelings; such a statement is admissible as evidence. "In an action on a policy of life insurance, the account which the deceased gave to the medical witness some days after she had obtained the certificate of health, respecting her state on the former day, was admitted as evidence. The account which a person gives of his bodily feelings, in an-

swer to questions put to him by the surgeon with a view of ascertaining what he has suffered by reason of assault, is also admissible as evidence. It is the duty of the medical practitioner, in all such cases, to examine at the time the correctness of the statement, to sift its truth, and to declare to the court the conclusion at which, from a consideration of all the circumstances which he was enabled to observe, he arrived. "This kind of evidence," says Professor Amos, "of a person's own statement of complaints and symptoms, is admitted on the ground of its being part of what the lawyers call the '*res gesta*,' being not evidence of a by-gone fact, but part itself of the fact which it accompanies, and necessary to shew the true character of that fact."

When medical opinion is admissible as evidence.—Not only are the inferences you deduce from the facts you have ascertained admissible as evidence, but in certain cases even your opinion also. You may be called upon to give your opinion of the representation made by other medical witnesses, relating, for example, to the conduct, manner, and general appearance of an alleged lunatic. In like manner, in trials relating to policies of insurance on life, you may be called to deliver your opinion of the state of health of the deceased from the statements of others at the time of effecting the policy. In all this, the principle is precisely the same as that on which the reception of medical evidence in general is founded. In these as in all other cases in which the medical man is called to give evidence in a court of justice, he is a skilled witness; it is conducive to the ends of justice that the court should receive the advantage of his peculiar experience; and under all the circumstances in which the statement of that experience has a tendency to shed light on the facts of the case, the statement is admitted as evidence.

Conditions on which dying declarations are admissible as evidence.—You will often be called upon to state to the court words uttered in your presence by deceased persons in the immediate view of death, dying declarations as they are termed. Statements made in the immediate view of death are admitted as evidence, although no oath has been administered to the dying person, and although his declarations have not been subjected to cross-examination, on the ground that the solemnity of the occasion is a guarantee of the truth. The dying declarations of a person supposed to have been murdered, or who has sustained any kind of violence, are taken as evidence for or against the accused. But to give validity to such declarations, it is necessary that the person should be-

lieve himself to be actually dying; nay, that he should apprehend recovery to be impossible. Consequently the suggestion on your part of the hope of recovery to the dying, will, if there be evidence that the deceased paid any regard to your suggestion, completely invalidate his dying declaration. "The deceased asked me if his wound was necessarily mortal, (said Dr. Darling,) on a trial in a case of duelling; upon which I told him that his case was one of extreme danger; but that there had been instances where persons had recovered under such a wound; to which the deceased replied, I am satisfied." This conversation took place before the statement made by the deceased, which Dr. Darling was called to declare in evidence. Both Lord Tenterden and Mr. Justice Parke considered that this statement could not be admitted as evidence, because the remark of the physician was calculated to kindle a ray of hope in the dying man, and consequently his declaration could not be said to have been made in *articulo mortis*.

A female died by taking poison at the instigation of the father of her child, which was illegitimate. She related all the circumstances shortly before her death; but her declaration was not admitted as evidence, because, after it was made, she had requested that more medicine might be sent for; thus affording a presumption that the hope of recovery existed in her mind; and consequently that the declaration was not delivered after *every ray* of hope was extinct.

Not only must all hope of recovery be extinguished in the mind of the dying person, to give validity to a dying declaration, but the statement must be strictly voluntary. If you should be selected as the depository of the communication, and your character and office, as well as your situation about the patient, will be likely to suggest to his mind that you are the most fitting person to receive it, you should be merely the recipient of his communication; you should put no leading question; you should suggest nothing, do nothing, but simply receive the statement; excepting, indeed, it be to obtain an explanation of any thing that may be vague, doubtful, or liable to misapprehension. The exact words uttered by the deceased you should record. When there is time for it, you should take the deposition down on paper, read it over to the dying person, and obtain his signature to the document. If you should be unable to do this, on account of the rapid death of the deponent, you should make a memorandum of the conversation at once, while every syllable is fresh in your memory, and to this document you will be allowed to refer in giving your evidence in court.

In every case of this kind it is your duty to endeavour accurately to ascertain the state of the patient's mind at the moment he makes the declaration: whether he be collected; whether he be in the full possession of his reason; whether he be calm, coherent, clear; or agitated, confused, and incapable of sound judgment; whether his mind be under the influence of any particular bias; whether he be labouring under emotion, good or bad, in favour of or against the accused. Malevolence does not always fly from the human heart when death is felt to be at hand. Several appalling instances are on record of a determined purpose on the part of a dying person to attach the guilt of a capital offence to one known by the accuser to be innocent. In such a case you may have the satisfaction of being the safeguard of the innocent; your observation and sagacity may enable you to seize upon some circumstance capable of proving the falsehood of the accusation: it may thus be in your power completely to rescue the intended victim from the snares which are spread around him; snares from which it may be impossible that he should emancipate himself; and if ever you discern the slightest indication of a malevolent feeling or a vindictive purpose, you should use your utmost art and skill to elicit and establish the fact.

Confession.—If, as may sometimes happen, you are called upon by an accused person to receive a confession, take care that you do not hold out to him any inducement of any kind to make the communication; receive what is said to you without comment; reduce it to writing as soon as possible; read it over to the prisoner, and get it signed by him, and counter-sign it yourself, if you are the surgeon acting officially.

In the case of voluntary confession on the part of a person unaccused and unsuspected, while you make an accurate record of all that is stated, do not yield implicit credence to the story, but examine carefully into the state of mind of the deponent, and as far as you are able compare the particulars of the statement with all the notoriously true facts with which they purport to stand related. False self-inculpatory evidence, sometimes obviously resulting from mental disease, at other times induced by motives not at first view apparent, is by no means uncommon. Peculiar circumstances, and more especially such as are capable of producing disease, acting on persons of a certain mental conformation, may induce in them a real and firm belief that they have been guilty of crimes which they never committed, and these crimes they may confess with sincere contrition. About four or

five years ago, a very handsome young man, who resided at a town twelve miles distant from London, was attacked with fever, and lay at the point of death, being given over by his medical attendants. He was conscious that his end approached; but his spirit was so oppressed with guilt that he could not depart in peace without disclosing the dreadful secret that weighed upon his conscience. The curate, at his request, was sent for, to whom he confessed a criminal conversation with no less than six young ladies in the town, all recently married. The curate promulgated the confession, and threw the rural community, husbands, brothers, and fathers, into the most violent commotion. The young man recovered, and solemnly protested his utter unconsciousness of what he had confessed, and with like solemnity protested, that at all events there was not one word of truth in the statements. He was, however, obliged to leave the country, for no one would ever again admit him into their house or society; whereas the person who really deserved banishment was the curate.

In the war of the French revolution the *Hermione* frigate was commanded by Captain Pigot, a harsh man and a severe commander. His crew mutinied and carried the ship into an enemy's port, having murdered the captain and many of the officers, under circumstances of extreme barbarity. One midshipman escaped, by whom many of the criminals who were afterwards taken and delivered over to justice, one by one, were identified. Mr. Finlaison, the government actuary, who at that time held an official situation at the Admiralty, states, "In my own experience I have known, on separate occasions, more than six sailors who voluntarily confessed to having struck the first blow at Captain Pigot. These men detailed all the horrid circumstances of the mutiny with extreme minuteness and perfect accuracy; nevertheless, not one of them had ever been in the ship, nor had so much as seen Captain Pigot in their lives. They had obtained, by tradition, from their messmates the particulars of the story. When long on a foreign station, hungering and thirsting for home, their minds became enfeebled; at length they actually believed themselves guilty of the crime over which they had so long brooded, and submitted, with a gloomy pleasure to being sent to England in irons for judgment. At the Admiralty we were always able to detect and establish their innocence, in defiance of their own solemn asseverations."

Penalties impossible on the medical witness for non attendance.—Since it is often of the last importance to the interests of indi-

viduals, and of the community, that the information which you, as medical witnesses, are capable of affording, should be given in the courts of justice, the legislature has made effectual provision for causing your evidence to be forthcoming. It has armed the Courts with the power of compelling your attendance. Medical witnesses are summoned by a writ of subpoena, which commands them to appear at the trial at a fixed time, to give testimony in the case, under a penalty of one hundred pounds to be forfeited to the king. If the parties suffer from the non-attendance of a medical witness, unless he can show a lawful and reasonable cause to the contrary, he may be compelled to pay such reasonable compensation to the party aggrieved as by the discretion of a jury, under the direction of the Judge of the Court out of which the process issues, shall be awarded. This is effected either by an attachment, that is, seizure of the person, for a contempt of the process of the Court, or an action of common law. But this cannot be done unless it be in proof that the witness was *personally* served with the subpoena, and that his reasonable expenses were tendered or paid at the time of the service. On this account a guinea is usually paid when the subpoena is delivered to the medical man, whereas to an ordinary witness a shilling only is given. This relates to civil cases. In criminal cases attendance is compelled, either by the process of subpoena, or the witness is bound over when first examined either by the justice or by the coroner, and, as in the former case, he is liable for disobedience to an attachment or to a committal for contempt.

Courts of law in which the medical witness may be called upon to give evidence.—The courts in which, as medical witnesses, you may be called to give evidence, are five: the High Court of Parliament, the Ecclesiastical Court, the Civil Court, the High Criminal Court, and the Coroner's Court. I have already stated the several classes of subjects upon which you may be required to give evidence in these different courts. The questions which you may be called to elucidate in the High Court of Parliament, and in the Ecclesiastical and Civil Courts, are sometimes questions of the utmost delicacy and difficulty, the true solution of which will require, on your part, the most accurate and comprehensive knowledge, and the exercise of the most cautious and sagacious judgment. To these questions a full consideration will be given in a subsequent part of the course. But it is in the Court of the Coroner, and in the High Criminal Court, that you will be most frequently called to appear; here the questions on

which you will have to decide are often those of life and death; questions on which you cannot decide safely, justly, without putting into requisition diligently and skilfully the utmost resources of your art and science.

Constitution of the court of the coroner.—

In accordance with the value which is attached to human life, in every civilized community, and the sacredness of which has been shown to increase with the progress of civilization, no human being, however low his rank, is allowed to die in a sudden and unknown manner without the institution of a solemn inquiry into the cause of such death. In every country in which civilization has advanced beyond a state of barbarism, this inquiry is not left to chance, but devolves upon an authority legally appointed and duly constituted for the express purpose of examining into every circumstance connected with the fatal event, not only when the cause of it is altogether unknown, but also when the cause of it is either doubtful or of a suspicious nature. In our own country this authority is vested in a special court, called the court of the coroner. The court of the coroner consists of an officer called the coroner, and of a number of persons who are termed jurors.

The coroner (*coronator*), so called because he has principally to do with pleas of the crown, or such wherein the king is principally concerned, was, in ancient times, an officer of great dignity. By the statute of West. 1, it was enacted that none but lawful and discreet knights should be chosen; and there was an instance in the 5 Edward III. of a man being removed from this office because he was only a merchant. "Now, indeed," says Blackstone, "through the culpable neglect of gentlemen of property, this office has been suffered to fall into disrepute, and get into low and indigent hands; so that, although formerly no coroners would condescend to be paid for serving their country, and they were, by the aforesaid statute of Westm. 1, expressly forbidden to take a reward, under pain of a great forfeiture to the king, yet, for many years past, they have only desired to be chosen for the sake of their perquisites; being allowed fees for their attendance by the statute of 3 Hen. VII. c. 1., which Sir Edward Coke complains of heavily; though, since his time, those fees have been much enlarged."

There is no good reason why a public servant should not be paid for his public service; payment for the performance of the duties of an office is the only ground of responsibility universally recognized, and it does not follow that because a person "condescends to be paid for serving his

country," he is necessarily "low and indigent." For the proper performance of the duties of the coroner, which are extremely arduous, and which occasionally occupy large portions of time, the requisites are not wealth and rank, but education and skill; an acute and sagacious mind, thoroughly imbued with legal and medical science, and well practised in the business of life. To render the office efficient for its object, it is clothed with considerable authority and power. The court of which the coroner is president may consist of an indefinite number of persons, among whom it is not necessary that there should be an absolute unanimity of opinion, but twelve of them must agree in opinion, to constitute a verdict. The individuals composing the coroner's court are all severally sworn to the due performance of their duty, as in the high criminal court.

Object of the coroner's inquest.—The object of the coroner's court is to make inquiry concerning the death of any person who dies suddenly, under unknown, doubtful, or suspicious circumstances; who is found dead in any unusual situation, or who has come to his end by violent means; whether from accident, from the action of any external physical agent, or from intentional human agency. A person unknown is found dead in the street, or in a field; a person well known retired to rest in apparent health, but is found dead in his chamber; a person drops down dead unexpectedly and suddenly, in the midst of his friends, no cause of death being apparent; such a body ought not to be removed from the place in which it is discovered—ought not to be so much as stirred, unless for the purpose of resuscitation, until it has been viewed by the coroner and his jury. It is a misdemeanor to bury a body, under such circumstances, until the coroner has made his inquest into the cause of death; and after such a body has been interred, it is in the power of the coroner to order its disinterment, for the purpose of judicial examination.

It is the duty of the persons who discover such a body, to apprise the coroner of the event; who must summon his jury, inspect the dead body, inquire into the cause of death, record the verdict of his jury, and act upon that verdict, if it be of a nature which requires any further proceeding—that is, if the evidence adduced show that the death has been caused by violence produced by intentional human agency.

Duties of the coroner.—The coroner can make no inquiry of death without a view of the body, both by himself and his jury. The inquest must be *super visum corporis*; for if the body be not found, the

coroner cannot sit." Any inquisition taken without an actual view of the body is void; but after the body has been viewed, the inquisition may be carried on in any convenient place.

The coroner can summon witnesses, and compel their attendance. His authority may be enforced by the power of the Lord Chief Justice of the King's Bench, who is the principal coroner of the kingdom, and may, if he please, exercise the jurisdiction of a coroner in any part of the realm.

Every witness examined by the coroner is examined on oath; and no evidence can be taken but on oath, excepting in the case of Quakers and Moravians. By a recent act, these two denominations of dissenters are allowed to bear witness, even in criminal cases, on their solemn affirmation, instead of on their oath; a privilege which the Quakers had previously enjoyed in civil cases only.

The coroner's inquest is generally an impartial inquiry into the facts of any event which may come before the court: but such an event may assume a form which changes the ordinary functions of the coroner into those of a judge. The friends of the deceased may become accusers; some suspected person may stand in the coroner's court in the condition of a defendant. In such a case, the coroner is bound to hear counsel and evidence on both sides, and if he refuse to do so, the inquisition may be rendered void.

It is the duty of the coroner to take notes of the evidence. The evidence of the medical witness, more especially, is taken down with extreme care; and if the case be referred for trial to the high criminal court, the record of the coroner is presented at that court, and the evidence which you have already given before the coroner will be compared with that which you now give before the higher tribunal.

If the coroner's jury decide that the death in question has been produced by violence, and that there is reasonable ground of suspicion that a particular individual, or individuals, have been concerned in producing it, the coroner must arrest such persons and place them in prison, where they must await their trial in the high criminal court.

The coroner must enrol the verdict of his jury, return the inquisition to the justices of the next gaol delivery of the county, or certify it to the King's Bench, and bind over the witnesses to appear at the trial, to give in the higher court the evidence which they have given before his.

Distinction between the verdict of the jury of the coroner's court and the verdict of the jury of the high criminal court.—The verdict of a coroner's jury against an individual is,

then, sufficient legal ground for sending that individual to trial before the high criminal court, and therefore for his arrest and imprisonment, that his appearance may be compelled in that court. The verdict of not guilty, by the high criminal court, constitutes legal innocence. Whatever evidence of his guilt it might be possible subsequently to adduce, no further proceedings can ever be taken against him for an offence of which he has been declared innocent by the highest tribunal.

But this is by no means the case with the verdict of the coroner's court. The object of that court is to make inquiry, to investigate circumstances, to obtain evidence, to secure the person of an individual when suspicion falls upon him; but nothing is decisive, nothing final, in that court; consequently, although the verdict of the coroner's jury be that death has arisen from a natural cause, or (as they commonly call it) from the visitation of God, yet, should circumstances subsequently occur which beget a suspicion of murder, the former coroner's verdict is not sufficient to prevent further investigation.

In like manner, if the coroner's jury find the case in question to be one of murder—if suspicion have fallen on a particular individual—if the coroner's court, after the most careful and protracted investigation, decide that there is no real ground for suspicion in regard to that individual, and consequently this court take no proceedings against such individual, that person is not in a legal sense acquitted—not safe from any subsequent proceedings; but if strong grounds of suspicion again arise, he may at any time be arrested, imprisoned, and brought to trial in the high criminal court.

Recent act providing for the attendance and remuneration of medical witnesses at coroners' inquests.—The court of the coroner, then, is one merely of inquiry, not trial—of investigation, not decision. Still this inquest can seldom proceed many steps without the assistance of a medical man. Commonly, indeed, for obvious reasons, some medical practitioner is sent for in the first instance, and thus necessarily becomes a principal witness; and if, on the assembling of the coroner's court, no medical man be found in this position, one of the first steps of the coroner usually is to call to his assistance some medical practitioner. The court of the coroner has to determine whether the death in question has been produced by a natural cause, or whether it has been produced by violence of any kind; and if by violence, whether there be ground for suspecting that such violence has been produced by intentional human agency. The former fact can seldom be ascertained without medical investigation. A post-mortem

examination is usually indispensable, and it is often necessary that the contents of the stomach and intestines should be analyzed. Without such investigations, carefully and skilfully performed, the decision of the coroner's jury must often be to the last degree vague and uncertain. But no man can carry on such investigations without bringing to the inquiry much professional knowledge, and devoting to it a considerable portion of time. For the time and skill thus devoted to the public service, it is but just that the public should provide reasonable remuneration. Until very recently, though the coroner might summon to his assistance the medical witness, he had no power to recompense him for his services. Now, however, the case is completely altered. By an act which bears date the 17th of August, 1836, entitled "An Act to provide for the Attendance and Remuneration of Medical Witnesses at Coroners' Inquests," (for which the profession and the public are chiefly indebted to Mr. Wakley, one of the members for Finsbury), the medical witness is expressly entitled to a fee of one guinea for simple attendance, and of two guineas for a post-mortem inspection, or for a chemical analysis. By this act it is provided, that "whenever, upon the summoning or holding of any coroner's inquest, it shall appear to the coroner that the deceased person was attended at his death, or during his last illness by any legally qualified medical practitioner, it shall be lawful for the coroner to issue his order, in the form marked (A) in the schedule hereunto annexed, for the attendance of such practitioner as a witness at such inquest; and if it shall appear to the coroner that the deceased person was not attended at or immediately before his death by any legally qualified medical practitioner, it shall be lawful for the coroner to issue such order for the attendance of any legally qualified medical practitioner, being at the time in actual practice in or near the place where the death has happened; and it shall be lawful for the coroner, either in his order for the attendance of the medical witness, or at any time between the issuing of such order, and the termination of the inquest, to direct the performance of a post mortem examination, with or without an analysis of the contents of the stomach or intestines, by the medical witness or witnesses who may be summoned to attend at any inquest; provided that if any person shall state upon oath before the coroner that in his or her belief the death of the deceased individual was caused partly or entirely by the improper or negligent treatment of any medical practitioner or other person,

such medical practitioner or other person shall not be allowed to perform or assist at the post-mortem examination of the deceased." It is further provided that "whenever it shall appear to the greater number of the jurymen sitting at any coroner's inquest, that the cause of death has not been satisfactorily explained by the evidence of the medical practitioner, or other witness or witnesses, who may be examined in the first instance, such greater number of the jurymen are hereby authorized and empowered to name to the coroner in writing any other legally qualified medical practitioner or practitioners, and to require the coroner to issue his order, in the form hereinbefore mentioned, for the attendance of such last-mentioned medical practitioner or practitioners as a witness or witnesses, and for the performance of a post-mortem examination with or without an analysis of the contents of the stomach or intestines, whether such an examination has been performed before or not; and if the coroner, having been thereunto required, shall refuse to issue such order, he shall be deemed guilty of a misdemeanor, and shall be punishable in like manner as if the same were a misdemeanor in common law."

The act then goes on to provide for the enforcement of the attendance of the medical practitioner, in the terms following:—

"And be it further enacted, That where any order for the attendance of any medical practitioner, as aforesaid, shall have been personally served upon such practitioner, or where any such order not personally served shall have been received by any medical practitioner in sufficient time for him to have obeyed such order, or where any such order has been served at the residence of any medical practitioner, and in every case where any medical practitioner has not obeyed such order, he shall for such neglect or disobedience forfeit the sum of five pounds sterling, upon complaint thereof made by the coroner, or any two of the jury, before any two justices having jurisdiction in the parish or place where the inquest under which the order issued was held, or in the parish where such medical practitioner resides; and such two justices are hereby required, upon such complaint, to proceed to the hearing and adjudication of such complaint; and if such medical practitioner shall not shew to the said justices a good and sufficient cause for not having obeyed such order, to enforce the said penalty by distress and sale of the offender's goods, as they are empowered to proceed by any act of Parliament for any other penalty or forfeiture.

SCHEDULE TO WHICH THIS ACT REFERS.

(A.) *Form of Summons.*

Coroner's inquest at . . . , upon the
body of . . .

By virtue of this my order, as coroner for . . . , you are required to appear before me and the jury at . . . , on the day of . . . , one thousand eight hundred and . . . , at . . . of the clock, to give evidence touching the cause of death of . . . , (and then add, when the witness is required to make or assist at a post-mortem examination,) and make or assist in making a post-mortem examination of the body, with, or without, an analysis, (as the case may be), and report thereon at the said inquest.

(Signed) ———, Coroner.

To ———, Surgeon (or M.D. as the case may be.)

(B) *Table of Fees.*

1. To every legally-qualified medical practitioner, for attending to give evidence under the provisions of this act at any coroner's inquest, wheret no post-mortem examination has been made by such practitioner, the fee or remuneration shall be one guinea.

2. For the making of a post-mortem examination of the body of the deceased, either with or without an analysis of the contents of the stomach or intestines, and for attending to give evidence thereon, the fee or remuneration shall be two guineas.

(C.) *Coroner's Order for the Payment of Medical Witnesses.*

By virtue of an act of Parliament passed in . . . session of . . . , holden in the . . . , intituled . . . , I, the coroner of and for . . . , do order you, the overseers of the parish (or township, as the case may be), to pay to . . . the sum of (one guinea, or two guineas, as the case may be), being the fee (or fees) due to him for having attended as a medical witness at an inquest holden before me this day of . . . , upon the body of . . . , about the age of . . . , who was found dead at . . . (or other particulars or description), and at which said inquest the jury returned a verdict of . . .

(Signed) ———, Coroner
of . . .

Witnessed by me, ———.

To the Overseers, &c.

Special object of the medical witness at the court of the coroner.—The special object of your attendance as medical witnesses at the court of the coroner, is to assist that

court in determining on the propriety of further proceedings. It will mainly depend on the evidence given by you whether the case be dismissed at once as requiring no further judicial inquiry, or whether it be carried to a higher tribunal. If your evidence be inaccurate or incomplete, the guilty may escape, or, on the other hand, unjust suspicion may be fastened on an innocent man, who may be thrown into prison, and confined there several months before he can be brought to trial. At the trial in the high criminal court, as I have already stated, the evidence you give will be compared with that which you have already given in the court of the coroner, and there your evidence will be subjected to all the sifting of cross-examination; all the ingenuity which counsel can employ will be exerted to find in it something incorrect or incomplete; and if any such flaw can be detected, and more especially if your statements can be made to vary materially from the evidence you gave before the coroner, your reputation is lost.

Effect of evidence given before the coroner's court in case of the sickness or death of the witness.—There is one circumstance connected with evidence given before the coroner's court which it is highly important that you should bear in mind. If a witness be prevented by sickness from appearing at the trial in the high criminal court, or be overtaken by death before the trial comes on, the evidence of such witness is taken without any reserve or limitation, although it may not have been delivered in the presence of the accused, and although it be no longer possible to sift its truth and correctness by the process of cross examination. This should make you exceedingly cautious in the evidence you give before the coroner, and should render you watchful of the evidence given by others relative to all the subjects on which your profession renders you a skilled witness, or places you in a favourable situation for observing and collecting evidence. It has been already stated, that if a woman swear positively that a man has violated her, and die in the interval between her deposition and the trial, her deposition is taken as evidence against the prisoner without reserve. Whenever, therefore, you are engaged in any investigation of this kind, it is incumbent on you to avail yourself of every means in your power to sift the truth and correctness of her evidence, in order that you may prove a shield to the accused, if he be falsely accused.

The case which you are called upon by the coroner to investigate will frequently turn out to be a case of natural death,

occurring under circumstances which have given rise to a suspicion of violence, perhaps of poisoning. It may really be a case of poisoning. Then you must collect and state the evidence of the fact. On the other hand, it may be a case of natural death; however plausible may be the suspicion of poisoning, however clamorous and violent may be the rumour of it, at the close of your investigation you may be in possession of indubitable proof that the death in question has resulted from a natural cause. Whenever the facts ascertained by you clearly lead to this conclusion, you should state the result in the most decided and positive manner; for it is even more important to prevent suspicion from falling on the innocent, and to put to silence unfounded clamour, than to bring the guilty to justice. The following case, recorded by Dr. Christison, affords a good example of the completeness with which it may be sometimes in your power to accomplish these desirable ends.

A middle aged man, who had long enjoyed excellent health, one afternoon returned home tired. He had the misfortune to live on very bad terms with his wife; and after having been well beaten by her he went to bed. In about a quarter of an hour afterwards he was found by one of his workmen gasping, rolling his eyes, and quite insensible, and he died in a few minutes. From the evil treatment he received from his wife, it was suspected that he died of poison. Poisoning, however, in this instance was out of the question; for on inquiry into the circumstances of the case, it was proved that on the morning in question he had breakfasted about nine o'clock; that he had taken nothing whatever into his stomach from that hour to the moment of his death, and that he had continued perfectly well until his return home about two o'clock; consequently an interval had elapsed of five hours between his having taken any thing into his stomach and his death, during the whole of which period he had remained in his usual state of health.

This history was alone sufficient to put an end to all further inquiry as to the question of poisoning; it was alone sufficient to prove that death must have arisen from some natural cause. No inspection of the body, no analysis of the contents of the alimentary canal, were requisite to establish this point, because no poison but one of the most active narcotics, taken in a large dose, could have occasioned death with such rapidity; and the operation of such a poison, in such a dose, could not possibly have been suspended for the space of five hours. Of course, even in such a case as this, for your own satisfaction as well as for that of the jury, you

would examine the body; but then you would do it to discover the exact cause of death, to ascertain the true pathology of the case, and not to satisfy yourself that it must be referred to some natural cause. Of that you might be assured without any further investigation.

Without any knowledge of the species of violence inflicted, or the particular poison administered, it sometimes happens that a strong suspicion prevails that violence of some sort has been done, or that poison of some sort has been given; and you will be called upon by the coroner to decide whether there be a certainty, a probability, or a possibility, that the death in question has been occasioned by any species of violence or of poison.

The infliction of any kind of external violence capable of producing death, is so palpable, that you will in general have no difficulty in deciding a case of this class: to determine whether death has been produced by poison, when there is no indication of the individual poison that has been administered, requires much care. The means of arriving at a just conclusion I shall state hereafter; but commonly the chief object you will have to accomplish will be to establish the impossibility of poisoning, and, in the manner I have just explained, to remove plausible suspicion, and to silence vague rumour.

Special object of the medical witness at the High Criminal Court.—If, on the other hand, the evidence which you are constrained to give be of a criminative character, the case will be referred to a higher tribunal; and when you appear in the high criminal court, your position will be changed, and you will have a somewhat different object to accomplish.

Here, the indictment almost always charges the accused with the commission of some particular species of violence, or the administration of some one particular poison. If it be your conviction, from the investigation you have made, that the facts justify this indictment, then your main purpose must be to bring together and to state the whole medical evidence to this effect, and to secure it against the doubts which the ingenuity of counsel will be sure to throw over your conclusions, if the premises be any where false or defective. If your minds have been thoroughly imbued with the doctrine which has been delivered relative to the nature of evidence, and the means of rendering it correct and complete, you will, in every investigation of this kind, be constantly upon the search for infirmative facts; and consequently you will be always prepared to give full effect to every medical circumstance favourable to the accused. It will be impossible that you should ever allow your zeal for the

reputation of scientific accuracy or professional skill, to exceed your love of justice. To all instructed and well-regulated minds in the community, there is no sight more hateful than that of a medical witness at a trial assuming the character of a partizan. It is the part of a barrister to take a side; it is his office to bend every energy of his mind to make that side triumphant. Sophistry and falsehood, if they do but accomplish that end, are as good for him as reason and truth, and are used by him without scruple. But it is the part of the physician to search after Truth, and when he has found her, to reveal her so clearly that Justice can take no side but hers.

Sometimes the indictment charges the prisoner with administering a particular poison; and also some poison unknown to the prosecutor. There are cases in which the evidence of the particular poison is merely presumptive, and in which that presumption may not be strong. It may be impossible to prove what poison has been given, and yet possible to prove that some poison has been given. This is what is termed a charge of poisoning, in a general sense, and convictions have recently been obtained under such a charge; cases in which no satisfactory proof existed what poison had been given, but in which satisfactory proof was adduced that poison of some sort had been administered. These are cases of some delicacy and of great importance; and in a subsequent part of the course I shall direct your attention to the evidence upon which these cases may be sustained, and without which they must fail.

LECTURES
ON THE
ENTOZOA,
OR
INTERNAL PARASITES OF THE
HUMAN BODY,

*Being Part of a Course on Morbid Anatomy,
delivered at the London Hospital,*

By T. B. CURLING, Esq.

LECTURE III.

HAVING in the last lecture concluded the description of the common tape-worm, I come now to speak of the next in the list of the Entozoa, the

7. *Bothriocephalus latus*,

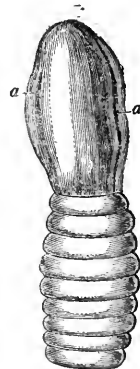
(from *βοθριον*, a small pit, and *κεφαλη*, the head.) This species of tape-worm may be readily distinguished from the *Tenia solium* by the form of the segments, which are broader than they are long, and by the

position of the genital pores, which occur in a series along the middle of one of the flattened surfaces of the body, and not at



B, Joints of the *Bothriocephalus latus*, shewing the central pores.
C, Ova, magnified.

the margin of each segment, as in the *Tenia solium*. The head, from the formation of which the name of the worm is derived, was discovered by Bremser. It differs considerably from that of the *Tenia*, being of an elongated form, two-thirds of a line in length, and presenting, instead of the four round mouths characteristic of the true *Tenia*, two lateral longitudinal depressions or fossæ. The worm is thinner



Head of *Bothriocephalus latus*, magnified.
a, Longitudinal fissures.

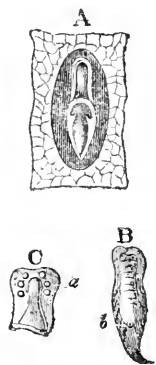
than the *Tenia solium*, and its length likewise is generally rather less; they are occasionally, however, met with of considerable size. Boerhaave states that he effected the expulsion of one, three hundred ells long, from a Russian. The *Bothriocephalus latus* is much less apt to part with single joints than the other spe-

cies of tape worm, which is perhaps owing to the circumstance that the longitudinal fibres entering into its composition are continued from one segment to the other, instead of being, as in the *Tænia*, distinct in each articulation. It inhabits the small intestines, in which as many as three or four are often found to co-exist. The people amongst whom they occur most frequently are the Russians, Poles, and Swiss. In the French they are as common as the *Tænia*, but they are rarely met with in England.

The symptoms which they occasion, and the treatment necessary for their removal, are similar to those of the *Tænia*.

8. *Polystoma pinguiola*,

(from *πολύς*, many, and *στομα*, stomach.) The body of this species of *Polystoma* is flat, truncated anteriorly, and pointed at the posterior extremity. On the under side of the head there are six orbicular pores disposed in a semilunar form. A large suctorious cavity exists on the ventral aspect, and a small orifice is situated at the apical extremity. This parasite has



Polystoma pinguiola.

- A, *Polystoma* in situ.
B, Natural size.—b, Suctorious cavity.
C, Head, shewing the six pores.

only been found once in the human body. Treutler, a German, discovered it in a small tumor about the size of a hazel-nut, situated in the adipose tissue connected with the left ovary of a young woman who had died in child-bed. The tumor, which appeared to consist of indurated fat, was of a reddish colour, and hollow within.

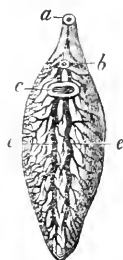
Cruveilhier questions, and I think with good reason, whether the body found by Treutler was really a parasitic worm. In the first place, the ovary is not surrounded by fat; and it is somewhat surprising that this most curious animal should only have been seen by one individual. Besides, loose albuminous bodies are so often found floating free in ovarian and other

serous cysts, that it is not impossible that this body was nothing more than coagulated albumen, the orbicular pores being only small accidental openings in it.

A second species of *Polystoma*, the *Polystoma venarum*, is also mentioned by Treutler. It was found in the saphena vein of a man, which was accidentally ruptured while bathing, and is generally supposed to have been a non-parasitic worm belonging to a species of *Planaria*, and to have been introduced accidentally into this strange situation.

9. *Distoma hepaticum*, or Liver Fluke.

The form of the *Distoma hepaticum* is oval, flattened, and elongate. It measures generally from one to five lines in length, and from half a line to a line in width. The anterior pore, or mouth, is round and small; the posterior cavity, which is imperforate, and subservient only to adhesion and locomotion, is large, transversely oval, and situated on the ventral surface of the body in the anterior half. From the anterior pore proceeds a short wide tube, the œsophagus, which terminates in two nutrient canals, which run along the centre of the animal to the opposite end, sending off from their outer



Distoma hepaticum; natural size.

- a, Mouth.—b, Genital pore.—c, Nutrient canals.

sides cæcal branches or processes, but have no anal outlet. Between the two pores already described there is a third orifice, exclusively destined, like the opening on each joint of the *Tænia*, to the generative system; and from which there is generally protruded, in the full-sized specimens, a small tubular process.

The *Distoma hepaticum* inhabits the gall-bladder and biliary ducts, and appears to derive its nutriment from the bile. In the liver of a sheep which I lately dissected, the biliary ducts containing these animals were much enlarged, some of them being capable of admitting the little finger; and the surrounding tissue was atrophied. Cruveilhier states that he has found the ducts cartilaginous, and even ossified, in animals affected with them. This worm very rarely infests the human species; in-

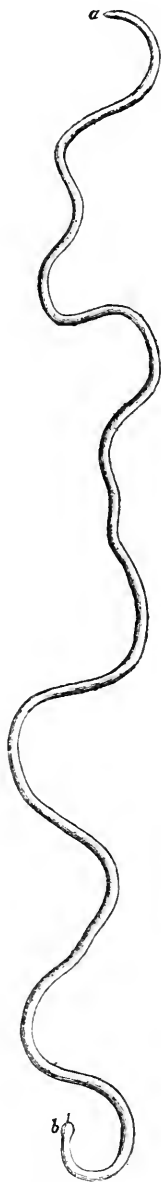
deed Bremser has never met it, and has only been able to collect five or six well-authenticated cases in which it has been found. It occurs, however, in many animals, and very commonly in sheep suffering from the disease called the rot. There is no account of the symptoms which the *Distoma* occasions in man. It would probably, as in animals, be attended with considerable emaciation. Chabert relates a case in which he effected the expulsion of a considerable number from a girl twelve years of age, by means of his empyreumatic oil.

Caecimintha.—We have in this, the fourth class, eight kinds of parasites infesting the human body.

10. *Filaria Medinensis* — *Medina, or Guinea Worm*—*D. acunculus*.

The length of this worm varies from six inches to two, eight, or twelve feet; and its thickness from half to two-thirds of a line. It is generally of a whitish colour, but sometimes of a dark-brown hue. The body is round, nearly of uniform size, but a little attenuated towards the anterior extremity. It is traversed by two longitudinal lines, corresponding to the intervals of the two well-marked fasciculi of longitudinal muscular fibres. At the anterior extremity there is a circular orifice, with swollen edges, which is the mouth. The caudal extremity of the male is obtuse, and emits a single spiculum: in the female it is acute, and suddenly inflected. This worm is so rarely obtained in a condition admitting of its organization being accurately examined, that a good account of its internal structure is still to be desired. No distinct tubes for digestion or generation have yet been discovered. In other kinds of the *Filaria* infesting animals, a simple ovary, oviduct, and uterus, have been observed. The generation of the *Filaria* is viviparous, and the progeny, which are extremely numerous, are not included in a special generative tube, but float freely along with a granular substance, in the common muscular integuments of the body. It is curious that the body of the young differs from the parent, one extremity being capillary, and terminating in the finest point.

The *Filaria Medinensis* is developed in the subcutaneous cellular tissue, generally of the lower extremities, especially the feet. It has been met with, however, in nearly all the superficial parts of the body, even beneath the conjunctiva of the eye; and several have been known to co-exist in the same patient. In 184 cases mentioned by Sir James Macgrigor, the dracunculus occurred 124 times in the feet, 35 times in the legs, 11 times in the thighs, 7 times



Filaria Medinensis.

in the hands, twice in the scrotum, and likewise twice in the groin and on the body. It occurs at all ages and in both sexes, and appears to be endemic in the tropical regions of Asia and Africa, where it appears generally in the hottest seasons. It is by no means confined to the natives: many facts tend to show that it may be

communicated from one individual to another; and Lind and other writers are of opinion that Europeans, on visiting the countries where it exists, become affected with it by contact with the negroes. In America it is said to make its appearance almost exclusively amongst the negroes, and chiefly in those who are newly arrived from Africa. We occasionally meet with it in this country, in individuals who have recently returned from the tropics. There is a specimen in the museum of the College of Surgeons, twenty-two inches in length, which was extracted from the leg of a boy who was a patient of the late Sir W. Blizard, at this hospital. We have also a worm in the museum, removed by Mr. Headington, who was formerly surgeon of this hospital, from the leg of a sailor, where it had been imbedded for two years; and there was another patient in the hospital at the same time, likewise affected in this way. From some observations by Sir James Macgrigor, it would appear that the disorder originating in these worms prevails at times in an epidemic form. Thus he mentions that some troops having embarked, after a residence of two months at Bombay, were attacked whilst at sea so generally, that out of three hundred and sixty as many as one hundred and sixty-one became affected with it. Clot Bey, a French surgeon, mentions that he has seen as many as a hundred patients, labouring under Guinea worm, in hospital at one time. Little seems to be known which can account for the development of this parasite, though authors have speculated abundantly on the subject. It has been noticed, however, that it occurs much less frequently amongst the officers than the common soldiers, and those who occasionally lie on the ground, or go about with their feet and arms naked.

The Guinea worm may remain imbedded beneath the skin for many months, without occasioning any inconvenience, but sooner or later it excites irritation and inflammation in the structures around, which vary in severity according to the constitution of the individual, and the situation and size of the worm. The symptoms commence in a formicating sensation or uneasiness under the skin, accompanied with a superficial cord-like elevation on the surface. A phlysaecous vesicle or pustule forms, which bursting, gives exit at a circular aperture, either immediately or after suppurating for a day or two, to the head of the worm. These local symptoms are preceded usually by slight derangement of the system generally. When situated about the fingers or toes, the worm is often productive of much suffering, and is with difficulty got rid of.

When deeply seated it sometimes causes considerable fever, great swelling, and tedious abscesses and sinuses, giving out a serous ill-conditioned discharge for many months without the worm making its appearance.

The treatment consists in the cautious and gradual extraction of the worm, special care being taken to avoid breaking it across, as this accident is liable to be followed by an aggravation of the inflammation, and the formation of sinuses in its course, together with great constitutional disturbance. These unfavourable effects are attributed, by Hunter, to the contact of dead animal matter, with a large extent of living surface, to which it now bears the relation of a foreign body; such violent symptoms being rarely seen so long as the dracunculus is alive and uninjured. When the worm protrudes, it should be laid hold of and gently drawn out from its resting place as far as possible. The part removed should then be secured at the aperture with a strip of plaster, or tied to a piece of stick, and the traction may be repeated once or twice in twenty-four hours until the entire worm is brought away—a process often requiring many days, or even a month, for its completion. Some surgeons recommend cutting down upon the middle of the animals, and by pulling simultaneously at both ends, shortening the period requisite for the removal of the dracunculus—a plan which is practised by the native Indians. When the worm is got rid of, the sinus usually closes readily. The use of internal remedies and external applications do not appear to be of any essential service. Sir James Macgrigor found that the extension of this affection amongst the troops was checked by requiring great attention in regard to cleanliness, and separating the soldiers troubled with these worms from those that were free from them.

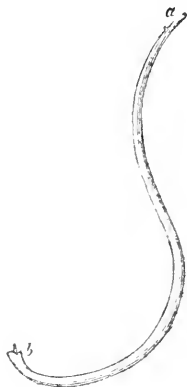
11. *Filaria Oculi*.

The discovery of a species of *Filaria* in the interior of the human eye, we owe to Dr. Nordmann of Odessa. On examining an opaque crystalline lens, extracted by Baron von Gräfe, and partially covered by its capsule, he detected in the *liquor Morgagni* two very small and delicate rings, which, with the aid of a microscope, he recognised as convoluted *Filariae*. One of them had been wounded, probably by the instrument used for opening the capsule, and the intestinal canal was seen protruding at the rupture. The other was entire, about three quarters of a line long, and through the transparent integument could be seen a straight intestinal canal surrounded by convolutions of the oviducts. In a lens extracted by Dr. Jungken, Dr. Nordmann afterwards found a living *filaria*, five lines and a half long.

A larger species of the *Filaria* has been noticed in the eye of the horse.

12. *Filaria bronchialis*.

A third species of *filaria* infesting the human body, was detected by Treutler in the enlarged bronchial glands of a man. It was about an inch in length, slender, and emitting the male spiculum from an incurvated obtuse anal extremity. Treutler

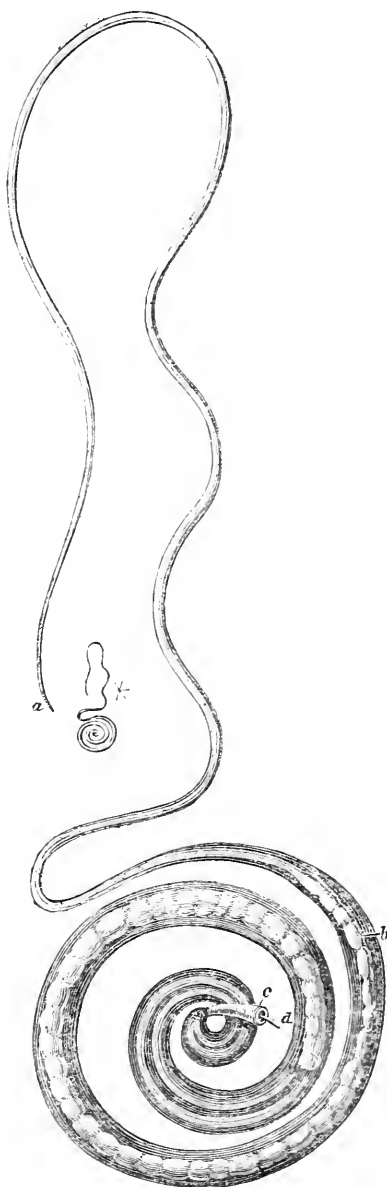


Filaria bronchialis, magnified.

is the only pathologist who has met with the *Filaria* in this situation in man; but in many animals the bronchial tubes are often loaded with worms of this species.

13. *Tricocephalus Dispar*,

(from *τριξ*, *τριχος*, *hair*, and *κεφαλη*, *head*.) or *Trichuris*, as it was formerly called. This entozoon was only discovered towards the close of the last century. Like the *Filaria*, it is characterized by an orbicular mouth, but differs from it in the capillary form of the anterior part of the body, and in the form of the sheath or preputial covering of the male spiculum. The *Tricocephalus* measures from about an inch and a half to two inches, and its capillary portion, which makes about two-thirds of the entire length of the animal, is transversely striated, and contains a simple straight intestinal canal. The thick part of the body is spirally convoluted, and plainly exhibits the intestine which is here sacculated, and terminates in an obtuse anal extremity, from which projects the spiculum and its sheath, in form like an elongated cone, and attached by its apex. The corresponding extremity in the female possesses a simple foramen, which, like the outlet of a cloaca, serves the office both of anus and vulva. The head of the worm is acute, with a small simple mouth. It is necessary to observe that many writers, and amongst others



Tricocephalus Dispar.

* Natural size.

- a, Capillary anterior extremity.
- b, Intestinal canal.
- c, Preputial extremity.
- d, Spiculum.

Dr. Baillie and Dr. Mason Good, have fallen into the error of describing the long attenuated part of the animal as the tail.

The *Tricocephalus Dispar* inhabits the large intestines, and principally the cæcum; but it occurs occasionally in the small intestines. It is often found adhering by its small or anterior part to the mucous membrane, the remainder of the animal being lost in the canal. It is in consequence of this circumstance, and the minuteness of their size, that they are seldom observed in the stools. This species of worm, which infests many of the malarialia as well as man, exists sometimes in great numbers.

In the winter, at the commencement of the year 1761, the period at which this worm was first discovered in Germany, an epidemic broke out in a corps of the French army stationed at Göttingen. Röderer and Wagler, who have given us an account of this epidemic, under the name of *morbus mucosus*, having frequently found numbers of the *Tricocephalus* in the dead bodies of the soldiers who died of the disease, were induced to regard these animals as the cause of it. Now it is remarkable that M. Thibault, who has recently investigated the cholera morbus during its fearful ravages at Naples, in a communication lately made to the Royal Academy of Medicine, at Paris, states, that in the examination of more than 150 bodies of persons, all cut off within the fourth or fifth days of the disease, he found more or less capillary injection of the mucous membrane of the intestinal canal, hypertrophy of the follicles, a considerable quantity of a thick and viscid mucus, and large numbers of the *Tricocephalus*, together with the *Ascaris lumbricoides* and *Vermicularis* in the intestines. The unexpected discovery of this species of worm, in cases of cholera, which is due to Ramaglia, was thought at first to account for the disease; but M. Thibault and his *confrères*, acting with greater caution, and in a more philosophic spirit, than their German predecessors, Röderer and Wagler, extended their researches to persons dying of other diseases than the epidemic, when it appeared that in the inspection of the bodies of eighty individuals, dead of various affections, these worms, together with the viscid mucus, enlarged follicles, and preternatural injection, were found in all of them without exception, although they were present generally in less quantity than in the bodies of cholera patients. These worms have been detected in Germany, since they were first observed, in conjunction with most forms of disease, and in neither of the epidemics alluded to can their existence be fairly regarded in any

other light than as an accidental coincidence, though they may have tended to modify and complicate the diseases, for the cause of which they were mistaken.

No symptoms characteristic of the presence of the *Tricocephalus* have hitherto been noticed, and they appear to reside quietly in the cæcum without exciting much uneasiness.

MALIGNANT FEVER OF BRITISH GUIANA.

To the Editor of the Medical Gazette.

SIR,

THE newspapers have from time to time informed us that a fever of a very malignant character was raging in that rich and important colony, British Guiana. As I am not aware that any account of this fever has appeared in our medical publications, and as the mortality in the colony was at one time so great as to excite considerable alarm, I think the inclosed extract of a letter, lately received from my much-valued friend, Dr. William Fraser, health-officer of the colony, will be perused with considerable interest by many of your readers. The facts brought forward by Dr. Fraser are, perhaps, the strongest that were ever adduced in confirmation of a certain variety of the yellow-fever being highly contagious—at least they leave no doubt on my mind that the late epidemic in Demerara was not only contagious, and propagated only by contagion or infection, (I know not which is the best term), but that it certainly was introduced into the colony in consequence of the quarantine laws having been suspended at the urgent request of the merchants of George Town (the capital), because they led to some trifling expenses; and as alleged, “that their ships were *needlessly* and *uselessly* detained, merely because they came from places where contagious or infectious diseases were prevalent, though they had not a sick man on board.” Now I believe, sir, there is no doctrine more firmly established than this—that a man may convey contagion from one person to another without in any way suffering from the disease himself, and therefore maintain that we ought never in any instance to allow the crews of ships, coming from infected places, to have free intercourse with the inhabitants of the port at which

they arrive, until at least some precaution has been taken to render the danger of their causing infection less probable. Dr. Fraser is a physician of great experience, having practised in Demerara for more than twenty years. He, of course, had often treated every variety of the colonial fever, and bilious remittent, so prevalent in that land of mud; yet he confesses he had never seen the *yellow fever* until the late epidemic made its appearance. I can testify from my own experience, that the bilious remittent of Demerara is often characterised by that yellowness of the surface which is sufficient to entitle it to the name of yellow fever; but it seldom happens that any thing like black vomit is observed, unless when the constitution of the patient is very much impaired; and then, when such a symptom did occur, the case always terminated fatally. Dr. Fraser himself had the epidemic in a very aggravated form, but luckily recovered; and the letter from which I send the inclosed extract was written during his convalescence; it is dated the 5th of October, at which period the disease had been for some time on the decline.

I have the honour to be, sir,

Your obedient servant,

ANDREW HALLIDAY.

Hampton Court, Dec. 29, 1837.

Extract of a Letter from Dr. William Fraser to Sir A. Halliday, dated George Town, Demerara, October 5, 1837.

"Having made allusion, upon more than one occasion, to the epidemic which has caused so much sickness, alarm, and destruction of human life, throughout this colony, I cannot resist the temptation of making a few comments in regard to its introduction and appearance in this community.

"In the latter end of Feb. last, the small-pox having made its appearance in the colony, the quarantine laws, so far as regarded the West-India Islands, were again revived and put in force; as this loathsome and pestilential disease had spread itself far and wide throughout every island of the Archipelago, in some of which the disease had proved exceedingly fatal to the lives of the inhabitants. During the latter part of the year 1835, and the early part of 1836, the yellow fever, with its terrific offspring, the black

vomit, were raging and spreading dismay and death throughout Surinam. In February and March, 1836, I put vessels arriving from Surinam in quarantine, for which I was attacked and shamefully abused in the columns of the *Guiana Chronicle*. From Kerinane, during the summer months of the same year (1786), this dreadful pest found its way to the island of Barbados, where it was instrumental, in a few short months, in sending hundreds of the ill-fated inhabitants to the grave.

From having read a great deal, and seen *nothing*, of the yellow fever—one author pronouncing it contagious, another not—I was myself sceptical as to its real nature, or what claim it might have, or not have, to hold a place among diseases which are considered by all parties as contagious, or infectious. *Late experience* has, however, decided this question, and set at rest every doubt which *my mind* had formerly entertained on so important a subject to the pathological inquirer.

But to resume the history of the particulars connected with its introduction and spread in this colony.

From Barbados it found its way into this place, where it commenced its ravages among the ill-fated community in the early part of last spring. From a small beginning it went on progressively, until, in the months of May, June, and July, it had attained its *acme*, carrying death, sorrow, and lamentation, in its train, wherever it found an entrance. The centre of Water-street formed the focus in which it had, in the first instance, concentrated its pestiferous force; from whence, at a subsequent period, emanated the infection to every quarter and corner of the colony. Among the crews of the shipping the mortality was dreadful; some vessels losing half the number of their crew, others three, four, five, or six; no vessel escaping some loss in the number of its crew. Six, seven, or eight funerals, formed the daily average at this awful and calamitous period of the destroying angel's visitation. No shade or caste of colour, no peculiarity of country, made the least difference, provided there existed a susceptibility in the system to catch the infection. *New comers* were, at the first outset, the greatest sufferers, few of them escaping the deleterious effects produced by the fever in the course of

its progress to a final termination. Although the new comers suffered or sustained the greatest degree of mortality in the commencement, it at last carried off its victims from among every description of the human race inhabiting this province, who had communication with the infected. It is said, and I believe with some truth, that the African and his descendant suffered less by death than any other race domiciliated among us since the appearance of the fever. I have heard it stated that the *black vomit* is unknown among the sable race; but whether this is true or not I cannot, from my own experience, attest or contradict. I have had two or three cases of blacks under my care affected with the fever, but they soon recovered and returned to their usual occupation. Since the appearance of yellow fever amongst us, there have prevailed three different kinds or types of fever. First, the colony intermittent, arising from marsh miasma; secondly, the common bilious remittent, or seasoning fever, both of which are endemic, and neither of which are infectious; and, thirdly, *the intruder* lately imported amongst us from Barbados, which is, as I have already stated, *highly infectious*, as I think I shall prove to your entire satisfaction before I have brought the subject to a conclusion.

People, you are well aware, are, in respect to the nature or existence of infection, as much divided in opinion as they are in other matters, wherein the mind is allowed to wander from what truth and experience ought to teach, to the more intricate and out-of-the-path road of speculation and abstruse theory. There is no truth, however clear and self-evident its existence to the unbiassed and unprejudiced mind, but there are to be found theorists fool-hardy enough to deny and cast under foot. Both theology and the philosophy of the schools furnish ample and sufficient illustration of this assertion. It may appear a matter of wonder and astonishment to the mind accustomed to contemplate the works of creation and Providence, and bow down in humiliation and prayer to the Sovereign Lord of all, that men, rational thinking men, should be found who prided themselves in denying the God of heaven, and avowing openly, and with the most audacious effrontery, their atheistical

doctrines; yet such are to be found even in the heart of these countries where the gospel of Christ has spread its benign and celestial influence. It is not to be at all wondered at, then, that when men are to be found who act the part of a Voltaire, a Jean Jacques Rousseau, a Hume, or a Tom Paine, in denying the very essence of the groundwork of religion, that other men should be found equally strenuous in denying that contagion or infection does exist, and that it proves the fertile source through whose agency disease is engendered and communicated from the infected to those wholly free from infection.

The most bigotted non-contagionist, be he a Bancroft, a Jackson, a McLean, or a Burnett, must allow the fact, and of course the pathological axiom from thence arising, that, "*all diseases communicated from persons labouring under them, to those free from them, are infections.*" If this truth is admitted, then I maintain, and will prove from circumstances which came under my observation during the prevalence of the epidemic, (now, thank God, happily on the wane,) that *infection alone* formed the source and entire medium of its existence and propagation.

Two missionary parsons residing in Berbice, came down here on some business connected with their mission, in May or June last; having completed their business they returned to Berbice, the one feeling himself rather sick or indisposed, the other apparently well, or in his usual state of health. On their arrival at their respective homes, the one who left Demerara indisposed, got worse and died; the other got the fever, and black vomit also, and died likewise. The wife of the first deceased, who also happened to be the sister of the last, was in like manner attacked, taken sick, and consigned to her grave. From *these three* individuals the infection extended itself throughout the town and district of Berbice, causing great mortality in its progress from one part of the district to the other.

M. Schomburg, the traveller sent out by the Royal Geographical Society of London, returned to us here in June last, from his excursion up the Corantyne River, bringing with him, as his attendant, an Indian, indigenous to that part of the colony. Mr. S., be it remembered, had had repeated attacks of

the colony fever and ague, and the colony bilious remittent fever, not only while engaged in his exploring duties in the interior, but while residing in Georgetown, after his return from his former expeditions. On his late return, however, from the Corantyne River, he took up his quarters in the house of a druggist up town, who accommodated him with a bed and bed-chamber in which, two or three weeks before, a young man belonging to the establishment had died of yellow fever, and its terrific derivative, black vomit. Poor Schomburg and his Indian attendant had no sooner come within the pernicious influence of the fomites of the bedding, &c. of the room, than first he was seized, and soon after his attendant, with the yellow fever, from which they both made a very narrow escape. Many of the Arrowack and other Indians, who had come to town with their little commodities to traffic, caught the infection, returned to their sylvan villages, and not only fell victims to the disease which they had thus casually caught, but communicated the infection to others of their tribe who had not on that occasion left their homes, and of which some also died.

A lady, sister to a gentleman who conducts a large mercantile establishment in town here, went and paid a friendly visit to another lady, the wife of a neighbour of hers, whose husband lay very dangerously ill of the disease. I warned her of the very great danger and risk she was running of catching the infection. She, however, contrary to my advice, paid her visit. The consequence was what I foretold, that she was taken sick, and communicated the infection to the maid-servant who waited and slept in the same room with her. From mistress and maid the infection extended itself to the book-keeper, brother clerks, and every single individual about the store, which was in consequence for at least a couple of weeks shut up. I was their annual medical attendant; and I am happy, and truly proud to say, that I was fortunate enough not to lose one out of so many sick of the infection.

A ship arrives in the river; the captain tries by every possible means to avoid having communication with either the shipping or shore. This precautionary sort of measure does very well for about a week or a fortnight; but at the expi-

ration of that period the rigorous restrictions he has placed himself and crew under become relaxed, and they steal a visit to see some of their old messmates, who may perhaps be at the time very ill with the fever. The visitor sits and has a long yarn with the visited; he returns to his ship, his system having imbibed a sufficient dose of the infection, which, perhaps after a few days' illness of fever, may terminate in black vomit and death. The rest of the crew become smitten with the infection, and one, two, three, or more of them, may be consigned to the grave in the same way. An overseer of an estate comes to town with produce to be shipped; he visits some friend's house where the fever is raging; he returns to the estate, sickens, and dies; and his fellow overseers go through a similar ordeal, from having caught the infection from him. From this point the fever spreads itself to every family in the district. Every member of the medical profession, who from their avocations were necessarily exposed to the infection, have been attacked, and undergone a most severe fit of sickness; none of them, however, have died. The mortality has been great among the clergy, considering their very limited number; not less than seven or eight of them have gone to their long account.

If the facts I have adduced in regard to the manner in which yellow fever extends and spreads itself throughout a district of country, or a province, prove not its eminently infectious nature, I know not what will establish or prove it, either in respect to the yellow fever, or any other disease which is on all hands agreed to be propagated and spread by specific contagion or infection. The facts and illustrations I have given above correspond exactly with the manner in which *I have seen*, and it is well known, the small-pox, scarlet fever, measles, and typhus fever, are communicated from those labouring under either the one or the other of these diseases, to those who, *previous* to having had communication, had been free from sickness or disease, and were likely to have continued so had they taken the necessary precaution to have kept beyond the sphere of contamination or infection. In seven months, that is to say, from the month of January last till the end of August, the number of deaths in Georgetown *alone*, the Colonial sex-

ton reports to have been, within two or three of six hundred. This number of course includes the white inhabitants of the town, the seamen, the free coloured, and the apprenticed labourers.

RECOLLECTIONS OF CHOLERA;

ITS NATURE AND TREATMENT.

BY WILLIAM GRIFFIN, M.D.

Limerick.

The Cholera at Limerick.

No. III.

THE many harrowing scenes and incidents to which I was witness during the prevalence of this epidemic, have left impressions on my mind which I shall never forget. One may see much of the vicissitude of fortune, and even instances of appalling misery, in ordinary life, but it is in such wasting pestilences that the influence of the more powerful passions are thoroughly displayed, and human nature laid nakedly open to us in its strength or its weakness. Nothing struck me so forcibly on all occasions as the extraordinary resignation and deep religious feeling evinced by the very poor, in the most desolate situations. The temper in which they generally met the most frightful visitations, and beheld themselves suddenly stripped of every earthly comfort, might very well put to shame the bearing of those in far lighter trials, who descant superciliously about their ignorance and superstition. I remember one instance of this among many, which I cannot refrain from mentioning. As I was walking round the ward one night, administering medicines from bed to bed, my attention was attracted to the door, when I saw a poor widow, whom I had formerly known, tottering in with her only son, a fine boy of fourteen years of age, on her back. She laid him down, cold, blue, and shrunken, on some straw, and flinging herself at my feet, clasped her arms convulsively round my knees:—"My only boy, Doctor!" she exclaimed with broken sobs; "I have brought my only boy, my hope, my support; I trust him to you, and if he was ten thousand times dearer to me I'd trust him to you; save him for me; oh! save him for me, won't you? God will bless your efforts

if you try to save him, for I have only him in the world belonging to me." Having quieted the poor woman as well as I could, I hastened to get something done for her child, who was in a bad collapse. The most unrelenting attention was paid to him; but, unhappily, time after time as I moved round the ward, I found him getting worse. The wretched mother, sitting beside the straw on which he lay, watched me anxiously when I approached, ever repeating with beseeching look the only question of interest to her in the world, "Is there any hope, Doctor?" At length I came round again, and, though despairing of his recovery, was astonished to find him dead, for there was no new complaint of the mother—no fresh gush of sorrow when her last tie to earth was divided, that could have apprized me of the event. She was still sitting quietly beside his remains, waving her body backwards and forwards with a see-saw motion, and clasping and unclasping her hands softly, as if she feared to disturb him. When I stood beside the bed, she looked up at me and whispered faintly, "The Lord's will be done, Doctor! I wasn't to have him."

To me, who have never witnessed the progress of a pestilence which dispatches its victims so summarily, the various modes in which the patients die in cholera is a subject of considerable interest. Some were struck down by their own firesides without previous complaint, breathing their last, blue and cadaverized, within two or three hours; some who were under treatment, and did not appear to be in very imminent danger, when resting on the elbow to take a drink, dropped back suddenly, and died without moan or struggle; others seemed to wear out, and lose life so slowly and insensibly, that it was often difficult to tell in passing the bed whether they were living or dead. It was beyond description pitiable to see little infants, of a year or a year and a half old, in this condition. They did not cry or become peevish, as in ordinary illness, nor look as in health for the mother's attendance; but as if their little faculties were matured by the greatness of the calamity, seemed impressed with some sense of its awful nature, and exerted their feeble energies uncomplainingly to resist it. I remember one little fellow who, when lying

apparently insensible at the foot of his mother's bed, used to start up suddenly whenever he heard any one near him, call for water, drink greedily, and without taking the least notice of those about him, drop off into the same state again. With older persons, however, the appearances of death were more imposing than in infants. I have on several occasions, after a patient had as I supposed expired, been much startled to see the seeming corpse turn in the bed, and call in low whispers for "cold water." Even where they went far beyond this, and with every other sign of mortality were speechless and motionless, it seemed sometimes possible to call them, as it were, back again to the world by the application of some powerful stimulant. I saw one young girl in this state, who was several times brought back suddenly to life, by the application of flannels wrung out of scalding water to the chest. It is, indeed, a very remarkable feature in this extraordinary disease, that both the mind and sentient powers remain perfect as long as the most feeble spark of life remains.

The manner in which drunkards died struck me as peculiar and extraordinary, presenting as it did some of the phenomena observed to follow poisoning by prussic acid. The expression of the features usually remained very much the same as it was before death; the eyes continued open and glistening, and the body maintained the position it had been lying in, however different from that usually assumed in the last moments of existence. A woman, aged about 40 years of age, was brought into the hospital to me, one morning, by her own son. She had a wild staring look, appeared stupid, which is unusual in the disease, and was pale and pulseless. She had been, by her son's account, in a state of intoxication for three days, and was, I believe, then brought direct from the public-house in which he had found her. "She has been drinking, sir, continually," he exclaimed, "in defiance of all I could say; and if Hell was opening for her, she would still drink on!" I had her immediately removed to bed and attended to. In about an hour afterwards, when I went round the ward with the apothecary, she was lying on her side, her head resting in a very natural position on the pillow, and her eyes staring at me with a glistening

wild kind of expression. I sat down, asked her how she felt herself, and commenced filling out some medicine; but there was neither movement or reply, and she still eyed me with the same fixed, life-like glare. On shaking her by the shoulder, I found she was stone-dead.

The deaths of such patients as went into consecutive fever were as singular and various as of those who did not outlive the stage of collapse. It is hard to conceive how human life could come to its close in a more quiet way than it did with some who lay down and died, as if, a little wearied, they were only enjoying a refreshing slumber. This treacherous end was slow of approach and had its forewarnings. A beautiful blush first appeared on the cheek which the day before was of a corpse-like paleness—there was a constant drowsiness or disposition to sleep, and when addressed, the patient usually answered, perhaps with a smile, but always with a happy expression of countenance, "I'm finely, sir." On the succeeding day he was usually found in a still sounder sleep; but when roused up and questioned, returned the same reply. On the third day he was snoring—it was harder to awaken him, and though yet muttering "finely, sir!" to all inquiries, there was an appearance of stupor and wandering about him. On the fourth morning he was generally found insensible. How opposite and how shocking were the deaths of others, happily but a few! After outliving a long and deep collapse, they seemed to struggle into a kind of congestive, half insensible state, with the eyes upturned and reddish, the lips dark and frothy, and putrid blood ejecting from the bowels. Unable to swallow, or speak, or look up, their moans in this state were horrible beyond imagining; and when placed apart from less hopeless cases, they often writhed themselves off their straw, half across the room, as if in the agony of their last hour. I have known them continue in this state to the second or third morning, the horrible moan never leaving their lips night or day.

To get a perfect idea of a plague, we should, after all, visit the dead-house, especially on the first or second morning after the incursion of the disease, when, probably, the mortality and confusion have been greatest. It is like visiting the battle-field on the morning

after contest. On entering, one may behold naked bodies stretched in various positions, such as they chanced to fall into when heedlessly dung down by the hardened cot-men. None of the faces have the calmness observable in a short time after death by ordinary disease, but each exhibits some peculiar expression of the pestilence, or of the agony with which it closed. One, with pale and dragged features, looks as if he died after a violent struggle in some bloody combat; another has a quieter, yet painful expression, as if the hand of the assassin had laid him suddenly low; a third seems to have sunk under some painful and protracted illness;—a fourth resting against the wall in a corner, stares at you with glistening eyes, as if an insane consciousness lit up life again, and he was angered at your scrutiny. Then the revolting recklessness with which here and there they are heaped over one another—the mockery of all our strongest feelings and instincts—the familiarity with death which it betrays, and the recollection that all this was but the work of a few hours!

That startling phenomenon, automatic motion after death, which has been noticed both in India and on the continent of Europe, occurred but twice in the course of my attendance on cholera cases, and in both instances in St. John's Hospital. The apothecary, however, who was resident in the house, seemed to be more familiar with it, although he had not before reported the circumstance. In one of these cases the man had died in collapse, and the blanket was as usual thrown over his face until he should be removed to the dead-house. Some time elapsed before this was thought necessary, when the surgeon whose watch it happened to be observed the blanket stirring! His attention, and that of the nurses, was immediately excited, and to their horror and astonishment they saw the blanket gradually lifted, and the hand of the man moving with a tremulous motion, until it rested above the head. Soon after the leg at the same side was lifted up with the same quivering movement, and slowly crossed over the other, and subsequently it was brought back again.—The surgeon after this took the hand that was laid above the head and placed it on the breast, where it was before, but it was once more lifted up and deposited above

the head. There was little or no movement afterwards. The body showed no other sign of vitality all this time, and was in fact perfectly dead!

In the other instance, the medical attendants, while walking in the ward, were alarmed by piercing cries from the dead-house, and on running down, found the nurses aghast at the door, exclaiming in terrified accents that one of the patients who died was moving within. On entering to ascertain the truth of this story, they perceived a corpse, which was resting against the wall opposite the door, elevating and depressing its arms alternately. These motions continued for a few minutes; and on examining the body there was not the least sign besides that could be considered indicative of life. My brother, Dr. Griffin, of Pallas-Keury, was witness to an incident of the same kind, but differing in the nature of the motions. There was a crowd of people *wakeing* the body of a cooper, who had died some hours before of cholera, when the sheet was suddenly observed to move. One of the relatives immediately raised it, to ascertain the occasion of the movement, and to his utter horror saw the fingers of the corpse stirring. There was an instant scream throughout the room, and the company betook themselves to flight. When my brother, who was sent for, arrived, and examined the body, he found the man was perfectly dead; yet, on looking down at the hands, the motions were still going on—the fingers slowly contracting towards the palm of the hand, one after another, and again expanding. He then looked to the feet, and found the toes were contracting and extending in a similar manner: the motions soon after ceased.

ON SOME OF THE EFFECTS OF ENDOCARDITIS;

And particularly on the Scutiform Thickening of the Aortic Valves.

BY JOHN THURNAM.

As the peculiar morbid condition of the sigmoid valves of the heart, which has been lately described by Dr. Watson, in the pages of the *MEDICAL GAZETTE**, has, during the last fifteen months,

* *MEDICAL GAZETTE*, vol. xxi. p. 450.

frequently fallen under my notice, and as the conclusions which I have for some time come to, regarding it, differ somewhat from those of that gentleman, I am desirous of offering a few remarks upon the subject.

With respect to the normal structure of the valves in question, although I am disposed to believe that the bases are thicker and stronger than the upper free portions of the valves, yet I think it will be found that there is no portion of the valves into the structure of which the fibrous tissue does not enter. It would, however, appear that the fibres constituting this tissue are frequently separated from each other by rather considerable intervals, which are, of course, closed only by the investing fold of endocardium; and that such portions of the valves are, from this cause, much more liable to become ruptured than those which are adequately supported by fibrous tissue. This defective supply of fibres occurs, I believe, more often near the angles of attachment of the valves than elsewhere, in consequence of the fibres being gathered into bundles at these points; a circumstance which would account for perforations of the valves being more frequently met with in this situation.

The cribriform condition of the valves here alluded to, it may be observed incidentally, was known to, and depicted by, Senac*; but it appears to have been first *particularly* described, about the same period, by two independent observers—Dr. C. J. B. Williams and Dr. Kingston†; and, as Dr. Watson remarks, is not very uncommon. I can scarcely conceive that, in the extreme case of this lesion, which is given on the authority of Dr. Budd, the edges of the valves could possibly have remained uninjured, in the form of strings, unless we admit that the delicate endocardium forming them was supported by a bundle of tendinous fibres, which formed the free borders of the valves. I believe it will also be found, that in the healthy condition of the valves they are transparent throughout. The view which I have taken of the structure of the semilunar valves of the heart in man appears to be strongly supported by an examination of those of some of the larger ruminant quadrupeds. In

the bullock they are distinctly seen to be formed of bundles of tendinous fibres, more or less completely interlacing each other, but presenting the largest interspaces near the angles of attachment of the valves, from which the bundles of fibres diverge*.

There appears to me to be so much analogy between the endocardium and the proper serous membranes, that I cannot but conclude, with M. Bouillaud, that the phenomena which are manifested by it when the seat of inflammation, have a corresponding analogy to those which, under similar circumstances, are presented by the pleura, the pericardium, and the peritoneum. That the lymph, and other more fluid products of inflammation, are neither so frequently nor in such large quantities met with on the surface of the endocardium as on that of the other serous membranes, cannot be matter of surprise; and it is even remarkable that the current of the blood should, so often as there is reason to believe it does, leave any such traces of inflammation undetached. From three or four marked cases of acute recent endocarditis, which I have now had an opportunity of seeing, I have little hesitation in stating that the true anatomical character of this disease consists in a beautifully delicate stratum of semi-transparent, granular, red lymph, of greater or less extent and depth of colour, closely adherent to, but capable of being separated from, the subjacent endocardium.

In two of these cases rheumatic pericarditis had also existed, and a recent false membrane, having the above characters, was met with in considerable patches in the left auricle, stretching on to the surface of the mitral valve. In both of these cases, the scutiform thickening of the aortic valves, closely resembling Dr. Watson's second figure, was also met with. In the third case the endocarditis had its seat in the inferior or ventricular surface of the anterior large portion of the mitral valve, and extended also, in a diminished form, to the ventricular surface of the aortic valves. This was in a case of aneurism

* *Traité du Cœur*, 1783, tom. i. pl. 19.

† *MED. GAZETTE*, vol. xvii. p. 887 and 955; and *Med. Chir. Transactions*, vol. xx. p. 90.

* Since writing the above, I have satisfied myself, by repeated examination of both the right and left sigmoid valves in the human subject, that their structure is such as I have described; but I may observe that this is most easily verified by an inspection of their internal or arterial surface, and especially by viewing this with the assistance of transmitted light.

of the aorta, of singular interest; and which, I believe, will be shortly communicated to the profession by Dr. Roe.

It appears to me to be the more necessary to allude particularly, on the present occasion, to those cases of endocarditis which have presented a pseudo-membranous exudation, (which Laennec states, and I think correctly, to be the most unequivocal, and, with the exception of ulceration, the only certain sign of inflammation of the lining membrane of the heart), in consequence of M. Bouillaud, who of all authors has contended the most strenuously for the existence, and indeed for the frequent occurrence of this disease, having adduced scarcely a single case which displayed the unorganized or recent form of such an exudation. The "first category of cases of endocarditis" of this author*, contains thirteen cases which proved fatal during the first stage of the disease, or during that which, according to M. Bouillaud, is characterized anatomically by sanguineous congestion, by thickening, softening, and ulceration of the membrane, and by a *pseudo-membranous secretion*.

Upon looking these cases carefully over, I do not find more than one (42.) which makes any pretensions to the last-named incontestible character of recent endocarditis; and even in that, I am strongly inclined to believe that the "*caillots albumineux*" were in reality a deposit from the blood. I cannot agree with M. Bouillaud that it is an error of trifling importance to mistake a discoloured and adherent sanguineous concretion for a pseudo-membranous production, inasmuch as the former, there can be little doubt, are frequently found in cases where there has never existed the least trace of inflammation of the endocardium. With the exception of one case (50.) in which ulceration is reported to have existed, the remainder of the cases placed in this class are chiefly characterised by redness of more or less intensity, in addition, however, to certain *chronic* changes, such as thickening and fibrous excrescences of the valves. Redness, taken alone, appears to be open to so many fallacies, that I cannot help concluding that several of the cases in question ought to have been referred to the

"second class of cases of endocarditis," which is characterised by the organization of the secreted products, or of the fibrinous concretions—by vegetations or excrescences—adhesions of the valves—organized patches of false membrane, in the form of white spots, on the internal surface of the heart—and by the hypertrophy of the endocardium and its subserous cellular tissue.

I have not unfrequently met with hearts exhibiting imperfectly organized false membranes, of the kind just alluded to, on the internal surface of the left ventricle in particular, which were capable of being raised from the endocardium beneath them, and which had doubtless originated in the transformation of patches of delicate pseudo-membranous exudation, such as I have described as having met with in the three or four cases already alluded to.

CASE.—I have a drawing by me at the present time, of the interior of the left ventricle, from a female aged 26, subject to rheumatism, who died rather suddenly after an illness of about four months, in which the chief symptoms had been pain, palpitation, extreme breathlessness, and a double sawing sound and purring tremor over the aortic valves, at the last only accompanied by very little œdema of the ankles: pulse varying from 90 to 120, very thrilling. In this case the aortic valves were remarkably altered, by the presence on their ventricular surfaces of a thick layer of semi-organised lymph, which was here and there accumulated in the form of irregular masses, by which the valves were soldered together to a considerable extent. The valve which was the most extensively affected in this way was likewise perforated by a round hole, about a line in diameter; but independently of this, the changes were of such a description as not only to have acted as an obstruction to the course of the blood, but also to have permitted its regurgitation. Immediately beneath the valves there was a large white patch, underneath which a bristle was introduced; and this being pushed forwards with the slightest force, came out at the free margin of one of the valves, having insinuated itself between the valves and the superimposed mass of false membrane. There was likewise contraction of the mitral orifice; and in the left auricle, near the border

* *Traité Clinique des Maladies du Cœur*, par J. Bouillaud. Paris, 1835; t. ii. pp. 9 and 171.

of the valve, there were two or three small recent patches of granular lymph, being the fourth case in which I have observed its presence. The heart was considerably hypertrophied and dilated, especially the left ventricle; and there was a thick white spot on the pericardium, covering the point of the heart.

From the cases I have above alluded to, as well as from the examination of numerous others, in which the products of the inflammation had undergone various changes, I have no doubt that in endocarditis lymph and serum are, as in the case of the other serous structures, deposited on the free surface of the membrane.

In some particular cases, and I believe especially in those in which, from some cause or other, there exists an obstruction to the circulation of the blood, I think that the effused patch of lymph may serve as a nucleus for the deposition of the fibrin of the blood, and that in this way may be explained the formation of some of the *larger forms* of excrescences and vegetations*. This, however, I should regard as an accidental complication of endocarditis, and not at all as an essential result.

It may be observed, that whilst the morbid products which result from these inflammatory actions have their seat on the free surface of the endocardium, there are other different adventitious productions, such as steatomatous, atheromatous, cartilaginous, and osseous matter, which are deposited *beneath*, or, in other words, on the attached surface of this membrane: just as it is observed that cartilaginous and osseous plates are often developed on the attached surface of the pleura, and other serous membranes.

If we admit the view which, I hope not on insufficient data, I have ventured to offer of the phenomena of endocarditis, and in which I have chiefly followed M. Bouillaud, there appears to me no difficulty in explaining the peculiar morbid condition of the aortic valves, which has been so well described by Dr. Watson, and depicted in Mr. De Morgan's Sketches.

Every time the heart acts, during its diastole, and when the sigmoid valves are engaged in closing the arterial orifices, certain portions of these valves are

necessarily brought into apposition with each other; and I believe it will require very little consideration to perceive that these are precisely the "thin crescentic portions" spoken of by Dr. Watson, and that the festooned line, to which he has drawn attention, forms the exact boundary between that portion of each valve which is presented towards the ventricle, and which is actually engaged in closing the mouth of the artery, and that other portion which is in coaptation with the corresponding part of the adjoining valves. The festooned line in question is doubtless the result of hydrostatic and mechanical laws, and where not obviously present, might in any given case be indicated with mathematical accuracy, could we appreciate the precise force of the ventricle, and other co-operating influences.

In case of these valves becoming the seat of acute or subacute inflammation, and having, as the result of this, a stratum of lymph effused on their ventricular surface, it is easy to conceive, or rather it is what would *a priori* be expected, that this lymph would be rubbed off those "crescentic segments" of the valves which during the diastole are brought into mutual apposition, and that it would become accumulated in the form of a ridge on those festooned lines which form the lower boundaries of these segments.

Pericarditis is a disease which strikingly exemplifies the modifying effects of motion and attrition on the appearances presented by the inflammatory products, and which may be here adduced analogically. Upon this principle we must doubtless explain the peculiar appearance of the surface of the heart, which varies so much in different cases of this disease, and which has sometimes been compared to the second or honeycomb stomach of ruminant animals, to tripe, or even, as by the older authors, to hair and wool—(*cor hirsutum, tomentosum, &c.*)

I find from my note-book that, during the last fifteen months, I have met with this *scutiform* thickening* of the aortic

* The "Globular Excrescences" of Laennec and "Fibrinous vegetations" of Bouillaud.

* The term "scutiform," which I here apply to this morbid condition of the valves, appears to be justified by the degree of resemblance which there is between the form of the thickened portion of the valves and that of some forms of heraldic shields. I was very desirous of retaining the term "festoon," introduced by Dr. Watson; but this cannot be done without much circumlocution. The term "festooned thickening"

valves, in a more or less advanced stage, in thirty-seven out of seventy-seven hearts taken indifferently from nearly all the cases which I have had an opportunity of examining during that period. In only one or two of these cases, do I find that the pulmonic valves were also affected, and then only in a much less degree. In a considerable proportion, and in all in which I attempted the proof, I found that this peculiar form of thickening was dependent upon the presence of a scutiform pellicle of false membrane which adhered closely to the ventricular surface of the valves, from off which, however, it was capable of being peeled more or less completely, either with the finger-nail, or the edge of the scalpel. When the false membrane had been in this way separated from the valves, their surface was left comparatively healthy, and all trace of the festooned line was destroyed.

In several of the cases the false membrane covering the valves extended beyond their bases to the surface of the muscular substance of the heart beneath them. Whilst in a large proportion the thickening was slight, and had not proceeded farther than that represented in Dr. Watson's first figure, in others it equalled, or even exceeded in extent, that represented by his second, and the festooned line, as there represented, was studded with minute granules of lymph. As a further confirmation of the view which I have taken of this pathological condition, I may remark that, in a few of the cases, and especially in one which it has occurred to me to observe since the publication of Dr. Watson's paper, the valves had gained an adhesion to each other for the extent of a line or two from their attachment, the medium of which adhesion was evidently to be found in the layers of false membrane covering the valves. I may also state, that I have never observed it on the upper or arterial surface of the valves, *only on the ventricular surface*.

The cases in which I have found this condition the most marked, were generally those in which some other more or less important disease ex-

isted, such as white spots on the pericardium; thickening and contraction of the mitral valve; a peculiar form of hypertrophy of the lining membrane of the left auricle; and different degrees of hypertrophy and dilatation of the cavities of the heart. I have, however, met with it in cases in which no other lesion was detected, and I believe that, after very slight alterations of the thickness and capacity of the heart's cavities, it must be regarded as the most frequent pathological alteration of this organ.

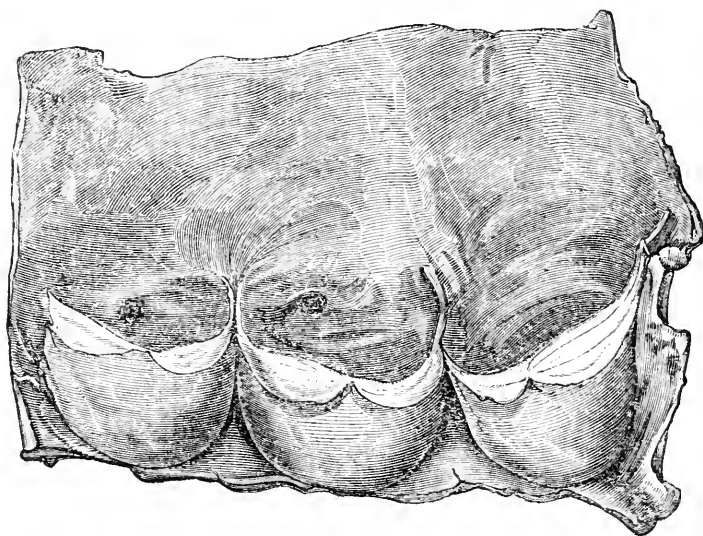
That I might leave no means unresorted to for determining accurately the nature of this lesion, I performed the following experiment, which had for long suggested itself to me, and to which I was naturally led by the view which I had taken of the subject, and which has been above explained. I took the commencement of the aorta of a bullock, with its valvular apparatus complete, and after having equally distended it with glazier's putty, so as, with the valves, to close the mouth of the aorta, I painted their inferior surfaces, remaining *in situ*, with a strong solution of gelatin, coloured with vermilion. Upon laying open the aorta after this had dried, I found that I had in this way produced an artificial specimen of the festooned thickening, closely resembling, as the accompanying sketch [see next page], by my friend Mr. Wm. Gerrard, will shew, that which occurs naturally as a pathological change.

It may be observed, that the mechanism of the production of this peculiar form of thickening of the aortic valves, which I have advocated, is equally applicable, whether the matter producing it be traced to an exudation of coagulable lymph, as I have contended, or to a deposit of the fibrin of the blood.

It is remarkable that a condition of the aortic valves, which is so very far from uncommon, appears to have escaped the notice of two such accurate observers as M. Bouillaud and M. Bizot. Since the foregoing observations were written, I have had the pleasure of perusing the admirable "Recherches on the Heart and Arterial System," by M. Bizot*. It is gratifying to me to find that many of the views which I have now taken are supported by the investigations of

would be incorrect, for the portions of the valves included by the festooned lines are those which are not implicated in the thickening. The term "scutiform" may perhaps be received with the less hesitation, as it has already found a place in anatomical nomenclature, as well as in that of another department of natural history.

* Mém. de la Soc. Méd. d'Obs. 1836, t. i. p. 262.



an observer of such great merit as M. Bizot, who has examined with excessive minuteness the hearts of 156 subjects, upon which his memoir, which is richly stored with facts connected with the pathology of the heart, is founded.

Although he does not allude to the festooned arrangement which has now chiefly claimed our attention, yet I feel little doubt that he has been describing this lesion under the head of "Alterations of the tubercles of Arantius," in speaking of which he alludes to "minute albuminous bodies" in the immediate neighbourhood of these tubercles, which are "round, reddish, polished, and half-transparent, and evidently of recent production." In a more advanced stage they pass into "an opaque and more solid or even cartilaginous state." M. Bizot met with the cartilaginous form of this alteration 37 times in 121 cases*. Dr. C. J. B. Williams has likewise alluded to this condition of the sigmoid valves, under the term of "the thickening or hem of the tubercle of the Arantius†."

I may just observe, that I should not suppose that this condition of the valves, even when in a very extreme form, could of itself give origin to symptoms of disease; but I have no doubt that it has been and still is very frequently passed over in dissection, in cases where

there is chronic disease of the heart, and in which these valves are reported healthy.

Westminster Hospital,
Jan. 11th, 1838.

ON THE SO-CALLED CHOROID GLAND OR CHOROID MUSCLE OF THE FISH'S EYE.

To the Editor of the Medical Gazette.

SIR,

ABOUT five years ago I examined the structure of the body in the eyes of the fish known by the name of the "choroid gland," or "choroid muscle," and obtained the following results:—

When the sclerotica is removed from the back part of the eye of a fish, of a cod (*gadus morrhua*) for example, a silvery-like membrane is seen covering the posterior surface of the choroid coat. Tear this off, and a dark red swollen body, somewhat of the shape of a horse-shoe, and encircling irregularly the entrance of the optic nerve, will be exposed. This body, the relief of which can be seen before the removal of the silvery-looking membrane, is the so-called *choroid gland* or *choroid muscle*. Its extremities meet, but are not incorporated together, at the line or fissure which runs from behind, forwards,

* Op. cit. 362.

† Med. Gaz. vol. xvii. p. 890.

along the lower half of the eye. A fine aponeurotic-looking expansion covers the body.

A great number of large vessels are connected with the outer edge of the so-called gland or muscle, and a little careful examination will show that vessels enter by its inner edge also.—How do these vessels comport themselves? Examined with a magnifying glass, it will be discovered that at either edge the vessels, after subdividing to a considerable degree of minuteness, anastomose with each other, and form a network. If now we subject the central substance of the body to microscopical examination, we shall find it is composed of an aggregation of minute straight vessels running across and connected by their extremities with the plexus on either edge of the body.

Close by the optic nerve two blood-vessels are connected with the eye; one of these, thicker in its coats and a little larger than the other, an artery, after having entered the eye divides into two branches which run along the inner edge of the body, giving off twigs which by their subdivision and anastomosis form the net-work already mentioned at the inner edge. From the minute plexus at the outer edge vessels emerge, which run together to form larger twigs. The latter enter pretty considerable trunks, which at first skirt the outer edge of the body, but afterwards turning away from it, proceed forwards and ramify in the choroid coat.

The vessels which emerge from the outer edge of the body have very thick fleshy-looking coats. The minute vessels forming the central substance of the body, have also thick coats; on account of which circumstance, and on account of their straight parallel course betwixt the two net-works, the body, when torn, presents a fibrous structure. And this structure, on account of the red colour, (which is derived, in a great degree, though not entirely, from the blood globules the vessels contain), simulates very much the appearance of muscular fibre. It is easy to inject with mercury the artery entering the eye as far as the network, on the inner edge of the body; but I have not found the mercury go across to the network at the outer edge. Sometimes, however, a minute globule may be pressed across.

After this description there can be little doubt but that the so-called cho-

roid gland or muscle of the fish's eye is a tissue of vessels—that it is in fact a *rete mirabile*. But this is not the only *rete mirabile* in the eye of the fish, for the arteries as they proceed forward in the choroid, many of them subdivide into small branches, which again unite into one, thus forming secondary and more simple *retia mirabilia*. Lastly, the arteries of the choroid terminate in a starlike manner, and the radiating twigs of these stars inosculate together.

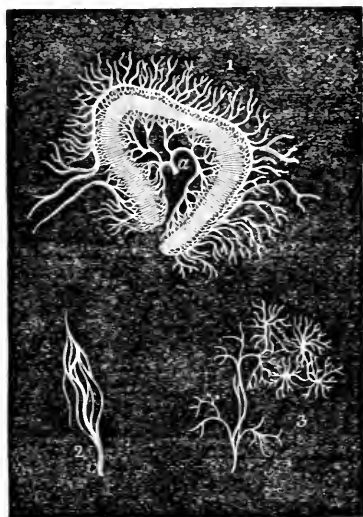


Fig. 1 represents the so-called choroid gland or muscle, or, as I would now call it, the great *rete mirabile* of the eye of the cod. Its posterior prominent surface is sliced away to a level with the entrance and exit of the vessels, in order to show the network at its edges, and the straight vessels between these networks, forming the central substance of the *rete mirabile*. It is to be remarked that the whole body is delineated of the natural size, but the lines representing the straight vessels are drawn in a gross diagrammatic manner: *a* is the artery at its entrance into the eye, subdividing into branches.

Fig. 2 is a magnified representation of one of the secondary *retia mirabilia*.

Fig. 3 is the star-like terminations of the vessels in the inner layer of the choroid, also magnified.

At the time I made the above observations, I thought of communicating them to the public, but I observed in

Carus's Comparative Anatomy a reference to a communication made about thirty years ago to the Royal Society of Göttingen, by Dr. Albus of Bremen, in which he also had compared the so-called choroid gland to a *rete mirabile*. I was anxious, before publishing any thing on the subject, to learn how Dr. A. described the structure of the body, but I could not procure the volume of the Göttingen "Gelehrte Anzeige," in which Dr. Albus's communication is noticed. I therefore thought no more of the matter. I now only write this at the suggestion of my friend Dr. Mackenzie of Glasgow, in order to confirm Dr. Albus's views of a structure which is still very much misconceived;—witness the most recent account we have of the Anatomy of the Eye, by Dr. Jacob, in Part xi. p. 205, of the Cyclopædia of Anatomy and Physiology. To call it, with De Blainville, a *vascular ganglion*, does not help us much.

I have since seen the report of Dr. A.'s communication in the 2nd volume of the "Gelehrte Anzeige" for 1806: no details of structure, however, are given: it is merely said, "The middle of the three membranes which, in the fish's eye, take the place of the choroid, consists in this species (the cod) of a most beautiful net-like plexus of vessels, the roots of which have their origin in a prominence of a horse-shoe shape, altogether peculiar to the fish's eye, and which was formerly so obscure. It has been considered by many zootomists a gland, by others a muscle; but according to the accurate researches of Dr. Albus, it is only a convolution of considerable blood-vessels, and is consequently to be looked upon as a kind of reservoir of blood (just as a *rete mirabile*)."

In conclusion I would observe that the demonstration of the above structure, and some peculiarities in the distribution of the veins outside the eye, which I have not now time to notice, remove from Mr. Alexander Shaw's reasonings * regarding the relations between the respiratory function, the eyelids, and vasa vorticosæ, the negative support which he seeks to draw from the eye of the fish.—I am, sir,

Your obedient servant,

T. WHARTON JONES.

Newman Street, Oxford Street,
Jan. 10, 1838.

TRANSPORT OF THE WOUNDED.

To the Editor of the Medical Gazette.

SIR,

THE transport of wounded from the scene of action is an affair of no small importance and difficulty in the operations of all armies, not only involving the lives and welfare of the maimed, but sometimes affecting directly, at others indirectly, the whole corps-d'armée. It is a subject, therefore, to which I have given much consideration during several campaigns in Portugal and Spain, with a view to determine, in my own mind, the best and most efficient plan, and put it to the proof of experience.

Twelve of Cherry's carts were sent out with the British Auxiliary Legion, at Mr. Guthrie's recommendation. They were employed on several occasions in the neighbourhood of Vitoria, and in all the actions in which the legion was engaged in Guipuscoa.

Upon the whole I found them serviceable, and much may be said with justice in their favour; but they are also liable to objections of some importance.

Small as they are in size, calculated only for four wounded capable of sitting upright, in heavy roads they become too much for one horse or mule, unless a very powerful one. It is rare also that a single draft mule will draw well; it was almost invariably necessary to employ two whenever we turned off the main road.

The shafts and the wheels seemed scarcely proportionate in strength to the work of the cart: in heavy roads they broke down in several instances, and at each time either in the shaft or the wheel. It is precisely in such situations, in narrow, deep, and hilly roads, where there is no possibility of flinging a broken-down vehicle out of the direct line of march, that such an accident is sure to cause great delay and confusion, by rendering it impossible for anything in the rear to pass or advance a step. Any other accident would only be to the detriment of the four wounded, but one of the nature pointed out may affect the march of a whole column, if more than one move by the same line, and at all events interfere with the closing up of the rear for hours, and of course, if

an active enemy be hanging on the skirts, is likely to compromise all that may thus be unavoidably detached from the column.

There seems to me another objection relative to the wheels, but upon which I speak less confidently. Speed can never be an object in these carts, but rather a capability of progression, however slow, under the worst circumstances of time, roads, and weather; this is of the last importance. I fancy, therefore, that if, at the same time that the wheels were made stronger, they were also somewhat increased in width of rim, they would cut less deeply into heavy roads, and be less liable to stick fast, and create a stoppage, which—however short it may be, as all military men know—is among the worst evils which can befall a column *en route*.

As to the remedy proposed, however, it will be for abler mechanics than myself to determine how far it is calculated to answer the end proposed.

The mechanism and construction of the carts in many other points—the slinging of the cot, the covering, and the mode by which the cart may be used, either with springs or without—I think excellent. As to the general question of how far they are adapted to supply the transport for wounded, required by an army on active service, I have formed an opinion unfavourable to their general—perhaps I should rather say—exclusive use.

A general action, even when but a few thousand men are engaged on each side, if at all well sustained, will often produce from 500 to 1000 wounded. To remove this number from the field, supposing one-half to be slight—and this is a very favourable proportion—the number of carts would be enormous,—sufficient to fill a town of themselves, and extend and encumber a line of march for miles.

A single campaign, however, is sufficient to prove to any one the importance of having means readily available for the transport of wounded from the immediate scene of action, without allowing a legitimate excuse for any part of the effective force being so employed. In all armies, I fancy there are a sufficient number of men who, although they may have passed their previous lives without manifesting any sympathy for the misfortunes of their neighbours, suddenly, on a field of

battle, become wonderfully compassionate to a wounded comrade; and never is this sympathy so strongly displayed, perhaps, as when there is a check or the probability of a reverse,—precisely the moment when their presence on the scene, and not their sympathy for their comrades going to the rear, is required. As battles are not generally fought upon plains, but upon some vantage ground, often steep or wooded hills, I believe the most rapid and effective mode of withdrawing the wounded from the scene of action to any point in the rear previously fixed upon, is the following, which I had opportunity of putting to the proof more than once, our deficiency of mules alone rendering it generally impossible.

A brigade of mules, say twelve to each column of 2000 rank and file, and four of Cherry's carts placed on the main road, or by the side if possible, at the point nearest to the centre of the column engaged, each mule having a bat-man, a chair or seat on each side (commonly used throughout the Peninsula, and forming a part of the pack-saddle), will in most cases be found capable of picking up the wounded within a few yards of their post, and without delay. The carts, from the smallness of their size, may often approach close to the rear of the force engaged, and pass along its line, although there be only a cross-road, or even none at all; and they would thus take up at once officers or men with fractured limbs, or who may be otherwise too seriously wounded to be able to ride or sit. These are the means of transport from the *immediate scene of action*, for at or near the point from whence they started should be a relay of spring waggons, capable of containing from twelve to fourteen wounded, and conveying them to the first ambulance or hospital station.

In this manner I think Cherry's carts might be employed with great advantage, as peculiarly fitted; and in proportion as the means of the corps-d'armée to which I was attached enabled me to make the arrangements described complete, I have always had the satisfaction of seeing the wounded more speedily transferred to the hospital station, and the number of sympathising and aiding comrades greatly diminished. I once observed, in less than an hour, a whole Spanish battalion tail off after some fifty

wounded! one carrying his comrade's musket, and another his little finger.

A better contrivance than the "jolters," or spring waggons, hitherto used in the British army, might, I think, be easily found; but I am firmly convinced, that whatever their construction, they should be adapted for carrying at least twelve wounded men. And upon the same grounds I think the proper use of Cherry's carts is to circulate as close as possible along the rear of the scene of action, for the purpose of removing men so dangerously wounded as to require a stretcher, and eight, twelve, or sixteen men from the effective force; and such a number may now and then be seen accompanying a wounded officer far into the rear.

The British Legion, which in the first instance was organized in all its departments closely upon the regulations and system of the British service, offered many difficulties to the medical officers upon whom devolved the duty of organizing general hospitals, and directing the service on the field.

A series of changes were effected in this branch of the service, under the sanction of the Inspector General, Mr. Callander, and authorized by the Lieutenant-General commanding; the results of which were beneficial in the extreme, —I believe in every way, I may confidently state, and acting not less directly upon the inmates of the hospitals than upon the effective force in the field.

There might be objections to some of these innovations in the British service, for many were adopted with reference to the peculiar and temporary nature of the force and other circumstances which, affecting the legion, would not have exercised any influence upon a portion of the British army. Nevertheless, I am strongly impressed with the conviction that the principle of some of these changes might be acted upon with manifest advantage in a more permanent service; and at all events the facts resulting from their adoption on service may claim some attention.

The first, perhaps the greatest difficulty experienced, was the impossibility of obtaining fitting men for the various and responsible duties of ward-masters, clerks, orderlies, &c. from the different corps composing the force. No commanding officer willingly sends for hospital service his best men, were there

not even a prevailing and very erroneous idea among military men, that any class of subjects will do for an hospital. Thus, if there be an incorrigible thief, a drunkard, or a skulker, the first requisition that arrives for an orderly, he is dispatched. And if a good man by any chance finds his way into the hospital establishment, no pains is spared by a zealous Colonel to get him back again, even although he may have learnt all his duties, and become familiar with the routine, in spite of his being declared indispensable in his situation. It matters not—he is a good man—*ergo*, the regiment wants him; and nine times out of ten the regiment gets him, and the hospital in exchange receives a stupid or a drunken fellow, or both, totally ignorant of all the duties, and as unwilling to learn as he is incapable of doing so.

Wearied at last by the perpetual recurrence of these annoyances, and convinced of the impossibility of establishing a regular and efficient system in the general hospitals while labouring under these disadvantages, I obtained permission to form two small corps, or companies, for the service of the hospitals and the transport of the wounded in the field. The first consisted of fifty rank and file, the skeleton of which the Inspector General had formed in England as conductors for the carts; these were generally able-bodied men, and received artillery pay. The second, or "hospital corps," was selected principally from the weakly men of all the regiments, and from the wounded who remained partially disabled. These men had infantry pay, and neither corps had any extra allowance. The two companies were commanded by a Lieutenant, under the orders of the Head of the Department; and a Deputy Purveyor of Hospitals, with 2s. 6d. additional pay per diem, was appointed Paymaster, and was enabled to perform the duties of that office as well as those of Deputy Purveyor.

The men were selected with reference in some degree to their trades; so that if the carts required repair, or any minor work in the hospitals, they were always at hand, and were the means of saving the government much expense, and the hospital administration delays—inevitable in Spain. From this last company the hospital staff were

allowed each a servant. The whole of the two companies were instructed in their duties, the wardmasters and clerks being selected from the "Ambulance Transport Corps," and the orderlies and inferior servants from the "Hospital Corps," with the prospective advantage, if their intelligence and conduct merited it, of promotion to the Ambulance Transport Corps. The conductors, and a small guard in charge of the carts, medical stores, &c., when sent to the lines, or on the field, were furnished from these last.

A month had not elapsed after these arrangements were made before the general hospitals assumed an improved appearance. The utmost order, regularity, and cleanliness, was maintained in every division: every man soon knew his duty thoroughly; and being under the entire control of the medical department, equally quickly learnt the necessity and advantage of strictly conforming to his instructions. The hospitals became a source of pride to all connected with them, and the General commanding often took occasion to express his satisfaction when visiting the wounded.

How far it might be possible or advisable, on service, in a British army, to form a corps of this kind, by selecting from time to time those who become weakly by climate and chronic complaints, or in consequence of flesh wounds, which often prevent the perfect use of an arm or a leg for months after they are healed, is a question upon which I do not presume to decide. But I feel assured, and the French have proved by long experience the fact, that a corps peculiarly and solely destined for the transport of wounded and the service of hospitals, is not only highly advantageous to the sick and wounded, but it appears to me a politic measure in every point of view. When properly formed and directed, such a corps must greatly tend to produce three important results: 1st, To prevent great straggling to the rear, of effective men accompanying the disabled, which not only weakens materially the actual strength of a battalion under fire, but tends also to produce a disorganizing effect, more or less strong in proportion to the character and discipline of the troops, and the nature of existing circumstances. 2d, To insure the more prompt and efficient transport of the wounded. 3d, A more efficient system of hospital treatment and admini-

stration; for as a surgeon, however zealous, cannot do every thing with his own hands, the result of his treatment depends, in no unimportant degree, upon others. Thus there would be a saving of human suffering, and the more prompt and certain cure of the patients. Not only this, but such a measure will remove many sources of irregularity and injury to the service, arising from the incessant struggle between the opposing wishes and interests of commanding officers, who are naturally intent upon the strength and efficiency of their corps, and medical officers not less anxiously bent upon the efficiency of their establishments.

To prove that these evils existed, not in the British Legion alone, but have always been experienced, and often commented upon, in the British army, I confidently refer to the testimony of the British officers who served in the Peninsular war; for from the lips of not one, but a hundred, I have heard them detailed. Why should not this be remedied? The Canadian war offers an excellent opportunity of trying, on a moderate scale, some new and better system.

Your obedient servant.

J. RUTHERFORD ALCOCK.

Bury Street, St. James's,
Jan. 10, 1835.

THE IRISH APOTHECARIES' HALL.

To the Editor of the Medical Gazette.

SIR,

A LETTER has been lately published in the Dublin Evening Mail, headed "Queries for Apothecaries," and subscribed "A Graduate in Medicine, Surgery, and Midwifery, of Dublin, Edinburgh, and London."

The main design of the writer of this letter appears to have been twofold. 1. To throw odium upon the apothecary branch of the medical profession, and 2d, to promote thereby the attainment of an object at present desired by a party in the Irish College of Surgeons, by degrading it and its members from the station they have hitherto held. With this design I feel myself no further concerned than other members of the medical and surgical professions at large; nor should I have thought it necessary to notice the letter, had its author been content to confine

himself to the discussion of the question at issue between him and the apothecaries. I am not in their counsels, nor am I their advocate; the task of justifying their proceedings I leave to themselves; and I only hope that, for the sake of the common profession, of which they constitute so useful and so respectable a portion, they will continue, as they have commenced, to defend themselves, without forgetting that something is due to the station which they claim to hold in the opinion of society.

In the course of his letter, however, the "Graduate" has thought fit to arraign the conduct of those persons who have become professors in the school of the Apothecaries' Hall, and to charge them with perjury. Such an accusation is, you will allow, too grave to be lightly regarded, or to be submitted to in silence by those who would claim credit for honourable feelings and action. The place which he holds in the opinion of others is to every man too valuable an object to be forfeited without just cause, and above all, in a profession the only distinction of which is public esteem. You will therefore, I doubt not, admit the validity of the motives which induce me to undertake the exculpation of the professors of the Hall School from an imputation which I hope to prove to be as devoid of foundation, as, if well founded, it ought to ensure the united condemnation of the profession and the public.

The charge against which I am about to protest, is contained in the following extract from the letter of the "Graduate":—"Your attempt at the Hall to establish a school, where *renegades from the sworn interests and honour of our surgical and medical professions, and colleges are to give lectures.*" Before proceeding further, I must pause for a moment to guard against a complaint of misrepresentation, and ask, what is to be understood by "sworn interests and honour?" Does he mean that the interests and honour have been put upon oath, or that they have been proved upon oath? These, the legitimate interpretations of the words as used, cannot have been intended, and I am forced to supply what must have been the writer's meaning, else the passage is pointless—viz. interests and honour to which the renegades have been sworn. At the same time I would suggest to the "Graduate," that in future, when he may

please to appear before the public as a censor, it would be well that he wrote with more accuracy both of style and meaning; a total disregard of which is displayed in many other parts of his letter besides that just adverted to; or, failing in the ability to do so, that he submitted his lucubrations to some one competent to correct them, in order that his readers may be spared the labour of unravelling his mysteries.

Now, sir, I ask, how have those who have been elected professors in the school of the Apothecaries' Hall, become thereby renegades to the honour or interests, whether sworn or otherwise, of our medical and surgical professions and colleges? In plainer words, how have those professions and colleges been dishonoured, or their interests deteriorated, by their members becoming professors to the Apothecaries' Hall? What is there in their honour and interests which causes them to be impaired by that which advances those of others? Is it not to the honour of every institution, that its members should be selected by others, as the fittest and best qualified individuals to discharge important offices; offices whose title to respect is independent of extrinsic circumstances, and which of themselves reflect honour upon those to whom they may be entrusted? Is it not to the advantage of every institution, that opportunities for utility and advancement should be opened to, and conferred upon, its members? And if so, how can their members dishonour or injure them thereby? There must surely be something peculiar in medical institutions which thus converts to poison what should be healthful and beneficial. Let us, then, inquire what obligations exist which have been violated; what interests are at stake, which are likely to be affected?

That we may the better understand the question, I will state the case as it is. The apothecary is compelled, as well by the wants of society as by the position in which he is placed by others rather than by himself, to act in the double capacity of the prescriber and the dispenser of medicine; and the Hall have, accordingly, long required the candidates for its license to attend certain courses of medical lectures. The number of these courses has been from time to time increased, until, at present, a certain certificate of attendance upon

each of the branches of medical science included in the ordinary medical and surgical curricula, is required. In conjunction with the extension of their course of education, they have established a school, and elected a body of professors to teach the several branches which candidates are expected to study; and because certain members of the Colleges of Physicians and Surgeons have sought and obtained chairs in this school, they are stigmatised by the "Graduate" as perjured renegades. I ask again, by what obligations are we bound, which we violate or disregard, whether as physicians or surgeons, by becoming professors in that school, and by lecturing therein upon our several departments? I know of none whatever, save only those which we took upon ourselves when we became fellows, members, or licentiates, of the colleges to which we belong. And what are they? They are comprised in the forms of oath and declaration prescribed by their charters for the fellows, members, and licentiates of the Colleges of Physicians and Surgeons; and are so similar for both, that those of either College will suffice for our purpose. I take those of the College of Surgeons, as more express and stringent. That for fellows, or members, is as follows:—"Well, truly, and faithfully to attend and execute his and their several and respective office or offices, place or places, duty or duties." What obligation, I ask, does this impose, which can be interpreted to forbid a member of either College from undertaking a professorship in the school of the Apothecaries' Hall, or any other honest occupation (no matter what) which is not incompatible with the discharge of such offices, or duties, as the College may intrust to him? Provided he "well, truly, and faithfully executes his duties," may he not do what else he pleases, and honestly can? I leave the answer to the "Graduate," only adding for myself, that I neither have neglected, nor will I so long as I continue a member neglect, the faithful execution, according to my judgment, of any office which the body may commit to me; and I say the same for my colleagues. The oath imposed upon licentiates is as follows:—"I do solemnly swear that I will observe, and be obedient to, the statutes, by-laws, and ordinances of the Royal College of Surgeons, and

that I will, to the utmost of my power, endeavour to promote the reputation, honour, and dignity of the said College; and that I will not, at any time hereafter, practise, follow, or pursue the business or profession of an apothecary, or druggist, or sell drugs or medicines, within the City of Dublin, or at any place within ten miles thereof. So help me God."

It cannot be necessary to insist that we do not pursue the business of an apothecary by lecturing in a school established by the Apothecaries' Hall. And there does not exist statute, by-law, or ordinance, whether of the College of Surgeons or Physicians, which forbids a member or licentiate from becoming professor in any school which may be open to him. On both these counts, therefore, we stand acquitted. But it may be said, that by so doing he does not promote the honour and dignity of his college. In what, I ask, does he detract from them? Does he do so by winning the preference over members of other bodies, by his talents, his industry, or his acquisitions? May he lecture in the pettiest, the meanest school, which private resources can establish, and not detract from those attributes of his College?—and may he not, consistently with a due regard to them, become a professor in one established by so respectable, so influential a body, as the Corporation of Apothecaries? It were but trifling to entertain seriously such a question. He not only does not detract from, but he advances the character of his College, by his election to the office. How, I would ask, is a member of any public body to promote its honour? Is it by being idle, and not exerting the talents with which he may have been endowed? Such a course might serve the purposes of others—perhaps of the "Graduate;" but it would reflect little credit whether upon himself or his College. Is it by allowing the members of other institutions to be preferred, or to appear to the public as preferred, for places of public service, in which scientific qualifications are considered the legitimate title to success? Such, again, is well calculated to promote the reputation of those Colleges, but not of his own. No, sir, such are not the modes by which the members of a public body can best advance its character: but the more useful, the more eminent, each man renders

himself, by the honest and legitimate exercise of his professional acquirements, the more does he promote the reputation of the institution with which he is known to be connected; the more does he raise it in public esteem; and in the selection of its members by another body, as the best qualified for the discharge of an important public duty, a compliment is paid to it through him.

Away, then, with the calumny—it does not deserve a milder name—the unfounded, the disingenuous calumny. It is plain that the honour of the Colleges is in nowise compromised by the conduct of their members, who have been elected as the most competent individuals whom the Apothecaries' Hall could procure as teachers for its school; but that, on the contrary, their honour and character have been maintained and advanced thereby.

But, say some, (and I cannot refrain from meeting the objection) “it is derogatory to a physician or surgeon, and to his College, that he should be connected with an Apothecaries' Hall, or that he should lecture to apothecaries. So far as we are personally concerned, I am entitled to reply, while I thank them for their disinterested solicitude,—Surely that is a matter for our consideration, not their's; they are not the worse; they ought, by contrast, to be the higher. If we take a different view of the subject, are we not at liberty to judge for ourselves? But what is there in an apothecary, or in his calling, to render a connexion with him or with his corporation degrading to the physician or the surgeon, or their respective colleges? Is it degrading to a barrister, or to the profession of which he is a member, that he should be counsel or law-officer to an Apothecaries' Hall? I trow not. And if not, why should it degrade either of the others to be a scientific professor to the same corporation? Let the Graduate answer. Again, I ask what is there in the apothecary which renders it derogatory to the other branches of the medical profession to hold a scientific connexion with his Hall? What difference is there between them? The apothecary is not so highly educated; and he compounds, dispenses, and sells medicine. Well, those differences seem likely soon to be at an end, at least between the apothecary and the surgeon. The former has determined to be well educated, and who

shall prevent him if he choose to be so? The latter seeks to make himself an apothecary, if he can. How appropriately, then, comes the objection, from men, who are now endeavouring to render the College of Surgeons more respectable, more exalted, and more dignified, by converting it into a corporation of drug-venders. Forsooth, its member may, with all due regard for his own and its honour and dignity, compound and sell a sixpenny draught to the pauper, who cannot give him a fee, and therein find remuneration for his professional service; but he is degraded by lecturing on a science to the men from whom those honourable gentlemen would wrest their means of livelihood if they could. Out upon them, out upon them—grovelling guardians of the science and honour of surgery! The objection comes well from others also. The time has not yet passed; and it seems to be now ordained that it never shall pass, at least in Ireland, when the physicians regard the surgeons, as a body, as much beneath themselves, as do the surgeons the apothecaries; yet it is surprising with what avidity licentiates and fellows of the College of Physicians have sought professorships in the school of the College of Surgeons when open to them, and yet they talk of the degradation of holding a professorship in the school of the Apothecaries' Hall! But, they say, the School of the College is a chartered school. A chartered school indeed! Chartered, in an Irish sense of the word, *i. e.* a school without a charter. Where is its charter? It has not one. The School of Physic has a charter, but it has not. Albeit, it has been set forth in medical almanacks, as one of two chartered medical schools, with which Dublin is provided, and as “established by Act of Parliament.” It was established by a body having a charter; and so has been the School of the Apothecaries' Hall; but no more: that charter was originally granted, as I will show in a future communication, for another purpose, and contained no mention whatever of a school, from one end or to the other.

Again, forsooth, it is debasing to teach Apothecaries! Is it not a little surprising that this was not sooner discovered? The Hall have, for some time past, required of the candidates for its license attendance on anatomy;

and where did they attend before the foundation of its own professorship? Through all the schools of Dublin, and probably not least in that of the College of Surgeons. Yet no complaint was made. No man felt his dignity in the least diminished by lecturing to apothecaries. And why? The "Graduate" can doubtless explain. Can it be that, while the Hall was content to impose lectures, and thus swell the classes of existing schools, it was well; but soon as it dared to imitate the example which had been set by others, and to establish a school which might intrude as a competitor into the field, that then was discovered the enormity, the degradation, of teaching a pupil, who intended to be an apothecary. The "Graduate" will do me a favour if he will also explain how it came to pass that at the opening of the same session which saw the professorship of anatomy in the Hall school first entered on, the profession and the public were edified with an introductory lecture from the anatomical chair in the school of the College of Surgeons, the object and end of which were, the high crimes and misdemeanors of apothecaries, and the necessity, which had been discovered to exist, for converting the college into a corporation of apothecary-surgeons. It was not said that the classes of the College school had declined, and that unless recruited they were likely to decline more. It was not said that the school of the Hall threatened to cope with that of the College, and that, now the lecturer's own province had been invaded, a *coup d'état* must be accomplished, which would at once annihilate not only its prospect of success, but also that of his more formidable rival of the University and the School of Physic. It was not said, that if the College of Surgeons could obtain the power of licensing apothecary-surgeons, the classes of the school, which is supported by its money, its influence, and its sanction, would be filled anew, while the benches of the School of Physic, and of the Hall School, neither of which is admitted by the College, must be abandoned and empty. None of these things were even hinted at, and, of course, they did not enter the calculation of the lecturer, whose only motive was the honour and

dignity of the College, and the welfare of the profession and the public.

Alas! how weak and fallible an attribute is reason, when blinded by intemperate zeal or by overweening self-bussness. A member of the Colleges of Physicians or Surgeons cannot, according to the "Graduate" and his abettors, advance himself by the honest and independent exercise of his talents, in a school established by a public body, and in an honourable position, perfectly compatible with his relation to his College, without being renegade to its honour; and yet our censor subscribes himself a "Graduate of three Colleges," while at the same time he stands forth the intemperate partizan of one, and the vilifier of another of the three. How, let me ask, is such conduct to be reconciled with a due regard to the interests and honour of those corporations? Perhaps he may say, my signature is only assumed and fictitious. I answer, it matters not: light objects indicate the current of the air; and this circumstance, trifling as it may be, proves either how little principle is concerned in the matter, or how dead to its obligations is he who voluntarily places himself in such a position.

Having, as I trust, satisfactorily shewn that the honour of their professions and colleges has been in no wise affected by the conduct of those physicians and surgeons who have become professors in the school of the Apothecaries' Hall, I postpone till my next the consideration of the obligations imposed upon them by the interests of these institutions, upon the true nature and present condition of which I hope to be able, at the same time, to throw some light.

Meantime I have the honour to remain, with all due personal respect, of course, for the "Graduate of three Colleges,"

Your obliged and obedient servant.

A PROFESSOR IN THE SCHOOL
OF THE APOTHECARIES' HALL
OF IRELAND.

Dublin, Jan. 9, 1835.

[The writer of the above has given his name, and any one who may choose to answer him must do the like. — Ed. Gaz.]

ON THE FORMATION OF PUS.

To the Editor of the Medical Gazette.

SIR,
I hail with pleasure the appearance of the papers of M. Bonnet, of Lyons, on pus, which you have so recently published, because it encourages me to hope that more correct opinions will, ere long, be entertained on this subject than those which now prevail. I see that M. Bonnet has proved by an interesting series of experiments that the composition of pus is "the same as that of the blood, minus its colouring matter."

If there is any credit due to this discovery, I must beg to be allowed to put in my claim for at least a share of it, upon the ground of having published the same opinion nearly three years ago. If you will have the goodness to refer to your No. for May 2, 1835, page 142, you will find it there stated as the result of an inquiry into the mode in which pus is formed, that "pus is not a secretion, but a simple effusion or exudation, and yet not so much a straining of the juices from the blood as the blood itself, *minus its colouring matter*." I have even gone farther than M. Bonnet; for in the succeeding paper (in the number for May 9), I have not only shewn the order in which the constituent parts of the blood are effused, but also the manner in which the globules of the blood become deprived of their colouring matter. Those papers were written nearly two years before they were published, so that my claim to originality is clear enough; but if I were to re-write them, I could press the last point very much more strongly; so much so, indeed, as to leave no rational doubt remaining about it.

As the papers I have alluded to contain the only *complete* revision of the whole subject of inflammation (not an unimportant subject, surely) which has been made within the last half century, nearly with the view of ascertaining how far Mr. Hunter's doctrines are affected by the various physiological and pathological facts which have been gradually accumulating during that time, I had expected that they would have excited some little attention. Probably, if I had proclaimed the discovery of some hydro-pyro-fuliginous extract of

calcined cockatrices, as wonderfully efficacious in the cure of cancer, &c., there would have been anxiety enough to know the proper dose; but it seems that an honest endeavour to improve the scientific part of our profession, from which general rules may be obtained, applicable, when properly modified under particular circumstances, to all cases, is received with indifference, and treated with neglect.

I have the honour to be, sir,

Your obedient servant,

JAS. WM. EARLE.

Loretto, Cheitenham, Jan. 11, 1838.

CASE OF HYDROPHOBIA*.

To the Editor of the Medical Gazette.

SIR,

As you have expressed a wish to have further particulars of the case I lately communicated for insertion in the MEDICAL GAZETTE, I submit the following:—

Salivation was induced by rubbing ʒj. of the ungt. hydrarg. fort. manè noct. on the inner side of each ankle. I have stated that the dread of fluids decidedly came on before there was any "dérailonnement." With regard to the medical treatment in this case, if we consider the loss of blood from leeches and phlebotomy, the total abstinence from food, solid and liquid, since the Saturday, it may be supposed that the draughts had a powerful effect. I have said that he took the first at 10,—then the pulse was perceptible; but at 12 it could not be felt, and he then appeared insensible. At half-past 12 the pulse was again perceptible, and he took the second draught: during the night he took altogether five. In the morning he was pulseless and insensible, and much altered from what he was on the previous day: then his mental faculties were greatly increased,—he was watchful and irritable: now he appeared insensible to what passed around him. It was then, and not till then, that he became incoherent.

I must say something to account for

* For the history of this case, see MEDICAL GAZETTE for December 30, 1837, p. 533.

the difference of opinion between the other medical attendants and myself. The fact was, they staid with the patient only a few minutes, merely to feel his pulse, view his tongue, and to ask a few questions; though I repeatedly stated to them that it was impossible to determine the nature of the case unless it was narrowly watched, and seen often. When I say the medical attendants, I must except one (Dr. Scholefield), who, indeed, staid one whole hour, in order to see the patient in a paroxysm of convulsions, but without success; and he refused to attend the case further, because a junior medical man had felt the patient's pulse before him, the senior medical man.—By some inadvertence the printer has misspelt my name.—I am, sir,

Your humble servant,

WILLIAM DU HEAUME,

M R C.S.L.

Jersey, Jan. 12, 1833.

PERNICIOUS EFFECTS OF ENAMEL CARDS.

To the Editor of the Medical Gazette.

SIR,

As you devoted several pages of your last number but one to the subject of arsenical candles, which are not demonstrated to have proved destructive to human life in a single instance, although it may be highly probable that they would be injurious, under certain circumstances, if brought into general use, I venture to consider you as not unwilling again to undertake the office of guardian of the public health in a much more flagrant case; and it is the more necessary that our respectable medical journalists should take upon themselves such duties, as we unfortunately have no responsible board or council to whom these are officially confided. I wish, then, to bring to your notice an evil which, if generally known, will, I am persuaded, make a strong impression on the public, and lead to the application of a remedy which at their hands alone can be obtained.

It will doubtless have fallen under your observation, that enamelled visiting and shop cards are now so much in vogue, that persons of fashion, and the

superior classes of tradesmen, scarcely use any other. The only advantages which, as far as I am aware, they possess over other cards, are, that they please the eye, and that they take a finer impression from a copper-plate, a thing of hardly any importance under the circumstances in which alone they are employed. The disadvantages attending their use are, 1st, that they blacken on exposure to mephitic air, which is often disengaged from common coal, as well as from the sinks and drains in private dwellings, and still more often from the gas in shops, which moreover, being always on ground floors, are particularly exposed to the noxious vapours arising from imperfect drainage. This defect in enamelled cards wholly precludes their employment for receiving the impressions of more permanent engravings. 2dly, That it is difficult to write legibly on them, whether in pencil, which will not mark, or in ink, which runs; so that one person cannot conveniently add his name to the engraved card of another; nor can visiting cards be used, as formerly, in writing addresses for boxes, parcels, or game. 3dly, That when slightly bent they crack and look unsightly; and that they cannot be applied, as old and otherwise useless cards often were, to the lighting of fires or candles, for they burn with great difficulty.

The advantages and disadvantages are thus, it seems, pretty fairly balanced; but why do I enumerate them, or wish to disturb the exercise of the general taste, in a matter apparently so unimportant? For a very cogent reason, to which it is the sole object of this letter to direct your attention, namely, that of the many deleterious employments carried on in this metropolis, the manufacture of these enamelled cards is the most deleterious. It is my belief, founded on inquiry, that the lives of many workmen are annually sacrificed to the production of this useless luxury; and that a far greater number are, from the same cause, more or less injured in their health, and unfitted, some temporarily, some permanently, for providing for themselves and their families. I would avoid the imputation of an overstrained sensibility, and I fully admit that the state of civilization in which we live renders necessary much risk of health in the exercise of the various arts that contribute to the supply of our

wants and comforts; but I do maintain, that, where the advantage to be gained is so paltry, common humanity should prompt us, one and all, to refrain from indulging in a luxury which can only be obtained at so heavy a sacrifice. It has been stated to me that the card-makers are only occasionally employed in making enamel, and that none of them would be thrown out of work if not another card of this kind were ever used.—I am, sir,

Your obedient servant,
M.D.

London, Jan. 13, 1838.

MEDICAL GAZETTE.

Saturday, January 20, 1838.

“Licet omnibus, licet etiam mihi, dignitatem
Artis Medicæ tueri; potestas modo veniendi in
publicum sit, dicendi periculum non recuso.”

CICERO.

CASE OF SUPPOSED IDIOCY.

IN our last article we gave a sketch of the case of David Yoolow, a man who, on very slight pretences, was alleged to be an idiot, but who easily succeeded in disproving the flimsy evidence brought against him. We concluded with some quotations from the evidence of Dr. Christison, the chief witness for the plaintiff, who not only failed to show that Yoolow was, in the words of the brievé, *incompos mentis, fatuus, et naturaliter idiota*, but entirely broke down in the attempt to prove that he was deficient in good sense.

The next witness for the plaintiff was Dr. William Malcolm, who deposed that he asked Yoolow “if he had heard of the Reform Bill, and that farmers, of his extent of land, were entitled to vote? But this seemed to be all a blank to him*.” On cross-examination, it turned out that the witness believed Yoolow to have been the tenant of the farms and the two mills, since his father's death; and that he thought Yoolow was in

possession of the farm as tenant, when he (witness) said that he was entitled to vote under the Reform Bill. “If you had been aware that the farm is held by trustees, would you have considered Yoolow as entitled to vote? *I am not a lawyer, and am not able to answer that question*.*”

In his examination in chief this witness stated, that he had asked Yoolow how much the weight of a boll of meal was, and that he answered, “Sixteen pounds;” but on cross-examination it appeared that the question was really put in these words, “How much is a boll of meal?”

One of the strongest points in this witness's evidence against Yoolow was, that he could not tell how much 48 and 52 made, nor even 50 and 50. But, independently of the injustice of making arithmetical questions a sort of Procrustean bed on which to stretch every one's intellect, it appears, from the testimony of numerous witnesses, both for the plaintiff and the defendant, that Yoolow was in the habit of correctly answering arithmetical questions of a far more difficult kind, and this, although he had never been taught accounts.

It was also the opinion of Dr. Malcolm that there was no chance of Yoolow's recovering or improving. If by “improving” is meant increasing in knowledge, or being able to answer questions that previously had been too hard for him, we have abundant proof that Yoolow was capable of this, not only in the evidence of other witnesses, but in that of Dr. Malcolm himself, for he says, “At one of the interviews after the first, I resumed the subject of the Reform Bill, and he said he knew that the Reform Bill had passed. He also knew the price of meal. He appeared to have been told that†.”

* Report, p. 17.

† Ibid. p. 17.

A number of minor witnesses were then called, among whom was James Bruce, messenger-at-arms. We will give the whole of his examination and cross-examination, as they are not very long:—

“I was at school with David Yoolow, and considered him then a silly boy,—that was before he had his paralytic stroke.”

“Cross-examined by Mr. D. McNeill.—I will not swear that David Yoolow was not as clever as I was when we were at school together*.”

Next to the messenger-at-arms came a medical witness, who broke down most sadly in his attempts to convict the defendant of fatuity, as the following extract from his cross-examination may help to shew:—

“Did you put questions to him to try whether he knew the value of money? I did. What questions did you put? I put the questions to him, how many shillings were in five shillings, and how many shillings were in ten shillings; but he had difficulty in answering how many shillings were in ten shillings, though he said that ten shillings was half a pound. How did you put the question to him as to how many shillings were in ten shillings, or in five shillings? what words did you use? I used the words, ‘How many shillings are in ten shillings?’ and ‘How many shillings are in five shillings?’ And did he not know how many? He did not. Then he knew that ten shillings was half a pound, but he did not know that ten shillings was ten shillings: was it so? Yes†.”

Re-examination, and a second cross-examination, made the matter still more perplexed, so that on being asked what was the question he had put to David Yoolow relative to the five shillings, the witness was driven to say, “I am embarrassed, and cannot tell.” We are not at all surprised that a witness, though sensible and well informed, after falling into a mistake, should be so per-

plexed by examinations and cross-examinations, as to be unable to recover himself, and at last come to a deadlock; but we are surprised when we find that, in the examination of persons supposed to be of unsound mind, the slightest hitch or slip is immediately laid hold of, and triumphantly exhibited as a proof of their fatuity. The fact is, that the number of persons is very small, who have all their knowledge ticketed, labelled, and arranged, so as to be producible at a moment's warning. A few men, indeed, who have passed a large portion of their lives in examining, or being examined, may be expected to be ready with answers enough to satisfy a score of mad-doctors; but we may assume it as a general rule, that a plain man of retired habits, a young lady of fortune, and many other classes of persons, though not void of understanding, will be utterly unable to sustain the character of catechumen for ten minutes together. Like the medical witness before us, they will be obliged to say, “I am embarrassed, and cannot tell.”

A curious anecdote is told of Yoolow in the evidence of another witness:—

“He used to ask, ‘Are you a doctor? Manna (must not) doctors rise when they are bidden?’ I answered, ‘Certainly.’ Then David asked, ‘Must they do so when it's snawing (snowing)?’ ‘Certainly.’ He then turned to the maid-servant, and said twice exultingly, ‘I hae na (have not) to rise when it's snawing.’ The servant thereupon said, ‘Na, David, ye have not to rise when it's snawing; you're better off than doctors,’ which appeared to give him great delight*.”

This story seems to be cited by the witness as a piece of fatuity; to us it appears rather an instance of shrewdness, exhibited in the natural, but somewhat hard-hearted, triumph of this comfortable landholder over us poor doctors.

* Report, p. 23.

† Ibid. p. 24.

* Report, p. 27.

The plaintiff had thus pretty well put himself out of court with his own witnesses; nevertheless, several were called for the defendant, in order to make assurance doubly sure. The first witness was the Rev. James Flowerdees, who gave an account of a conversation which he held with Yoolow on religious subjects, and in which he acquitted himself to admiration. The students at some of our most celebrated seats of learning might really prepare themselves for their examinations with most of Yoolow's answers.

Dr. John Argyll Robertson deposed to having conversed with the defendant on various subjects; the general result was, that his memory was good, and his powers of comparison good likewise, though his means of drawing comparisons were, from the habits of his life, not equal to those of most men. He understood money matters very well; *e. g.* the meaning of a receipt; the effect, to a bank, and to the holder of a bank-note, when it is destroyed; the meaning of being a *cautioner*, *i. e.* bail, &c. The following traits, too, are not bad:—

“He told me that some medical man had been plaguing him very much, and that he had said to him he had better go home and look after his pills, and not fash (plague) him. I asked him something upon the subject of farming, and he said, ‘I know nothing about it, and you may as well ask me about Latin or Gaelic, which I have never learned.’”*

We shall pass over the evidence of several other witnesses, all exceedingly favourable to the sanity of the defendant, and come to that of Mr. Lowe, (p. 42-3) who found him able to answer many questions in arithmetic and money-matters. He knew how many pounds of beef, at 6d. per pound, a pound sterling would buy; a question which, though simple, would not be

instantaneously answered by all who have been instructed in figures; and he did not know whether two-thirds or three-fifths of any thing were greater; a question which would puzzle three-fifths, or even two-thirds, of those whose coats and hats are received as *prima facie* evidence that they are educated men.

The jury, without retiring to deliberate on their verdict, unanimously found that David Yoolow was not an idiot.

Many of our readers will be struck with the numerous points of resemblance between this case, and the one of Miss Bagster, which we reported at length, and on which we offered some comments, in our tenth volume. In both, the persons accused of being imbecile had gone on for years without molestation, until a change of circumstances made it desirable for some relation to get possession of their property. In Yoolow's instance this change was caused by the death of his sister; in Miss Bagster's, by a marriage which it was thought desirable to dissolve. Under the supervision of a doting grandfather, Miss Bagster's education had been extremely neglected, and she was allowed to learn what she pleased, which, as is common in such cases, was little indeed. Arithmetic is the favourite test in supposed cases of unsound mind, and unfortunately Miss Bagster, as appeared from her own evidence, had never been obliged “to do her tables;” when she felt unwilling to learn them, her grandfather used to send his compliments to the governess, that she might be excused. In like manner, Yoolow's education was neglected, on the erroneous supposition that his paralysis made him incapable of any; though the progress he made without assistance clearly shows what he might have done with judicious management. As it was, his attainments in arithmetic

* Report, p. 31.

tic were decidedly superior to Miss Bagster's; for though sometimes *floored* by easy questions, at other times he succeeded in answering those of a higher order. We may here remark, that their attainments were naturally of a different cast: the careful Scottish landholder, an invalid of more than forty years' standing, inclined to the grave; the petted London belle was more versed in the gay. Next to divinity, in which Yoolow's attainments were very considerable, he knew most about matters concerning his property—such as the sum he had at his banker's, the number of bolls of meal he ought to obtain in exchange for a cow, &c. &c.; while Miss Bagster talked of "the opera, Paganini, Sir W. Scott, and the exhibition*." Miss Bagster's definition of a marriage was not bad, though perhaps not the one to be expected from a grave moralist, or from David Yoolow. She said "she did not consider herself married, for a proper marriage was when they went to church with carriages, and had cake and wine, with favours and bridesmaids†." This manner of defining a marriage by some of its outward signs or ceremonies, is so far from being fatuous, or idiotic, that our readers will recollect, that in the Latin poets, *tæda* (which properly signifies a torch) is made to mean a wedding.

The great error of the witnesses, in both these cases, was the wishing to obtain answers from the supposed imbeciles on subjects in which they took no interest, and in which thousands of persons in their station would also have been found wanting. Thus, in the case of Miss Bagster, a learned Doctor of physic asked her what was the interest of 10,000*l.* four per cent. stock; a question which would pose half the young ladies in London.

Another point of resemblance between the two defendants, is, that they both improved during the examination; the hosts of professional and lay questioners serving as so many tutors in arithmetic and the other subjects of inquiry: plainly demonstrating that there was no incapability of learning. There is, however, one unfortunate difference between the two cases: David Yoolow was unanimously set free, while Miss Bagster was adjudged to be of unsound mind. More than five years have now elapsed since this mistaken verdict, and we would gladly learn if any thing has been done, in this long interval, to complete the education of this neglected girl. Has not the Lord Chancellor the power of directing that the education of nominal or supposed imbeciles shall be attended to?—and may we not imagine instances where the fingering of fifty or a hundred thousand pounds would be a kind of premium for keeping a flighty relation ignorant of the multiplication table?

DEATH OF MR. HENRY EARLE.

It is our painful duty to record the death of Mr. Earle, which took place the day before yesterday, at his house in George Street, Hanover Square. The illness which proved fatal to Mr. Earle commenced as an inflammatory sore throat, which soon put on an unfavourable appearance, and became attended with delirium.

Mr. Earle was a thoroughly straightforward honourable man, zealously devoted to his profession, and a general favourite among his brethren, by whom his decease will be very sincerely regretted. His death occasions a vacancy in the office of Surgeon to St. Bartholomew's Hospital, and also in the Council of the College of Surgeons.

* MED. GAZETTE, vol. x. p. 525.

† Ibid. p. 523.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Jan. 9, 1838.

THE PRESIDENT IN THE CHAIR.

The History of a Female who has Four Mammaræ and Nipples. By ROBERT LEE, M.D., F.R.S., Physician to the British Lying-in Hospital, &c. &c.

THE individual in whom the above-mentioned peculiarity presented itself was 35 years of age, and was prematurely delivered of a still-born child on the 21st of July, 1835. The mammaræ having afterwards become excessively painful and distended, she was compelled most reluctantly to permit the author to make an examination of them, by which it was discovered that she had two mammaræ and two nipples on each side. The inferior or pectoral mammaræ were fully developed, and in the natural situation; and their nipples, areolæ, and glands, presented nothing unusual in their appearance. Near the anterior margin of the axilla, a little higher up on each side, was situated another mamma, about one-sixth the size of the others. The nipples of these were small and flat, but when gently pressed a milky fluid flowed copiously and readily from several ducts which opened upon their extremities. When milk was drawn from the lower breasts, a small quantity usually escaped from the nipples of the upper; and when the draught came into the former, the latter invariably became hard and distended. From the flatness of the nipples of the upper breasts the patient had never been able to suckle with them.

The author considers the above case as furnishing one of the best examples on record of quadruple mammaræ in the human subject, and cites from several foreign authors some of the most striking instances he has met with of the same malformation: the cases present no material deviation from the one under consideration. The author remarks, that in some women only one breast has been developed; others have had two nipples placed on one mamma; and a few individuals have had three breasts, two in the natural situation, and a third between them. Only one case has been recorded of five mammaræ in the human subject.

A Narrative of a Case in which the Parotid Gland was extirpated on account of Disease, in the Toronto Hospital, Upper Canada.

By C. WIDMER, Esq., Surgeon to the Forces. Presented by Sir J. McGrigor, Bt.

An elliptical incision having been made in the integuments of the most prominent point of the tumor, its removal was effected without much difficulty, and with little loss of blood, the facility being attributed

by the author to the adoption of the method of separating the mass from the lower part upwards. The external jugular vein and external carotid artery being necessarily divided, were immediately secured by ligatures, the latter being tied at both ends. When the removal of the mass had been entirely accomplished, the styloid process, and the transverse process of the atlas, were exposed to view. The result of the operation was quite favourable, the wound being entirely healed in six weeks.

Two Cases of Tumors pressing upon the Superior Vena Cava, causing Edema of the Face and Superior Extremities. By J. WILSON, M.D., Physician to the Middlesex Hospital.

In the first of the cases here related the compression of the vein was the consequence of an aneurism of the arteria innominata, the sac of which was found to contain a firm mass of fibrin. The vena cava superior was in contact with the sac, and communicated with it by a small opening. Portions of fibrin were found in the vein, which was, however, still pervious.

The obstruction of the vena cava in the other instance related, arose from the pressure of a tumor of large size and anomalous structure, which completely surrounded the vein. The head of the pancreas was, in this case, enlarged to the diameter of four inches.

PHYSICAL SOCIETY, GUY'S HOSPITAL,

January 13, 1838.

DR. HUGHES IN THE CHAIR.

MR. NORMAN CHEVERS read portions of a thesis on *Arteritis*.

In making some introductory remarks upon the structure of arterial tissue, the author described a tunic which he had found distinctly intervening between, and perfectly separated by cellular media from, the lining and contractile portions of the great arteries.

He applied to this the term "aponeurotic," as it appears to be made up of two or three distinct layers easily separable from each other, and from the tissues which surround them, each lamina having a dull metallic aspect, nearly resembling that of fascia, and appearing to consist of many minute fibres disposed in the longitudinal direction of the tube, being easily torn into strips lengthways, and not admitting of separation when strong traction is applied in the line of the vessel's axis; and at last yielding only by a short fracture. This tunic appears to undergo considerable increase of development under certain conditions of arterial disease; its layers become more distinct, its fibres are

more tenacious, and may be seen behind the serous lining, passing longitudinal lines. It becomes hypertrophied, probably for the intention of defending an already dilated and weakened tube from the effects of inordinate distension.

In its normal condition, the aponeurotic tunic appears not only to offer a better surface for the attachment of the serous lining, and the distribution of its vascular supply, than could be afforded by a part of so active function as the contractile coat, but it must also assist greatly by its elasticity in enabling the tube to return from extension in a longitudinal direction, while it prevents it from yielding too greatly under force so applied.

In adverting to the subject of acute aortitis, and detailing the morbid changes induced by that state, distinctions were drawn between the various kinds of reddened marks which so often appear upon the lining arteries after death. As to the supposed effect of the imbibition of blood, the author related some experiments, from which he adduced that blood does not usually produce a stain upon tolerably healthy arteries until after the time at which the average number of examinations are made; and that the marks, when produced, are unlike those resulting from organic change. There is another state of partial reddening which cannot be ascribed either to an inflammatory or staining process. The author endeavoured to account for this appearance by surmising, that during life the main surface of the aorta presents a light pink hue. It has long been proved that in the moment of death the aorta becomes flattened, its opposite surfaces being placed nearly in apposition: during a *perfect* contraction of this kind the vessels supplying the serous lining would become emptied of their contents, leaving the surface, which they covered, perfectly blanched; but should any circumstance occurring during, or even shortly after the agony, as a failure of supply from the organic nerves at any point, prevent the cylinder from exerting or maintaining its tonicity at that spot, the vessel's internal surface would there retain its original aspect, while the remainder became entirely deprived of colour. After advancing facts connected with the symptoms of this disease, and the pathological changes which appear in connection with it, it was submitted that as almost every case of acute aortitis arises in constitutions rendered morbidly irritable by certain vicissitudes, and in conjunction with similar states of inflammation in tissue analogous to the arterial linings in general junction and organisation, the disease in that division of the circulatory system results not from transmission of disease from other parts, but

spontaneously, as an individual effect of general irritation and depression then existing in the system. Upon this ground he strongly deprecated the treatment followed by the French practitioners, the use of extensive depletory measures, as being followed in the majority of cases by aggravation of the disease, and often by death, evidently from prostration.

Considerable difference of opinion existed among the members as to whether the structure described by Mr. Chevers should be regarded as a distinct coat.

Mr. HILTON said that he had listened attentively to the observations contained in the paper, and had also examined the preparations sent round by the author; but was not satisfied that there was any thing either in the description or dissection which could justify Mr. Chevers' conclusions. Mr. H. was inclined to regard the tunic described as distinct, to be nothing more than the cellular membrane connecting the internal and middle coats made more distinct by disease.

Mr. DENDY supported Mr. Hilton's views.

Mr. KEY vindicated the claims of Mr. Chevers to the discovery of a new arterial coat, which was distinct from both the internal and middle coats in the arrangement of its fibres. The preparations which he (Mr. K.) had seen, had satisfied his mind upon the point.

Mr. KING and Mr. GREENWOOD also bore testimony to the completeness of Mr. Chevers' observations.

On the motion of Mr. TWEEDIE, seconded by Mr. KING, a vote of thanks was carried to Mr. Chevers for his very valuable communication.

At the next meeting of the Society, Jan. 27th, Dr. Addison in the Chair, Mr. Aspland will read a paper *On some Diseases of the Heart*.

IRISH COLLEGE OF SURGEONS. PHARMACY LAWS.

THE committee appointed by the college to prepare a bill for the regulation of the practice of Pharmacy, presented their report at an unusually large meeting of the college on Tuesday the 9th inst. The suggestions were agreed to without a dissentient voice, and there was but one opinion as to the necessity of adopting the most vigorous and speedy remedies to meet the present emergency, and to protect the public from the consequences likely to result from the insubordination of the persons appointed by law to prepare the medicines ordered by the members of the profession for the patients under their care.

The first provision suggested by the committee is to enable all qualified practitioners to prepare and administer the

medicines necessary in their practice. Not, however, for the purpose of deriving income from the sale of them, but to secure correctness, and to deprive the apothecaries of the opportunity of meddling or tampering with their patients. The next is to protect and encourage regular apothecaries for the service of those who do not wish to dispense their own medicines, who shall be compelled to attend to the business of their shops, and not engage in the practice of medicine and surgery. The third provision is to prevent all persons practising medicine or surgery from compounding the prescriptions of other practitioners, or in any other way encroaching upon the department assigned to the regular apothecary. The fourth is to visit with severe penalties all apothecaries who employ ignorant, uneducated, or inexperienced apprentices or journeymen, in compounding medicines; such practice being attended with the most lamentable consequences. The fifth is to punish, in a summary and exemplary manner, all frauds and abuses in the preparation or sale of medicine, either by the use of impure, unsound, or inert remedies; or the substitution of cheap and counterfeit articles for those which are genuine and expensive. It is also proposed to enable all apothecaries regularly educated and licensed in England and Scotland, to practice as such in Ireland, and to afford persons of skill and capital so qualified every protection and encouragement to supply the present deficiency in this department throughout Ireland.

The committee also stated that they had received communications from all parts of the country expressing satisfaction at the prospect of a remedy for the present abuses, and affording much valuable information and suggestions.

The committee finally expressed their conviction that the college would probably experience no serious opposition to the proposed measure, except from those interested in the perpetuation of the existing abuses.

PETITION AGAINST QUACKERY.

To the Editor of the Medical Gazette
SIR,

I AM requested by the Leeds Medical Society, to forward you the annexed petition, hoping, should it meet with your approbation, that you will give it a place in your valuable periodical.

I remain,
Yours respectfully,
W. STEAD TAYLOR,
Hon. Sec.

Leeds, Jan. 10, 1838.

The members of the Leeds Medical Society, consisting of legally authorized practitioners, having some time ago had their attention directed to the unprecedented and increasing extent of quackery, appointed a committee to inquire what means could be adopted for its suppression. On receiving the report of the Committee, and at their suggestion, the Society have unanimously determined to petition the legislature for that protection against illegal competition to which an honourable profession is entitled.

It is a fact, that in the borough of Leeds the number of prescribing druggists and other illegal practitioners considerably exceeds that of the regular members of the profession; and the same calculation is applicable to many other places. The uneducated druggists are now in the full exercise and enjoyment of all those privileges which were intended, by the Act of 1815, to be secured to the apothecaries; while the latter, after years of preparatory study and expense, have the mortification of seeing *their* harvest reaped by a set of illiterate pretenders.

The following petition, it is presumed, will point out to the legislature a sufficient remedy for the evils in question; and the Leeds practitioners earnestly exhort their brethren throughout the kingdom to second their efforts, by immediately sending up to Parliament similar petitions, signed by all the legally-authorized practitioners resident in the district where the petitions originate.

As this is a question touching neither sect nor party, it is hoped that all parties will unite in the endeavour to obtain justice, and that no time will be lost in urging upon the legislature the necessity of removing this oppressive wrong.

GEO. WILSON,
Secretary to the Committee.

Leeds, Jan. 1, 1838.

To the Honourable the Commons of the United Kingdom of Great Britain and Ireland, in Parliament Assembled.

This Petition of the legally-authorized Practitioners of Medicine, residing in the Borough of Leeds,

Humbly sheweth,

That a very considerable number of individuals are now practising the different branches of medicine in England and Wales, without having undergone any preparatory education, or examination by any legally-constituted authority.

That, in consequence of their ignorant administration of remedies, very serious injury is done to the public health, and many cases have occurred in which their practices have been the undoubted causes of death. Not a few such cases have been

already before the public, as matters for legal investigation.

That the extent of employment which such parties are enabled to obtain among the uneducated classes (by advertising false cases, and various other unworthy means) injures materially the practice of regular members of the profession; and is more especially oppressive to the younger practitioners, who generally depend for their subsistence upon the very classes who are misled by the false statements of pretenders.

That your petitioners are required by law to undergo an expensive preliminary education; and then to engage for several years in severe and unhealthy studies,—subsequently to all which, they are subjected to rigorous examinations by the constituted authorities. Yet the only protection afforded them is by the Apothecaries' Act; which, however, is totally inadequate to redress their grievances, on account of the difficulty of obtaining the required evidence of illegal practice—the great length of the legal process—and the heavy amount of costs attending prosecutions.

That it is therefore inexpedient and useless to waste further efforts towards the conviction of illegal practitioners, under the very insufficient powers afforded by the Apothecaries' Act.

That your petitioners, therefore, beg most respectfully to suggest, as the only means of remedying the above evils, and protecting the public from the dangers of ignorant and empirical practice, that a clause be added to the Apothecaries' Act, empowering magistrates to summon before them any party charged with illegally prescribing or practising any branch of medicine in England or Wales, and in the event of such party failing to produce the license of the Apothecaries' Society of London (or proof of having obtained such license,) to inflict such a penalty as may seem adequate to prevent a repetition of the offence.

And your Petitioners, &c.

—

MEANS OF DIMINISHING QUACKERY.

THE Committee of the Leeds Medical Society, appointed to inquire into the means best calculated to diminish the amount of Quackery, having given the subject their best attention, beg to suggest the following propositions to the society:—

1. That it is expedient to discontinue any further attempts to procure the conviction of individual pretenders; such attempts being calculated, even when suc-

cessful, to diminish the evil in a very partial degree only. As moreover they are generally unsuccessful, the Committee are of opinion that our efforts and time ought no longer to be wasted in that direction; and, in support of their opinion, beg to refer the society to their statement of what has been in vain attempted by them since their appointment.

2. That the Society of Apothecaries, however desirous they may be to protect their Licentiates from illegal competition, are deterred from attempting it by the difficulty of procuring the necessary evidence of illegal practice—the expense of bringing witnesses into court, and the great amount of costs, &c. in case of a verdict being given against them.

In fact, the Apothecaries' Act, while it condemns the honourable candidate to an extensive, and (if properly pursued) a severe course of study, preparatory to his being even admitted to an examination by their court—while it requires such an outlay of money as to exclude from the list of Licentiates all men (however otherwise qualified) who cannot command a considerable capital—this Act affords to the Licentiates who have passed its ordeal, no kind of protection whatever against the rapacity and ignorant pretensions of the lowest dregs of the community.

3. The Committee think, that if a clause could be obtained in addition to the present Act, to make it lawful for any magistrate to summon before him suspected individuals; and, in case of their being unable to produce a diploma from some legally constituted board of examiners, to inflict upon them such a penalty (either fine or imprisonment) as shall be deemed expedient for every offence; such an addition would tend very materially to the suppression of quackery, and would ensure a consequent improvement in the prospects of the profession—more especially the junior members of it, who are necessarily the great sufferers from illegal opposition.

4. If the propriety of these suggestions be admitted, it is obviously the duty, as well as the interest, of the medical profession, to endeavour to obtain such an improvement in the existing laws as will confer a benefit both on themselves and the public. When an advantage is to be gained by any one, he must *himself* seriously strive after it, trusting to the exertions of no second person, and least of all to those of the individual from whom the benefit is to be obtained. This has been abundantly proved in the case of medical reform. The profession, instead of themselves struggling for the possession of those privileges and immunities to which they are reasonably entitled, have trusted

to the efforts of individual members of the legislature, who, having no personal interest in the matter, have allowed it quietly to sink into oblivion.

The committee, therefore, most earnestly call upon the members of this Society to be united in their endeavours, and to allow no party feeling, no foreign consideration, to interfere with their determination of having their grievances redressed. For the attainment of such redress, the Committee beg to recommend that petitions to the two Houses of Parliament be drawn up, and signed by all licensed practitioners in this Borough, setting forth in plain terms the injuries which medical men suffer, from the very insufficient protection afforded them by the Apothecaries' Act; and, secondly, suggesting to the notice of the legislature the summary process of punishing quackery by empowering magistrates to inflict a penalty on the party accused failing to produce a lawful certificate of qualification.

5. When the petitions have been duly signed in Leeds and the out-townships, the Committee recommend that other large towns, such as Manchester, Sheffield, Birmingham, Halifax, &c., be invited to co-operate with us, by preparing similar petitions, and in their turn inviting neighbouring towns to do the like; so that petitions may be presented, as if with one accord, from every part of the kingdom.

6. That it will be desirable to communicate with Messrs. Warburton and Wakley, to ascertain whether they have any intention of introducing this subject during the ensuing session of parliament; if so, what are their specific objects?—if not, whether they will assist the cause when we ourselves bring it forward?

7. That accounts of our determinations and proceedings be sent to the leading medical periodicals, begging their assistance in making the business generally known, and in encouraging the profession to come forward.

Lastly, the Committee would again urge upon you the necessity of being united. The undertaking in which we are about to engage, not only tends, if successful, to the immediate benefit of every present member of the medical profession, but also promises to remove many difficulties from the path of succeeding generations of practitioners. It is, therefore, surely unnecessary to use any arguments to show that it is the duty and interest of the profession to exert themselves in a cause of itself so good, and so entirely their own. The Committee have confined their considerations to the single subject of quackery, but do not therefore wish to

limit the field of improvement. They leave it to the profession generally to determine how far it may be prudent to confine their endeavours to the one object of the suppression of that abuse, or to extend them to the various details of medical reform which have been of late years before the public. They also leave it to this Society to determine on the propriety of calling a public meeting of the Leeds practitioners, for the discussion of this important subject, and the adoption of such measures as may seem calculated to obtain the desired end.

(Signed) SAMUEL HEALD,
GEO. BEARPARK,
GEORGE WILSON.
W. STEAD TAYLOR, Sec.

Sept. 22, 1837.

FEES FOR INSURANCE CERTIFICATES.

To the Editor of the Medical Gazette.

SIR,

MAY I request of you the favour of inserting, in an early number of your journal, the accompanying Resolution (with the list of signatures) adopted unanimously by the professional gentlemen of Devonport and Stonehouse.

I have the honour to be, sir,
Your obedient servant,
JOSEPH MAY, Hon. Sec.

Devonport, 11 Jan. 1838.

We, the undersigned Medical Practitioners in Devonport and Stonehouse, resolve that in future, we will answer communications made to us by Insurance Companies, relative to the health of individuals who may have been under our medical treatment, only on condition that such communications be accompanied by a suitable fee, considering the Insurance Companies are parties benefited by the information thus obtained.

J. Bone.	J. Lower.
J. Boyle.	J. May.
J. Burrows.	G. Rolston.
J. C. Buxton.	F. Row, M.D.
J. E. Clark.	T. Rutter.
W. Cole.	J. E. Rutter.
T. Crossing.	W. Simpson.
G. Daussey.	E. B. Squire.
G. Glasson, M.D.	P. W. Swain.
T. C. Hancock.	C. Thomas, M.D.
G. F. P. Harrison.	R. Topping.
J. Isbell, M.D.	H. Tracey.
W. J. Isbell.	C. Tripe.
S. Kerswill.	I. P. Tripe.
R. J. Laity.	R. Watson.
D. Little.	W. Welch.

TWO CASES OF STRANGULATED HERNIA.

By M. THOMASSIN, OF ST. ETIENNE.

CASE I.—Madame Tricot de Femè, aged 47, of a bilious temperament, deaf, and worn-out with cares, had had a tumor the size of an egg, in the crural region, for twelve years. The tumor was not moveable, and could not be diminished by pressure, nor did it communicate a shock to the hand when she coughed. There was nothing to show that it was a hernia, except the seat of the swelling, and the periodical disturbance which occurred in the alimentary canal. Its size was neither lessened nor increased by walking, or the horizontal posture. The irreducibility of the tumor, from the patient's not wearing a truss, and, in fact, every circumstance, made one think that no harm would happen, when, one day, without any known cause, the patient was seized with violent pain in the tumor; there was exquisite sensibility in the abdomen, with meteorism; and vomiting of all fluids that were swallowed, but not of fecal matter.

On being called to see the patient, I suspected that it was a case of strangulated hernia with adhesions. I applied leeches and ordered the warm bath, but the symptoms continued in spite of my treatment, and my repeated efforts at reduction were in vain. The long weakness of the patient made me dread the operation, yet, as it was certain she would sink without it, I resolved to perform it.

There was nothing unusual in the operation until arriving at the hernial sac; but after making the incision in the skin, and dissecting away the cellular tissue, I was much surprised when, instead of a transparent hernial sac, I saw a sac with radiated fibres, and of semi-cartilaginous consistence. I thought, at first, that I had come to a tumor arising from the abdomen, and causing all these symptoms by compression.

I made an incision, to be certain of the nature of the tumor, and found that the parietes of the sac were half an inch thick, and that my diagnosis had been correct. It contained a portion of omentum and a knuckle of intestine. After having relieved the strangulation, and replaced the hernial parts, which did not adhere, I removed a great portion of the sac; the rest came away, in fragments, from the wound, which became gangrenous. The patient survived.

CASE II.—A man, aged 55, of a lymphatic temperament, was affected with an inguinal hernia on the left side, which he had kept reduced for many years by means of a truss. He grew tired of wearing it,

however, as it incommoded him in business, and he left it off; but, one day, on exerting himself, a large mass of intestine made its way into the scrotum. I was called to the patient, and found him labouring under all the symptoms of strangulated hernia. In spite of the most rational treatment, the protruded parts could not be replaced, and I therefore proposed the operation, but it was not consented to. The patient suffered for a week under the following symptoms: vomiting of fecal matter, inability of swallowing fluids, extreme inflation of the abdomen, pulse wretchedly weak, face attenuated and livid; general infiltration [anasarca]. The patient, seized with alarm, now asked me to operate; but as I was persuaded that the attempt would be hopeless, I refused. All the symptoms continued for two months, and yet the patient recovered. Ever since this occurrence he has been attacked every three months by violent colic, inflation of the abdomen, obstinate vomiting, and general infiltration. This state lasts for eight or ten days, and then ceases.—*Gazette des Hôpitaux*.

POSITION DURING DELIVERY.

To the Editor of the Medical Gazette.

SIR,

I DID not expect my letter to you would have been honoured with the rather ambitious title you affixed to it, still less that it would have called forth precisely the kind of answer it has done from Mr. M'IVOR. As the left position has the sanction of common usage, it is the one I shall always adopt in ordinary cases; but I am still as much in the dark as ever as to the reason why it obtained that sanction. Mr. M'IVOR arranges his reasons under four heads, of which I deny the force of the first, third, fourth, and the greater part of the second. As to his remarks on the *relative position* of the patient and the medical attendant, I have only to observe that (according to my slender logic), while the *absolute position* of both are changed, the *relative position* may remain the same.

With respect to passing the female catheter, as I have practised both methods, I give the preference to the one which I can always accomplish without exposing the patient, having never done so but in one instance, where the diseased state of the parts might have puzzled even the erudite tact of Mr. M'IVOR. I cannot make the slightest pretension to pass the female catheter with "grace and courtesy," and I feel, therefore, the rashness of prolonging the con-

trovery with so highly gifted an opponent. I must decline the unequal contest, and only express my belief, that if Mr. McIvor would spare a little more of the last article, viz. courtesy, to an inquiring country practitioner, he would not find it a ruinous misapplication of that valuable quality.—I am, sir,

Your obedient servant,
HENRY COLES.

Cheltenham, Jan. 15, 1838.

To the Editor of the Medical Gazette.

SIR,
PERMIT me to direct the attention of your correspondents, Messrs. Coles and McIvor, to Nagle's Essay on the Mechanism of Parturition, translated by Dr. Rigby, pp. 12—30. They will find there full and ample reasons why women are delivered on the left side in preference to the right, and decidedly more satisfactory than those advanced by Mr. McIvor in the last number of your journal.—I am, sir,

Your obedient servant,
FORCEPS.

Guy's Hospital, Jan. 16, 1838.

ACCIDENTS ADMITTED AT THE LONDON HOSPITAL.

Surgeon of the Week, Mr. LUKE.

Assistant-Surgeon, Mr. CURLING.

Dec.	Sex.	Age.	Case.
26.	M.	68	Contus. d hip.
	M.	40	Contused foot.
	M.	63	Lacerated scalp.
	M.	36	Lacerated scalp.
	F.	50	Sprained wrist.
	M.	8	Severe burn.
27.	F.	42	Contused knee.
	F.	46	Rupture of the eye from a blow.
	M.	21	Severe contusion of the elbow joint.
	M.	45	—
	M.	51	Fractured ribs.
28.	M.	42	Contused side.
	M.	47	Contused foot.
	M.	31	Contused back.
	M.	21	Hæmorrhage after the extraction of a tooth.
	M.	20	Free hæmorrhage from a secondary syphilitic ulcer on the chin.
	F.	18	Contused knee.
29.	F.	55	Contused side and hip.
30.	F.	65	Dislocation of the femur on the pubes.
	F.	62	Contused thigh.
	M.	21	Contused knee.
	M.	23	Contused side.
	M.	32	Retention of urine from stricture.
31.	M.	9	Severe burn.
	M.	21	Contusion of the testicle.

M. 64 Fractured tibia.

F. 63 Fractured femur.

Jan.

1. F 27 Sprained ankle.

M. 55 Contused knee.

M. 37 Cut head, followed by erysipelas.

In patients 30

Out patients 51

Total during the week . . 81

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, Jan. 11, 1838.

Frederick Johnson Moore, Mosegrove, Exeter. — William Fisher, Chelford, Cheshire. — Charles Homes, Oxford.

Thursday, Jan. 18, 1838.

George Chaplin Child, Edinburgh. — Edward Newbold, Manchester. — Thomas Paltorpe, Lincolnshire — Charles Taylor, Boston, Lincolnshire. — John Seilvan.

WEEKLY ACCOUNT OF BURIALS

From BILLS OF MORTALITY, Jan. 16, 1838.

Age and Debility	24	Heart, diseased	2
Apoplexy	2	Hooping Cough	5
Asthma	3	Inflammation	10
Cancer	1	Bowels & Stomach	3
Childbirth	2	Brain	4
Consumption	25	Lungs and Pleura	9
Convulsions	14	Liver, diseased	1
Dentition or Teething	4	Measles	2
Diabetes	1	Mortification	3
Diarrhoea	1	Paralysis	3
Dropsy	6	Small-pox	6
Dropsy in the Brain	7	Sore Throat and	
Epilepsy	1	Quinsey	1
Erysipelas	1	Unknown Causes	44
Fever	6		
Fever, Scarlet	3	Casualties	8
Fever, Typhus	7		

Increase of Burials, as compared with the preceding week 13

METEOROLOGICAL JOURNAL.

Jan.	Thermometer.	Barometer.
Thursday . 11	from 55 to 27	29.87 to 30.04
Friday . . 12	4.5 23	30.17 30.25
Saturday . 13	13 26	30.21 30.11
Sunday . . 14	18 27	29.5 29.83
Monday . . 15	1 23	29.73 29.74
Tuesday . . 16	9 31	29.74 29.95
Wednesday 17	16.5 33	30.07 30.11

Winds S. by E. & S.W.; the former prevailing. Except the afternoons of the 12th, 14th, and following day, generally cloudy. Snow fell on the 11th, 14th, 16th, and 17th. The very low state of the thermometer has not been equalled during the past eighteen years. Upon reference to our journal for Jan. 1820, we find two remarkable incidences, viz. that during the previous days of the week the wind had been, as now, from the N.E., changing on the morning of the 15th to the S.W.; and the thermometer falling to 1 deg. It is singular enough, that, at the same date of the present year marks the same low degree of temperature, and precisely the same change of the wind. The severity of the frost has, however, been of longer duration than in 1820, having continued, without intermission, from the evening of Sunday the 7th to the present time, Thursday morning, the 15th, and yet no indication of a change.

WILSON & SON, Printers, 57, Skinner-st., London.

THE
LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JANUARY 27, 1838.

LECTURES
ON THE
PHYSIOLOGY AND DISEASES OF
THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL
AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of
1836 and 1837,

At the Anatomical School, Kinnerton-Street,
near St George's Hospital,

By CHARLES J. B. WILLIAMS, M.D. F.R.S.

LECTURE XII.

Diseases of the Air Tubes—General Pathology of Inflammation of the Mucous Membrane—Bronchitis—Mild Bronchitis: Signs and Course—Treatment of "a Cold" by Pectoratives—by Opium—by the "Dry Method"—Severe Bronchitis: its Signs and Course; Treatment—Asthmatic Bronchitis—Bronchitis of Infants.

HAVING studied those physical and vital properties of the organs of respiration, which, by their healthy and their disordered phenomena, constitute the general physiology and the general pathology of these organs, we next come to consider the groups or associations of these phenomena as they are presented to us in particular diseases, or special pathological conditions. Now you may judge that we have not time to enter into full descriptions of the various diseases of the chest, nor is it the object of these lectures to do so: these matters are to be found elsewhere, especially in the standard systematic works on the Practice of Medicine, which have appeared within the last few years. Our

notice of diseases will be mere outlines with regard to their general symptoms; and it is only where they can derive light from the newer views of pathology and diagnosis, and from the application of these to practice, that we shall be induced to enter into any details; and here I hope that you will find the principles which we have been already considering continually useful as a key and a guide, both in study and in practice.

Following the same course which we adopted in describing the structure of the organs of respiration, we have first to consider the diseases of the air-tubes; and as the most important class of these we shall commence with inflammations.

Bronchitis is essentially an inflammation of the mucous membrane of the bronchi, and it derives its chief characters from the structure and functions of that membrane. Now you know that mucous membranes are less simple in their structure than serous, and the effect of inflammation on them is consequently different; but, as we shall see by and by, the difference does not exclude many parallels. There are, an enlargement of the blood-vessels, an interstitial effusion, and consequent swelling; there is an exalted sensibility; at first there is a diminished, but, as the irritation proceeds, an augmented secretion from the membrane. This secretion is at first transparent and liquid, but more or less glutinous or ropy; and this uniformly viscid quality generally increases with the intensity of the inflammation. No sooner, however, does the inflammation begin to abate, than there is a manifest change in the secretion; it becomes partially opaque and clotted, and although still viscid, in parts even more so than before, it is not uniform, and the masses do not run together and coalesce into a glairy mass. This marks the transition of the secretion from the mucous,

or muco-serous, to the coagulated albuminous form, which may be simply opaque, muco purulent, purulent, or less frequently of a more solid albumen; and with these changes there are indications of a diminution of the intensity of the inflammation, and of the local swelling and irritation which accompany it. When brought to this state of secretion, the vessels either, having relieved themselves, recover their tone, and gradually return to their ordinary size and function, or if the irritation have continued too long, or have been of a very intense character, they are more permanently altered, and continue to secrete more or less of a muco-purulent or purulent matter, the disease thus becoming chronic.

Now if you bear in mind this general course of inflammation of the bronchial membrane, you will have a key to many of the signs and symptoms of bronchitis; and the character of the disease will in great measure depend on the stage and the extent of this inflammation, and may thus vary from an affection so trifling as not to interfere with the ordinary occupations of the individual, to one of the most serious and dangerous of diseases. Let us quickly go through the history of the more common varieties of bronchitis.

You know that a common cough, catarrh, or "cold in the chest," is a mild form of bronchitis. This commonly begins with a sore throat or a "cold in the head," the inflammation of which seems to travel down to the windpipe and bronchi, there causing pain or tightness under the sternum, and cough; at first with a feeling of roughness or dryness in the throat, but afterwards with a tickling, and the expectoration of a thin, glary salt tasted mucus, which is often profuse in persons of a relaxed habit: this secretion, so far from relieving the cough, obviously aggravates it; and I have no doubt that it possesses the same irritating quality that the humour of coryza has, the contact of which with the membrane that secretes it, causes such an unpleasant tickling, sneezing, and lachrymation, and which excoerates the external parts over which it runs. This has also a salt taste, and probably these and all other thin mucous secretions owe their irritating quality chiefly to the saline matter in them. Well, it is when this is secreted that the cough becomes most troublesome, and from its violence there is not unfrequently abrasion of parts of the membrane lining the trachea or larynx, and streaks of blood are seen in the sputa; these are, however, quite distinct, and do not tinge the mass. Patients are apt to be alarmed at this appearance of blood, but if preceded by a violent cough, this

symptom need cause no apprehension. Even mild bronchitis, in its acute stage, is generally accompanied with slight fever, and some shortness of breath; but I need not dwell more on the general symptoms.

It is by the physical signs that we more clearly mark the condition of the bronchial membrane. In the earliest stage, perhaps before the cough or other symptom of pectoral disease, various dry rhonchi, the sonorous and sibilant, with a diminution of the respiratory murmur, announce the narrowing of some of the air-tubes: more rarely, a total absence of sound in a part of the chest implies that the obstruction there is complete; but the unimpaired sound on percussion shows that the vesicular structure is free. These obstructions, no doubt, arise chiefly from the swelling of the mucous and sub-mucous tissues; we see the like take place in the nasal canals, when they are the seat of the kindred affection, coryza.* The bronchial tubes do not remain long in this dry state; the secretion commencing first, gives a roughness to the other sounds, and then adds to them a sound of bubbling, which is the mucous rhonchus; but this is seldom so loud as the other sounds, and when the disease occupies only the deep-seated tubes, it may scarcely be heard at all. According as the liquid is in the large or the small tubes, the bubbles, and the crackling which they produce, will be coarse and unequal, or finer and more uniform. The usual seat of all these sounds in the milder forms of bronchitis is in the middle parts of the chest, whether in front, behind, or at the sides, where the larger bronchi lie: in the severer forms they extend to other parts. You may suppose from what I said in a former lecture, in explanation of the sonorous and sibilant rhonchi, that the lower tones imply an affection of the larger tubes; but the acute notes do not exclusively indicate an affection of the finer tubes, for they may be produced also in the large tubes where the obstruction is considerable; and when you hear an acute or whistling note prolonged through the whole act of inspiration or expiration, you may be sure that it is not produced in the finer tubes, for the air is not so long passing through them. I believe that the deep sonorous rhonchus is generally seated at the branching off of a large bronchus, and so strong are its vibrations that it is often felt by the hand applied to the exterior, or by the patient, who can point out the spot where

* Dr. Stokes has suggested an additional cause in a spasmodic contraction of the circular fibres, now unduly irritable. It is very probable that this may co-operate.

it is. These various sounds may accompany either the inspiration, expiration, or both.

The decline of bronchitis is announced by a looser character of the cough, and the change before-named in the expectoration to the opaque concocted kind. This is generally first seen in the morning, that being usually the time of day in which most febrile and inflammatory diseases show a tendency to remission. Sometimes, without this opacity, the sputa assume a more consistent form. This renders the cough and expectoration easier, but it is not accompanied with the general improvement so remarkable when the sputa become simply opaque. In either case, the inspissation of the bronchial secretion causes some change in the physical signs. The bubbles are heard to break more rarely, and give more of a whistling or ticking sound, and the sibilant and sonorous rhonchi become remarkable; but they shift and vary with every cough or forcible act of breathing. The same clots of mucus that by their partial obstruction to the air cause this rhonchi, sometimes totally block up one or more of the tubes, and stop the sound of respiration in the part to which the tubes lead. But this stoppage is seldom permanent, and a cough or deep inspiration will often open it, or shift it to another part, and the air is then heard to enter with a whistling or clicking noise, where all had been silent before. The sound, on percussion, is still uniformly good; and this circumstance, with the varying respiration and rhonchi, characterizes bronchitis in this stage.

Before I notice the more formidable varieties of bronchitis, I must say a word or two on the treatment of its common mild kind. "A cold" is generally accounted a trivial complaint; and although the sufferer is often more annoyed and incommoded by it than by affections of a graver name, still it is "only a cold," and he gets little pity, and no treatment beyond that of a few domestic remedies, which may do neither good nor harm. Yet those of you who are liable to colds and coughs, will agree with me when I say that this class of complaints, by their frequent occurrence, by their own evils, and by the many aches and disorders which they often bring with them (tooth-ache, ear-ache, deafness, sore throat, weak eyes, rheumatic pains, indigestion, costiveness, &c. &c.), interfere more with the comfort and occupations of very many individuals than all their other illnesses put together. Surely, then, the subject deserves more attention than it usually gains; and if we cannot make people in general think it worth while to take our

advice for a common cold, we may just as well profit by it ourselves. It is a common notion that a cold must run its course; and Laennec cites a proverb much to the same effect:—"A cold well nursed, lasts forty days; a cold not nursed at all, lasts six weeks." I hope, for the credit of our art, that this is not generally true; I am quite sure that I have not found it to be so, or I would not trouble you with any further remarks on the subject. The ordinary mode of treating a cold attended with bronchial inflammation is certainly rather palliative than positively curative; but it generally mitigates its severity, and hastens its termination. A brisk purgative, conjoined (if febrile or gastric disturbance be present) with a moderate dose of calomel and James's powder, will generally take off the edge of the disease; whilst a sedative and expectorant mixture will be useful to quiet the cough, and to lead it to its natural termination by expectoration. In the mild acute inflammations of the air-tubes, the promotion of expectoration is a primary object of the treatment; and besides those means which reduce the intensity of the inflammation (such as sudorifics, evacuants, counter-irritants, and local depletion, some or all of which may be beneficial in particular cases), great advantage will be generally derived from the frequent use of a cough mixture. Various combinations may answer for this purpose. Formulæ of ipecacuanha, or tartarized antimony, with hyoscyamus, conium, or hydrocyanic acid, for the onset; and squill, ipecacuanha, opium, and camphor, for the subsequent periods, are what have appeared to me to answer best. But the efficacy of these remedies is decidedly increased by combining them with an alkali. From ten to twenty drops of the liquor potassæ, or an equal number of grains of carbonate of soda, or, in more asthenic cases, twenty or thirty drops of the sp. ammoniæ arom. are sufficient; and I am confident that in the greater number of cases the alkaline remedies quiet the cough, and promote expectoration far better than the oxymels and acid linets, or lozenges, that are commonly in use, and which, however they may appear at the time to "cut the phlegm," and cleanse the throat, tend to disorder the digestive organs, and often ultimately to increase the cough. To have their full effect, cough medicines should be taken frequently—at least five or six times a day; for besides that their object is to influence continually the secretion of the bronchi through the circulation, they seem to act in some measure directly on the glottis and its neighbourhood; and in the intervals it is useful to have in the mouth

a bit of gum arabic, or of liquorice, the solution of which tends, also by continuity, to sheathe these same irritated parts.

I have nothing further to add to what you must already know respecting this mode of treatment; the great objection to which is, that for its success it requires more or less nursing and confinement. To give diaphoretics and diluents, and at the same time to expose the body to transmutations of temperature, which are almost unavoidable without confinement, will tend rather to increase a cold than to diminish it; and I am quite sure that I have seen many colds and coughs kept up by this system. Now, many persons cannot afford the time to confine themselves to nurse a cold; and they either let it run its natural course, or make an even worse compromise, by nursing and sweating one part of the day, and exposing and chilling themselves at another. We want, then, a method of treating a cold, more applicable to those who cannot or will not confine themselves, and of this class especially are medical men. Now I know of two methods of this kind; but to be used with any success they must be employed at the onset of the disease, within a day or two from its commencement, and the earlier the better. The "cold," of which I speak, is an inflammation of the membranes lining the air-passages, accompanied by a secretion which, we have observed, is irritating, and tends to keep up the inflammation. Now, although we cannot cut short this inflammation, and stop this secretion by common emetics or antiphlogistics, yet we may, by other means, either remove or modify it so as to lead to a more speedy mode of termination. One method that sometimes has this effect is, by taking, at bed-time at the earliest stage of the cold, (whether the affection be felt in the nostrils, the throat, the chest, or in the system generally,) a full dose of opium in some form, and following it the next morning by a brisk cathartic. Ten or fifteen grains of Dover's powder, or two grains of opium with two of ipecacuanha, or half an ounce of compound tincture of camphor, are the most eligible forms of opiate; and I think it is safe to add a few grains of calomel, or some milder mercurial, to prevent the restraining effect of the opium on the secretions. When this remedy acts well, the patient sleeps soundly, generally perspires freely, and awakes the next morning free from his cold, but often with some headache and nausea, the after effect of the opiate. These are generally relieved by a well-seasoned black draught, and no further ailment is felt but some languor, which another night's rest may remove. I do not know who first suggested this

heroic cure for a cold; but I believe it is pretty generally practised by members of our profession, and it is by no means an unpleasant one, where opiates and strong purgatives do not disagree. But the stomachs of many do not bear this piece of *medicina perturbatrix*; it may disorder the digestive and alvine functions for weeks afterwards. Sometimes, too, the catarrhal irritation may be too strong to be lulled by the opiate, especially if it be in the chest, and in that case this treatment will hardly fail to aggravate it, and, by suppressing expectoration, it might even convert a bronchitis into a pneumonia.

The other method of stopping a cold is by what I call *drying it up*. I first practised this method in my own person, and you shall hear with what success. In early life I was subject to frequent and most violent colds in the head, which, after lasting a week or ten days, generally ended in a cough, which, even when nursed, I could scarcely ever get rid of in less than a fortnight more; and when neglected, has many times harassed me for double that time. Yet I underwent discipline severe enough; did not taste wine, and scarcely meat, for weeks together, and my chest even now bears the marks of the blistering and tartar-emetic counter-irritation which I inflicted on myself. About twelve years ago, on being attacked with one of these colds, I remarked that taking a quantity of tea, or any other liquid, although very comfortable at the time, was invariably followed in the course of an hour by an increased "stuffing in the head," and accompanying flow of scalding irritating humour from the nose and eyes. I thought I would try to prevent these exacerbations at least, by *cutting off the supplies*—by ceasing to drink. For twenty-four hours I did not take a drop of liquid of any kind, and to my agreeable surprise not only did I escape these occasional aggravations of the complaint, but the stuffing and discharge began to show evident signs of abatement, and the handkerchief was in less continual requisition. I persevered for twenty-four hours more, and my cold was gone; there being only now and then a little gelatinous opaque mucus collected in the nostrils and throat, without any stuffing or irritation, just as it takes place at the very end of a cold. What was of still more consequence, no cough followed; the whole catarrhal disease seemed to have been destroyed. This plan of treatment I have been in the habit of using ever since, with a success varying somewhat in degree, but on the whole amounting to this, that I have had no colds which have lasted three days, and never a severe cough, except once, and

that was when circumstances prevented a due perseverance in this dry diet. I have recommended this method of cure to a great many friends and patients; and those who have had the resolution to use it fairly, have generally succeeded in bringing their troublesome colds to a premature end. You may be ready to say, that the remedy is worse than the disease. I would answer, that either you do not really know what a bad cold is, or your appetites hold a little too much of an epicurean sway over you. I do not recommend the plan to those who have only slight or transitory colds; these may not be worth the penalty of any self-denial; but the amount of discomfort entailed by two days' abstinence from liquids is really trifling in comparison with that and the ill-health that a severe cold brings with it; and I have never hitherto seen any bad effects to arise from the practice. As no account of this method of curing colds has been before the public, except a short notice which I gave in the article *Coryza*, in the *Cyclopædia of Practical Medicine*, I will detain you with a few more observations on its mode of action and application.

The great effect of abstinence from liquids is promptly to decrease the mass of the circulating fluids. The natural fluid secretions continue, although in diminished quantity; the urine is still excreted, although its watery part is diminished; and the skin continues to perspire either insensibly, or, under the influence of increased warmth or exercise, in an obvious manner. Not so the morbid secretion from an irritated membrane: the irritation is lessened with the decreasing fullness of the blood-vessels; the morbid flux no longer continues, the scantier circulating fluid being now taxed for the necessary excretions too closely to supply it; and the diseased membrane, no longer irritated by its own secretion, soon returns to a healthy condition. If liquid be freely taken too soon, before the membrane have lost its diseased action, the discharge will return, and the complaint will be as bad as ever. But if, after the discharge has been stopped by twenty-four or thirty-six hours of dry diet, means be taken to keep up the other secretions, as by exercise, with a warm state of the surface, a little liquid may be taken with impunity, the bulk of the circulating fluid being still below the point at which it can supply any demands from the irritation in the diseased membrane.

I think that the physiological principle of the dry method is what I have now stated; and you can easily perceive that

there must be limits to its application. We do not yet know how far it may be pushed, or what may be the extent of its influence on various morbid conditions of the vessels and of secreting organs. I know from experience that partial abstinence from liquids is of great use in moderating many kinds of congestion, hæmorrhage, and local determinations of blood, and especially in diseased conditions of the mechanism of the circulation; but we have yet to find out how far a total abstinence may be carried to control acute affections of a similar character. It is necessary also to bear in mind, that other diseases may arise from a defect of liquid in the system; that the excreting organs may suffer from the undiluted excrementitious matters that would pass through them; that their function being disordered, some of this excrementitious matter may be left in the system, from which very pernicious effects would ensue; that some functions, as that of digestion, would be arrested by the want of a due proportion of water in the secretions that carry it on; and other evils which it is not necessary to enumerate. I say, there is reason to expect that such consequences might ensue from carrying this system too far; but I have never seen any such effects from that amount of the plan which is sufficient to cure the severest cold at its onset. I have more than once passed three whole days without tasting a drop of liquid. The only unpleasant effects were, at the end of the second day, slight headache, with some languor, decided diminution of bodily strength, and a small feeble pulse. These were relieved by a night's rest, returned in the course of the next day, but were entirely removed by two cups of tea at the end of the third day; and all that remained of the effects of the discipline was a desire during the next two days to repair the wants of the system by copious libations, the enjoyment of which was far greater than had been the suffering from abstinence. This suffering is, in fact, rather negative than positive: one certainly does not enjoy one's meals fully without drinking with them; but this is the diminution of a pleasure rather than an infliction of pain; and when the meal is over, one forgets all about it. But without dwelling on this point, which being one somewhat of taste, is not to be argued on, let me say a few words more on the application of this plan to the cure of a cold.

It is very essential for the success of the dry treatment that the cold should be in its early irritative stage, when it generally occupies the nasal and pituitary mem-

brane. If there be any fever, and especially if the state of the bowels require it, an aperient with an antimonial should be given, for this favours that free state of the secretions on which, as we have seen, the efficacy of the dry plan depends. In milder cases, however, this is not necessary. For the same reason it is expedient that the solid food be not of a too rich or heating kind; for this, undiluted by liquid, might be apt to disagree. Bread, or any consistent farinaceous food, with a little butter, vegetables, white fish, and white or gelatinous meats, light puddings, and dried fruits, will do very well for a dry diet; and who can call this starving? In fact, I have sometimes not changed the diet in any particular but in the point of abstinence from liquids; and with regard to this point, although total abstinence is, of course, the most effectual, I have found lately that a deviation to the amount of taking about a table-spoonful of tea, or milk, with breakfast and the evening meal, and a wine glassful of water on going to bed, does not interfere with the success of the plan, and it certainly adds to one's comfort. But a great advantage of this plan is, that it does not interfere with one's ordinary pursuits; it needs no nursing or confinement. In fact, if care be taken to clothe enough to prevent the surface from being chilled, exercise in the open air promotes the success of the plan, by promoting the natural secretions. On the other hand, those who treat their colds by slops and diluents, which act chiefly by increasing the perspiration, will suffer from the least transition of temperature, which will have an increased influence on a perspiring surface. The time necessary to effect a cure by the dry plan will vary in different individuals, according to the present quantity of their circulating fluid, the activity of their secretions, and the intensity of the catarrhal disease; and also somewhat according to the temperature and hygrometric state of the air, longer time being always required when the air is cold and damp. On the average, forty-eight hours of abstinence will be quite sufficient. I have often known thirty-six hours sufficient; but some few severe and obstinate cases required three days. The period may be shortened by exercise and warm clothing, or lying warm in bed, or by commencing with a purgative, or by any other dry means of increasing the natural secretions. The catarrhal affection is generally much relieved at the end of the first day, and only is troublesome at times; but the cure is not complete till all stuffing is gone, and nothing but a consistent mucus, without

irritation, is formed in the nasal or bronchial passages. Sometimes this secretion will continue for a few days; but unless fresh cold be taken, it causes no inconvenience, and soon ceases. In these cases it is generally prudent to take an aperient and diaphoretic on returning to the use of liquids, which it is always best to begin at night, when there is less risk of relapse from fresh exposure. Perhaps you think that I have been taking up too much of your time with this more trivial form of catarrhal disease, and this novel mode of treating it; but it is because the disease is thought too trivial, and because the treatment is novel, that I have been dwelling longer on the subject than its comparative importance would seem to demand.

The intense or severe form of bronchitis differs from the milder kind only in the greater extent of the bronchial tubes which it occupies. Its pathological nature and local signs are the same, but its general symptoms differ, inasmuch as the system suffers more from the greater extent of the functional mischief. This disease presents itself in two forms, the *sthenic* and the *asthenic*. In *sthenic* bronchitis, inflammatory symptoms are marked from the commencement: these are generally pain, and a feeling of tightness across the sternum; hard severe cough, with very glutinous expectoration; much fever; heat of skin; the tongue white and red at the edges; pulse quick and often hard; the breathing much shortened, often oppressed, and the least exertion occasions coughing. The physical signs are like those of the mild form, but are here heard more extensively throughout the chest. The rhonchi are at first sibilant and sonorous, afterwards mucous and submucous, reaching to the inferior portions of the lungs, with a weakened respiratory murmur, announcing the presence of the inflammatory mucus even in the smaller tubes; but the clear sound on percussion declares the vesicular structure still free. If relief be not afforded by expectoration, perspiration, or prompt remedial measures, the disease soon shews a change of character, from the increased dyspnoea and symptoms of partial asphyxia that ensue. Then come on feelings of great depression; the pulse becomes weak as well as quick; the sensorium is sometimes disturbed; the muscular strength is much reduced; the countenance becomes anxious and pallid, or partially livid, according to the quantity of blood in the system; the pulmonary congestion becomes evident by a slightly diminished resonance on percussion in the postero-inferior regions of the chest. The continuance of this state, and

the imperfect arterialization of the blood, further disturb other functions; the secretions become more scanty and vitiated; the tongue is loaded with a brown fur; the thirst is intense; and all these disorders concur in reacting on and aggravating the original disease, and in injuring the natural powers. Such is the loss of balance which ensues from the inroad which severe bronchitis makes on the important function of respiration. The share which the injury of this function has in giving character to the constitutional symptoms, is seen in the fact, that very similar effects are met with in persons who have been subjected to an asphyxiating influence. The step from this condition to death is but a short one, and happens too often, especially in cases that have been neglected at the onset.

Now the advantage of the physical signs is to inform us with certainty of the first coming on of an inflammatory affection of this character. When with the febrile state before described, whether the functional disorder be prominent or not, we find extensive rhonchi in every part of the chest, especially if they extend to the inferior parts, and there be little respiratory murmur audible, we should not hesitate to resort at once to such depletory measures—bleeding, cupping, or leeches—as the individual case will admit, and conjoin with these, mercurial and antimonial medicines to act on the secretions. Generally even moderate bleedings give speedy relief, by removing the congested state of the lung; and in this respect bronchitis differs from pneumonia, in which this congestion is a more fixed part of the disease. It is desirable, however, to produce an impression on the pulse, which will sometimes increase in fulness as the blood flows and relieves the temporary congestion. But large bleedings are not expedient, for the inflammation of a mucous membrane is not removed by them; it involves a certain structural change, probably interstitial effusion, that can be relieved only by a free secretion from it. Expectoration is a necessary process during the remainder of the disease, and there must be strength saved for this purpose. In many cases cupping or leeches are to be preferred to general blood-letting; and they may generally be added with advantage. When the edge of the inflammation has been thus taken off, a very large blister, or the free application by friction of a saturated solution of tartarized antimony on the chest, will keep up the relief given. The latter remedy is well adapted to the more sthenic forms of bronchitis; and to ensure its prompt action, the chest should be first rubbed with a flesh-brush,

or a piece of coarse flannel: it will then generally bring out a small pustulating eruption in the course of a few hours. Of the internal remedies, tartar emetic, in large doses, for the strong, and mercury, with ipecacuanha and James's powder, for the less robust, are the most effectual. I shall have an opportunity of noticing the antimonial treatment when speaking of pneumonia; and I will not enter now into common details of the treatment, as you will find these given in the standard works on practical medicine—as in the article *Bronchitis*, in the *Cyclopædia*, or more fully in Dr. Copland's *Dictionary of Practical Medicine*.* The good effect of the treatment will be apparent in the general symptoms before it is evinced in the physical signs. The breathing becomes less laboured, the countenance improves, the pulse becomes steadier and fuller. On listening, we may find that the air enters more freely into the lungs, but the mucous and other rhonchi are still present, and continue for some time; and it is only when the improvement is considerable that we can perceive that they diminish, and that the obstructions become less general; that instead of bubbling over the whole, or a considerable portion, of the chest, the respiratory murmur is heard, still mixed with clicking, whistling, and humming sounds.

The chief difference presented by the *asthenic* or *lunatic* form of bronchitis, or *peip ematica nostra*, as it was formerly called, is the early appearance of signs of depression, generally attended with gastric derangement, nausea, headache, &c.; and by the physical signs we learn, from the universal mucous rhonchus, the early presence of a profuse secretion in all the tubes. I have often observed a temporary dulness on percussion produced by this effusion, or by the pulmonary congestion that ensues in consequence of it: nay, I have known it to cause bronchophony and bronchial respiration, which lasted only during the evening exacerbation, and were gone the next morning. The transient character of these signs is enough to show that the increased density of the lung that caused them results merely from an increased quantity of liquid only, this liquid, probably, being the blood congested in the great pulmonary plexus of vessels, as well as the mucus in the bronchial tubes. This secretion is the great cause of the depression and danger; and its diminution or free expectoration is a chief object of the treatment. Bloodletting is scarcely

* To these must be added the valuable and comprehensive chapter on *Bronchitis*, in Dr. Stokes' work, published since this lecture was delivered.

borne here; the most available remedies are large blisters of a size to cover the whole chest; mercurial purgatives, and tartaric emetic in nauseating or larger doses, with expectorants of a somewhat stimulating kind, such as the decoction of senega, and the liquor ammoniæ acetatis, which do not increase the quantity, but facilitate the act of expectoration. Where the depression is greater, and the act of expectoration begins to fail, more stimulating remedies, such as the carbonate of ammonia, must be given, and the preservation of life will often depend on the judgment with which these are, from time to time, administered. Referring you to the works before named for further details, I shall now only add, that a certain degree of abstinence from liquids is highly serviceable here, not only by diminishing the mass of blood that has to pass through the now choked lungs, but also, as in the milder catarrh, by reducing the quantity of the bronchial secretion. That it has this latter effect I am convinced by repeated trials; and, on the other hand, I have many times seen a marked aggravation of dyspnoea take place in this and in other diseases of the chest after copious draughts of linseed tea, and other mucilaginous liquids, that are popularly taken with a view to *loosen* the phlegm.

In young children, a very dangerous kind of bronchitis sometimes come on very insidiously. A certain degree of drowsiness or stupor is present, which keeps the little patient from complaining or coughing. A close attention will, however, detect a great frequency in the breathing; and on applying the ear to the chest, the universal rhonchi, sibilant and mucous, at once declare the latent evil. Emetics and mercurial purgatives are the most successful remedies in these cases. The former must not be too frequently used, as they cause considerable determination to the head, and exhaustion; but they are eminently successful in emptying the bronchial tubes of their secretion; and I suspect that they do this not only by the action of the external muscles of expiration, but also by exciting the bronchial muscles to contract, as we know that the glottis is most forcibly closed during the act of vomiting. In the milder cases small doses of ipecacuanha may suffice. The utility of the physical signs is thus most evident in these cases; and in no instance of febrile disease in infants or young children should the practitioner omit to examine the state of the chest.

ON THE

OVA OF MAN AND MAMMIFEROUS BRUTES,

AS THEY EXIST IN THE OVARIES BEFORE IMPREGNATION;

AND ON

THE DISCOVERY IN THEM OF A VESICLE

Analogous to that described by Professor Purkinje in the immature Egg of the Bird.

BY THOS. WHARTON JONES.

Read before the Royal Society, June 18, 1835*.

THE organs in female mammiferous animals known by the name of *ovaries*, were at one time considered analogous in their function to the testes of the male. This opinion, however, has been long given up, and physiologists now universally admit that the function of the ovaries of the mammalia is the same as that of the ovaries of birds and other ovipara.

It was in the seventeenth century that this analogy was fully determined. Douglas (Bibliographiæ Anatomicæ, Specimen, p. 42), however, affirms that the true nature of the ovaries was known to John Mattheus de Gradi, professor of anatomy at Pavia, in the fifteenth century; and in this assertion he is followed by Portal (*Histoire de l'Anatomie et de la Chirurgie, passim*; and *Cours d'Anatomie Médicale*, vol. v. p. 506) and Cruveilhier (*Cours d'Etudes Anatomiques*, vol. i. p. 66.) In proof of what he advances, Douglas gives the following as a quotation from the work of Mattheus de Gradi:—"Testiculos muliebres duo ova carnis glandulosis cooperta." In the work to which Douglas refers (*Practica D. Magistri Johannis Matthei ex Ferrarii de Gradi, &c. Venice, 1520*), I have not been able to find any such expression. On the contrary, in a paragraph (charta 342, col. 1), with the marginal title, "*Muliebres habent duos testiculos ut viri*," we read the following:—"In matrice etenim sunt duo ova; ut dictum est supra, quæ sunt dictæ carnes glandu-

* See London and Edinburgh Philosophical Magazine, vol. vii. p. 209.—ED. GAZ.

losæ; et sunt duo testiculi mulieris ad rotunditatem magis declinantes quam testiculi viri et minores sunt testiculis virorum; et in eis fit generatio spermatis sicut et in testiculis viri secundum sententiam medicorum." The words *duo ova*, in this quotation, mean merely *two bodies of an oval form*. It thus appears that Matthæus de Graaf entertained no other opinion on the subject than the one commonly received in his day, and which continued to prevail for two hundred years after.

Of the distinguished anatomists who, in the seventeenth century, supported the doctrine that the mammifera are reproduced from ova generated in the ovaries, Steno and De Graaf deserve to be particularly noticed. I do not mention our great Harvey; for although he asserted that all animals, even man himself, are propagated by ova, he did not suppose that, in the mammifera, these ova are developed in the ovaries, but that they are first formed in the uterus. To the ovaries he ascribed an inferior function.

Nicholas Steno was professor of anatomy at Copenhagen, and afterwards Bishop of Titopolis, and Vicar Apostolical in the countries of the North. In his "*Observationes Anatomicæ Spectantes Ova Viviparorum*," he affirms that from the ovaries are produced the rudiments of the ovum, and that the uterus affords whatever is required for the perfect development of the fœtus. These propositions Steno supports by proofs drawn from human as well as comparative anatomy.

Regnerus de Graaf, who was a physician at Delft, gave in his treatise "*De Mulierum Organis Generationi inservientibus*," a very good account of the structure of the ovaries, describing more correctly than had previously been done the vesicles they contain. For this reason they are now commonly called the *ova*, or *vesicles of De Graaf*.

It thus came to be generally admitted, that the vesicles of the ovaries were ova, or rather that these vesicles contained something analogous to ova. But it was a question what the nature of that substance was; whether it had a determinate figure, or was a mere amorphous mass. As nothing but a drop of albuminous fluid could ever be observed in the vesicles, the latter opinion prevailed.

De Graaf, in 1672, discovered ova of

a definite shape in the fallopian tubes of rabbits three days after impregnation; these he delineated. But as they were so minute, so disproportionate in size to the vesicles of the ovaries, and as subsequent investigators failed in detecting them, De Graaf for a long time lay under the imputation of having asserted what was not true. To this injustice the authority of Haller, who failed in detecting ova in the sheep, mainly contributed. All doubts on the subject were, however, removed by the experiments of Cruikshank, performed in 1778, and published in the *Philosophical Transactions* for 1797 (p. 197). These experiments established the truth of De Graaf's assertion.

Prevost and Dumas, in their researches into the generation of the mammifera, found small ova in the horns of the uterus of a bitch eight days after impregnation, similar to those observed by De Graaf and Cruikshank in the rabbit on the third day. But that they had seen similar bodies in the fluid of the Graafian vesicles, without being fully aware of their nature, is rendered probable by the following extract from their memoir, entitled, "*De la Génération dans les Mammifères, et des premiers indices de développement de l'Embryon*," published in the third volume of the "*Annales des Sciences Naturelles*," Paris 1825:—

"Les ovules qu'on rencontre dans les cornes sont remarquable par leur petitesse. Ils ont en effet un ou deux millimètres de diamètre au plus, tandis que les vésicules de cet organe en possèdent un de sept ou huit millimètres, au moins. Ce sont donc deux choses qu'il ne faut pas confondre, et très-probablement les vésicules ou les œufs de l'ovaire, contiennent dans leur intérieur, les petits ovules des cornes, qui s'y trouvent environnés d'un liquide destiné peut-être à faciliter leur arrivée dans l'utérus. Il est survenu deux fois, en ouvrant des vésicules très-avancées, de rencontrer dans leur intérieur un petit corps sphérique d'un millimètre de diamètre." Notwithstanding these important steps in the investigation, it was not till 1827 that ova were first unequivocally demonstrated in the ovaries of the mammifera.

To Carl Ernst von Baer*, professor

* *De Ovi Mammalium et Hominis Genesi*, Lipsiæ, 1827. This memoir, which was addressed

of zoology in the University of Königsberg*, belongs the honour of this discovery, certainly the most important ever made in the history of generation.

Sir Everard Home and Mr. Bauer, in a paper on *Corpora Lutea*, in the Philosophical Transactions for 1819, describe and delineate what they considered an ovum in the human ovary. The supposed ovum was in the cavity of a corpus luteum; and from the presence of the hymen they imagined the woman had died a virgin. There can be no doubt that Sir Everard Home and Mr. Bauer were mistaken regarding the nature of the body they describe; it corresponds in no respect with the human ovum as it exists in the ovary.

I may here also mention, that Dr. Plagge, of Benthaim (Meckel's Archiv. vol. vii.; and Meckel's Deutsches Archiv. for 1829), endeavours to shew that not only he anticipated Baer in the discovery of the ovum in the ovary, but that it had also been previously described by De Graaf, as well as Sir E. Home and Mr. Bauer. On these pretensions I may remark, that De Graaf's claim to the discovery is not better founded than that of Sir E. Home and Mr. Bauer; and as to the claim of Dr. Plagge himself, I can only say with Baer, that "notwithstanding the most perfect willingness to do so, I cannot understand what it is he says he saw."

In our examination of the ova of the mammifera, an acquaintance with the structure of the ovum of an oviparous animal will assist us much. With this view I shall mention the points most worthy of notice in the structure of the hen's egg in the first stage of its formation. But as it is at this period contained in the ovary, it will be necessary to premise a short description of that organ.

The parts common to every ovarium are a parenchyma and an investing membrane. To the former, Baer proposes to give the name of *Stroma*, and to the latter that of *Indusium*. In the stroma are imbedded the capsules, in which the ova are formed. In the vertebrata each ovum has a proper capsule; but in some of the invertebrata,

one capsule contains several ova. In this case, there is within the capsule, besides the ova, an almost amorphous substance, which Baer calls *stroma internum*. The capsules he calls *thecæ communes*.

In birds, the yolk and its membrane are the only parts of the egg which are formed in the ovary. The chalaziferous membrane, the chalazæ, the albumen, the membrane of the shell, and the shell, are added in the oviduct. The ovary of the bird may be described as a cluster of capsules with their contained yolks, varying in magnitude from that of a millet-seed to the full-sized yolk. The stroma, which surrounds these capsules, is a laminar cellular tissue, delicate, and in small quantity. The indusium is derived from the peritoneum. It envelops almost entirely each capsule, and forms along with the vessels of the latter a pedicle, by which the capsule is attached to the body of the ovary. The very small capsules not being so completely enveloped by indusium, are not pedunculated, but sessile. The capsule in which the yolk and its membrane are found, is composed of two laminae, an outer cellulo-vascular, and an inner, soft and spongy, which receives small vessels from the larger branches ramified in the outer.

The yolk consists, at first, of a granular membranous sac, within which is contained a fluid with granules suspended in it. The vitellary membrane is at this period extremely thin and transparent, and adheres very closely to the inner surface of the capsule. From these circumstances it is not readily seen, unless carefully looked for with the assistance of a magnifying glass. It is from not having noticed it at this early period, that Baer has mistaken the granular membranous sack of the yolk for it. "Thus," he says, "*vitelli membrana hoc tempore crassissima et granulosa est ita, ut optime mereatur nomen membranae granulosa, quam Purkinje quoque ita describit.*" This granular membrane, he supposes, divides afterwards into the proper vitellary membrane and a granular layer, forming the periphery of the yolk. His words are, "*Posthac membrana granulosa in stratum externum laeve, continuum—membrana vitelli puta—et stratum granulosum, vitelli peripherium efficiens, sejungi mihi visa est.*" Burdach (Die Physiologie als Erfahrungs-

in the form of an epistle to the Imperial Academy of St. Petersburg, was followed a year after by a commentary, illustrating more fully the author's views.

* Now of St. Petersburg.—ED. GAZ.

wissenschaft, vol. ii. p. 54.) follows Baer in this mistake; and Baer himself repeats it in his "Memoir on the Exfoliation of the Epidermis of the Embryo of the Mammifera, applied to the knowledge of the metamorphosis of Insects."

It is not until it has attained a considerable size, that the yolk presents the colour and structure which it is found to possess in the perfect egg. On one or other side of the yolk, generally near the pedicle by which the capsule is attached to the ovary, the cicatricula is observed. At this place the grains composing the periphery of the yolk being more aggregated, form a zone or disc, the inner surface of which presents a mammillary eminence or cumulus, turned towards the interior of the yolk. In the apex of the cumulus is a pellucid pore, which may be seen on either side of the cicatricula. This pore, which is quite circular, has a diameter of about one-seventieth of an inch. It is occupied by a very small vesicle, which is imbedded in a cavity in the granular substance of the cumulus. This vesicle was first particularly described by Professor Purkinje, of Breslau (*Observationes nonnullæ ad Ovi avium historiam ante incubationem*: Vratislaviæ, 1825.) It is, therefore, commonly known by the name of the *vesicle of Purkinje*. It consists of a thin transparent envelope, within which is contained a fluid, not perfectly limpid, as Purkinje says, but somewhat granular. When the yolk has left the ovary, the vesicle is no longer to be found.

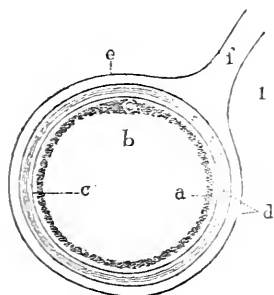


Fig. 1 is a diagram representing a section of the hen's egg within the capsule of the ovary, and the position of the vesicle of Purkinje.

a, The granular membrane, forming the periphery of the yolk.

- b, The vesicle of Purkinje imbedded in the cumulus.
- c, The vitellary membrane.
- d, The inner and outer layers of the capsule of the ovum.
- e, The indusium of the ovary. Betwixt the indusium and the capsule the stroma is seated.
- f, The pedicle by which the capsule is attached to the ovary.

Leaving out of view altogether the ova of the marsupial animals and monotremata, as I have never had an opportunity of seeing them, and as they have not yet, so far as I know, been minutely examined in the recent state, I proceed to the description of the ova of man, and mammiferous animals in general.

As in birds, so in the ovaries of man and mammiferous brutes, we find a parenchyma or stroma, and an envelope or indusium, derived from the peritoneum. But the stroma, immediately underneath the peritoneal envelope, is condensed into the form of a tunic, to which the peritoneum closely adheres. This is known by the name of the *tunica albuginea* of the ovary. Baer proposes to call it *indusium proprium*. The stroma in general is much denser in its texture than in birds. In the human ovary, which I assume as a type of that of the mammifera, the stroma is a dense laminar cellular tissue. Imbedded in it, and for the most part situated near the surface of the ovary, are found the vesicles of De Graaf. In the sow these vesicles rise considerably above the surface of the ovary; and in the hedge-hog they project so much, that the ovary resembles the raceme of the bird. The Graafian vesicles vary in size. The largest in the human ovary are about one-fifth of an inch in diameter. They sometimes project slightly above the surface of the ovary. As to the number of vesicles contained in one human ovary, that varies according to circumstances. In the ovary of a young adult as many as fifteen may be counted.

The proper capsule of the Graafian vesicle is composed of two layers. The outer is formed of dense cellular tissue, in which are ramified many blood-vessels. The inner layer is thicker, softer, and more opaque, than the preceding, to which it is closely united, and from which it receives vascular twigs.

The contained part or nucleus of the

vesicle of De Graaf consists of: 1st, a granular membrane, enclosing, 2ndly, a coagulable granular fluid; 3rdly, connected with the granular membrane on one side is a circular mass or disc of granular matter, in the centre of which is imbedded, 4thly, the *ovum*.

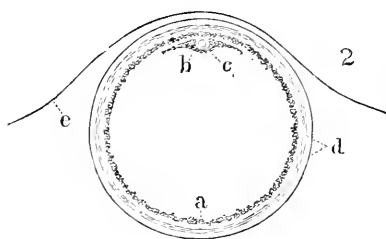


Fig. 2 is a diagram of a section of the Graafian vesicle and its contents, shewing the situation of the ovum.

- a, The granular membrane.
- b, The proligerous disc.
- c, Ovum.
- d, The inner and outer layers of the wall of the Graafian vesicle.
- e, Indusium of the ovary derived from the peritoneum, the stroma immediately underneath which is condensed so as to form the *tunica albuginea*.

The granular membrane is more tenacious and thicker the smaller the vesicle is. The disc of granular matter connected with it on the prominent side of the vesicle, appears to be at first quite distinct from it. This disc, called by Baer the *proligerous disc*, presents in its centre, on the side towards the interior of the vesicle, a small rounded prominence, called the *cumulus*; and on the opposite side a small cup-like cavity hollowed out in the cumulus. The cavity is for the reception of the ovum. To examine the ovum properly, it is necessary to separate entirely the proligerous disc from around it. A separation sometimes takes place spontaneously, particularly if the ovary has lain a day or two in water. It may, however, always be effected by careful manipulation with fine instruments under the microscope. I am surprised that Baer says he only succeeded once in doing this.

OF THE OVUM.—Baer calls this *ovulum*, not so much on account of its very small size, as from considerations which, in the course of this memoir, I

shall attempt to show are unfounded. I apply to it the name *ovum*, in the same sense as I would apply the term to the egg in any other class of animals.

The human ovum is so small, that it is just perceptible to the naked eye. It is about the one hundred and fiftieth part of an inch in diameter. Its shape is spherical, when suspended in water; but, in consequence of its softness, it falls into a lenticular form when there is not sufficient water to support it. When examined with the microscope it has the appearance delineated in figure 3. Figure 4 represents it still surrounded by the proligerous disc. The ovum has a soft external envelope, which is transparent, and very thick; hence is produced the appearance of a broad transparent ring forming its circumference, when the ovum is viewed with the microscope by transmitted light. Within this transparent external envelope is seen a substance composed of grains adhering together by the intervention of a delicate mucous tissue, so as to form a sac, which I have sometimes succeeded in withdrawing entire. (See figure 5.) Sometimes the grains do not hold together; hence, when the external envelope is torn, they escape, and diffuse themselves in the water surrounding the ovum. Baer appears to have misconceived the structure of the external envelope, for he describes it as a hollow sphere with thin walls, separated by a transparent interval from the central part of the ovum.

Besides the parts above mentioned, there is still another which enters into the structure of the ovum. It is a delicate transparent vesicle about 1-900th of an inch in diameter. It is seated at the inner surface of the granular sac. On one side of the vesicle there is a small elevation*, which, projecting among the grains composing the walls of the granular sac, fixes the vesicle in its place. This vesicle, which I had the fortune to discover (I made the observation in the beginning of September, 1834,) I consider analogous to that described by Professor Purkinje, in the cicatricula of the immature eggs of birds, and which exists also in the ova of other ovipara. Figure 7 represents the vesicle removed from the ovum. In figures

* I shall not at present venture on any speculation regarding the nature of this part, but that it performs an important function there can be no doubt.

3 and 6 the small elevation on the side of the vesicle is observed projecting among the grains of the granular sac. In figures 8 and 10 the whole vesicle is seen *in situ* in consequence of the grains composing the granular sac being few in number, and not adhering closely together.

As it is convenient to have a name for an object, I shall for the present employ for this vesicle the name of *germinal vesicle*, already given by Baer to the analogous part in the ova of the ovipara.

Besides the human ova, I have examined those of the cow, sheep, sow, rabbit, rat, and mouse. They differ in no essential particular. Those of the rat and mouse are smaller and more transparent. In all I have found the germinal vesicle, the size of which to that of the ovum is as 1 to 6.

Baer says he once found two ova in one Graafian vesicle of a bitch, and on another occasion he observed the same thing in the Graafian vesicle of a sow. If there is no mistake, this would be an approach to what is observed in some of the inferior animals, in which a single capsule contains several ova. I have found in the Graafian vesicle of the human ovary, an ovum somewhat like two joined together, but I could only perceive one germinal vesicle*. (See figure 10.)

Baer winds up his description of the ovaries and ova of the mammifera with the following parallel between them and those of other animals:—

“But these are sufficient for establishing a comparison between the ovaries of the mammalia and their contents on the one hand, and the ovary and ova of other animals on the other. We have in the mammalia a dense *stroma*, in which are imbedded the Graafian vesicles. If we look to the constituent parts of these vesicles only, we shall be struck with the remarkable resemblance they bear to the ova of other animals. Let us compare, for example, the ovary of any mammiferous animal with the ovary of a bird and a frog. There are present the indusium, stroma, and capsules, with this difference only, that the indusium and stroma in birds and frogs are more delicate than in the mammalia; that in frogs, moreover, the indusium being internal on

account of the internal cavities, appears very delicate; and lastly, that in frogs the capsules open towards the interior, and leave an internal calyx easily to be distinguished. Then comes the vitellary membrane, which in the mammalia exists under the form of a granular membrane. Seeing that the vitellary membrane in birds and reptiles appears at first under the form of a granular membrane, and is by and by exfoliated from it, I do not hesitate to compare the Graafian vesicle of the mammalia in this respect with the immature ovum of the other vertebrate animals. The yolk of birds and frogs is composed of many vitelline granules and a small quantity of fluid albumen. The Graafian vesicles differ only in this, that they have a smaller quantity of grains; but, on the other hand, a larger quantity of native albuminous fluid. In other respects they are similar to ova. They contain, in fact, a vesicle (the ovulum of the mammifera) imbedded in the *caulus*, and a proligerous stratum, at one time surrounding the vesicle, at another lying over it. *Therefore the Graafian vesicle, in reference to the ovarium and the body of the mother in general, is indeed the ovum of the mammalia.* But as regards evolution, it differs very much from the ovum of other animals, in which the whole nucleus of the ovum is carried off from the ovarium, to afford not merely a nidus to the future fœtus, but rather to be transformed into the fœtus itself. But in the mammalia the innate vesicle (*vesicula innata*) contains a more developed yolk, and, *in reference to the fœtus about to be formed, proves itself the true ovum. A fetal ovum, it may be called, in a maternal ovum.*” (De ovi mammalium et hominis genesi, p. 31.)

There can be no doubt that the capsule which contains the yolk in the hen's ovary is analogous to the capsule of the Graafian vesicle. At first sight, also, the nucleus of the vesicle of De Graaf resembles very much the immature yolk of the bird's egg. But I think there is no analogy between them. Thus there is no membrane similar to the vitellary membrane surrounding the nucleus of the vesicle of De Graaf. Baer's assertion that the vitellary membrane appears first under the form of a granular membrane, I have in a former part of this paper disproved. Therefore his comparison between the granular membrane of the nucleus of the vesicle of De Graaf and the vitellary membrane of

* In one instance, I think I have found two germinal vesicles in one human ovum.

the bird's egg, does not hold. As to the analogy which Baer has attempted to draw between the ovum of the mammifera and the vesicle described by Purkinje in the immature egg of the bird, I would remark that although, in its situation and mode of connexion within the Graafian vesicle, the former bears considerable resemblance to the latter in its situation and connexion, still these are the only points of resemblance. In all other respects they differ widely, as may be inferred by comparing the descriptions of them which I have given in this memoir. The vesicle of Purkinje, in fact, consists merely of a delicate capsule containing a fluid, while in the minute ovum of the mammifera we have all the essential elements of the egg of the bird and that of other ovipara—viz., 1. an external membrane which is analogous to the vitellary membrane of the bird; 2. a granular membranous sac, containing a thin fluid, together representing completely the immature yolk of the bird's egg; 3. a vesicle in every respect analogous to that described by Purkinje in the hen's egg, while still in the ovary*. On the existence of this latter part I would particularly insist.

From what I have said, I think I am justified in concluding, that in the small body discovered by Baer, in the Graafian vesicle, and which I have described in this memoir under the name of the *ovum of the mammifera*, we have the analogue of the egg of the bird, and that of other ovipara. I might also add, that the granular membrane, proligerous disc, and granular fluid of the Graafian vesicle, are probably superadded parts, of which there is no trace within the capsule of the bird's ovary†. They might, perhaps, be compared to the internal stroma of the acephalous mollusca. The occurrence of more than one ovum in a single capsule, as seen by Baer, would appear to favour this conjecture; for the internal stroma is found in capsules common to several ova.

From the above quotation it will be seen, that besides considering the ovum analogous to the vesicle of Purkinje, Baer correctly views it as a real ovum.

* Figure 9 is a diagram of a section of a human ovum.

† In the smaller Graafian vesicles of the mouse, I have found the ovum so large as to fill nearly the whole vesicle; so that there was very little granular matter corresponding to the granular membrane and proligerous disc.

It is, indeed, in it that the embryo is first formed. But to reconcile this fact to his hypothesis, he says that "the vesicle of the germ in the mammifera (as he calls the *ovum*) is developed in a preponderating manner, and draws to it all the generative faculty of the egg (*nucleus of the vesicle of De Graaf*).¹" In accordance with this notion, he calls the ovum of the mammifera, *ovum fetale*; and the nucleus of the Graafian vesicle, *ovum maternum*. "Mammalia ergo," says he, "habent ovum in ovo aut, si hæc dicendi formula uti licet, ovum in secunda potentia." To this, Baer adds, in a note, "Quapropter in vesicula Graafiana describenda voce *ovuli* semper usus sum, quia vesicula Graafiana ipsa ovum refert, respecto ovario, ex ovulo autem fit ovum fetale." The reason why I have used the term *ovum* is already obvious.

I cannot agree with Baer in his determination of the parts of the *ovum fetale*, as he calls the ovum of the mammifera. He considers the outer membrane of the ovum analogous to the membrane of the shell in the bird's egg, and accounts for a vitellary membrane, by conjecturing that there forms on the outer surface of the granular membranous sack, which is the vitellus of the ovum, a thin pellicle, in the same manner that he erroneously supposes the vitellary membrane of the bird's egg is produced. There is no such pellicle as that spoken of by Baer, and all analogy leads us to suppose that the external envelope of the ovum of the mammifera does not correspond with the membrane of the shell of the bird's egg, but rather with the vitellary membrane; although it, at the same time, appears to perform a different and a higher function*.

POSTSCRIPT †. — Since writing the preceding memoir, I have learned that M. Coste, in a work entitled "*Recherches sur la Generation des Mammiferes*," mentions his having observed, in the ovum of the rabbit, a vesicle which he compares to that described by Purkinje in the bird's egg. From this it would appear that M. Coste has antici-

* Mr. Jones here alludes to the opinion he entertained at the time this memoir was written—that the vitellary membrane of the mammiferous ovum afterwards formed the chorion. Mr. J.'s subsequent researches, as detailed in his second memoir, have proved this opinion incorrect.—ED. G. & V.

† This postscript was read before the Royal Society, along with the rest of the paper.—E. G.

pated me in the discovery of the germinal vesicle in the ovum of the mammifera.

The results of M. Coste's researches, however, which appear to have been confined to the ova of the rabbit, do not altogether correspond with what I have observed in the numerous examinations which I have made of the ova of man and several other animals, besides those of the rabbit. I am, therefore, not without the hope that my memoir will be found still to contain something both of novelty and interest.

M. Coste states, in his memoir, that he had examined 200 ova of rabbits before he observed the vesicle. He then goes on to say — “Je commençais à croire que les mammifères en étaient privés; lorsqu'un jour ouvrant une lapine non fécondée, dans le seul but d'étudier les œufs dans l'ovaire, je remarquai pour la première fois, un petit point transparent, parfaitement sphérique, placé à la surface du vitellus. Ce fait nouveau devint l'objet de l'examen le plus attentif, et après quelques instans d'observation je vis le petit point transparent se déformer et s'effacer complètement. Un second œuf fut placé sous le microscope et me presenta un phénomène semblable: il en fut de même pour presque tous ceux que renfermaient les ovaires de cette lapine. Le petit point dont il est question n'est

autre chose qu'une vesicule d'une ténuité et d'une transparence telles qu'il est impossible de rien voir qui ressemble davantage à une bulle de savon, dont elle a toute la fragilité.”

In the above extract from M. Coste's work, the vesicle is described as lying at the surface of the vitellus. I have always found it within the granular membrane composing the periphery of the vitellus, and distinctly visible at the surface only in those cases in which the grains of the vitellus were not coherent. In all my examinations of the ova of the mammifera, I never saw the germinal vesicle shrink and disappear, as M. Coste describes it to do; but I have generally been able, by tearing open the ovum under the microscope, to separate the vesicle quite entire, and examine it in the detached state. This M. Coste seems never to have done, although, without doing it, the vesicle, from its situation, can seldom be seen. The germinal vesicle is certainly very delicate and transparent, but I have not found it so fragile as M. Coste represents it to be. As to what M. Coste says about the necessity of looking for the vesicle soon after the death of the animal, I must remark that I have found it as distinct a week or ten days after the death of the animal, as at an earlier period.



Fig. 3 represents the human ovum as seen under the microscope by transmitted light. It is magnified forty-five diameters. On one part of its surface a small spot is seen, which is the elevation on the side of the germinal vesicle.

Fig. 4 is the ovum still surrounded by the proligerous disc, magnified fifteen diameters.

Fig. 5 represents the outer envelope of the ovum lacerated, and the granular membrane sac, composing the yolk, drawn out.

Fig. 6 exhibits a human ovum magnified forty-five diameters, in which the granular sac is so much contracted, that it does not fill up the whole cavity of the external envelope. The spot on the surface of the granular sac, which may be considered as the cicatrícula, is the elevation on the side of the germinal vesicle.

Fig. 7 represents, within a square area, the human germinal vesicle magnified forty-five diameters. On one side of it is seen the small elevation.

Fig. 8, a human ovum magnified forty-five diameters. In it the grains composing the granular sac were in such small quantity, and adhered so little together, that the whole germinal vesicle is seen shining through the outer envelope.

Fig. 9 is a diagram of a section of an ovum, representing the thick external envelope, within which is the granular sac, and, connected with the inner surface of the latter, the germinal vesicle.

Fig. 10 represents a human ovum, which I found in the ovary of a married woman of about thirty. It is somewhat like two joined together, but there is only one germinal vesicle.

RECOLLECTIONS OF CHOLERA;

ITS NATURE AND TREATMENT.

BY WILLIAM GRIFFIN, M.D.

Limerick.

Mortality of Cholera.

No. IV.

BEFORE entering on the consideration of the effect of remedies in cholera, it is above all things essential to have clear notions respecting the usual mortality of the disease in various circumstances. From the extraordinary fact

that the average mortality has been nearly the same in all countries, varying between one-third and half, those who reason on loose and general observation of the disease have been naturally led to conclude the medical art is altogether unavailing in its treatment; while others, whose views are limited to the narrow circle of their own experience, see, in the occasional diminution or increase of deaths, either new and successful modes of management or utter failures in practice. We shall not be so ready to complain of these or other erroneous opinions entertained on the subject by the public, when we consider that few, even among medical men, have sufficient facts before them to reason correctly at all in the case, and that those who are out of the profession have little other means of forming an estimate of medical practice except the amount of mortality. A prevailing and very natural error among the latter, is the belief that medicine cures, not merely by interrupting or altering some destructive process, or by assisting some spontaneous vital efforts at recovery, but by some inherent and absolutely restorative virtue. They do not know, or they do not consider, that the constitution is maintained in its healthful condition by a self-preserving influence, ever silently at work, but openly displaying its energies when called upon to repair the effects of injuries, whether inflicted by physical violence, by poison, or by disease; that the medical art can only be of use in proportion to its capability of assisting this influence, and must necessarily fail wherever the latter is exerted feebly or unequally, since medicine may sustain the vital efforts of the system in resisting any destructive process, but cannot possibly altogether supply their place.

A knowledge of this fact leads readily to the reflection that, in all severe diseases, the weak, the sickly, and diseased, must die under any treatment, and even a proportion of the strong and full-blooded, probably for a reason the very opposite to the former—that the actions which the preservative influence calls up, are, in such habits, apt to exceed the requisite amount, so that life is extinguished by the very violence of the natural effort to resist or repair the injury. There is thus a very certain and known average of deaths in almost all dangerous maladies, under the best

treatment. In typhus fever, this inevitable mortality varies according to the virulence of the epidemic; among the poor, from one in ten to one in thirty-two—among the rich, from one in five to one in twenty; being generally the same in the same epidemic. The mortality in the simply confluent small-pox, is (according to Dr. Gregory, of London) three in five; which is little short of that occurring in the collapse of cholera, when judiciously treated. In the coherent description, the mortality is one in four; and on the whole, it has been computed that out of every six persons who take small-pox in the natural way, one inevitably dies.

Cholera is a malady so rapid in its progress, and the consequent mortality is so great, that even successful treatment in its advanced stage is unsatisfactory, and has, indeed, the appearance of absolute failure. The fact nevertheless is, that no severe disease with which we are acquainted is so curable, or so capable of being influenced by medical treatment, if under judicious management from its commencement. Our judgment on this subject has been misled by our aptitude to form general conclusions, without comparing the actual results with those which we might rationally anticipate. All formidable diseases, if uninterrupted, commit much mischief in the frame, and the time comes, earlier or later, in which that mischief is irreparable. The previous period is obviously the only one in which we can hope for any success from our remedies; and this, in cholera, extends sometimes but to a few hours. It is essentially a rapid disease, like those which follow the exhibition of poisons, and we should no more consider it incurable, or beyond the control of medicine—because in its last stage, which it reaches so speedily, we are seldom able to bring about recovery—than we should that a person who has swallowed opium, or prussic acid, is irrecoverable, because those who have taken extravagant doses, or have had assistance too late, invariably die.

In estimating the mortality in cholera, it is highly important to inquire, first, what its amount might be in cases abandoned to nature; and secondly, what it has been under the most successful medical treatment. The extreme difficulty of attending an inquiry of this kind, can only be conceived by those

who are familiar with the variable character of the disease itself at different periods and places, and with the loose and uncertain nature of even medical records, or evidence. In considering the first point, it is necessary to hold in view the following habits of the disease:—

When first it sets fairly in, in any district, it is usually of a virulent description, that is, of the rapid type, all the cases in which medical assistance is not resorted to, speedily terminating in death, and more than half running into collapse before it can usually be obtained.

It generally mitigates in severity after some days or weeks: cases will then recover, under treatment, which, in the rapid type, would prove utterly ineffectual, and in some rare instances without any treatment at all.

It generally returns with sudden and renewed malignancy to any place it has once visited, and again as unexpectedly becomes mild and disappears.

While it prevails in any town or village, it is more or less variable in its character. The cases occurring in one night, or in one street, may run their course with frightful fatality, and in the very next assume the milder or protracted form. An hospital physician, in this way, may sometimes, on seeing a patient brought in, tell in what part of the city he was taken ill, or on learning the locality predict his recovery or death.

From this variable character of the disease, one may readily apprehend the difficulty of instituting any fair comparisons between different modes of treatment, and the danger of drawing inferences from a small number of cases, or without reference to the place or period of their occurrence: it would, in fact, be impossible to avoid error even with vast records before us, if we had not besides some certain mode of ascertaining in all cases at what stage of the disease it had fallen under medical care, such as we are furnished with in the temperature of the tongue and skin and in the state of the pulse. It is universally acknowledged that when it can no longer be felt at the wrist, the recoveries are few under any known treatment. But in those instances in which the pulse is still perceptible, many remedies, as we shall hereafter show, are highly influential, and some almost certain in arresting its course. In comparing the

results of cases abandoned to nature with the results of these medically treated, it is therefore necessary to consider separately such as are treated previous to, and after, the extinction of the pulse.

It was said by practitioners in India that no one recovered from cholera without medical assistance, and it certainly appeared to me, from my earlier experience, to be equally true of the disease in this country; for many months during which I witnessed the progress of hundreds of cases, both in and out of hospital, I did not meet with a single instance of natural cure; nor could I ascertain, on most diligent inquiry among medical friends, that any such had fallen under their observation. When, afterwards, the complaint partially subsided in Limerick, and began to attack the towns and villages throughout the surrounding country, where the people evinced the strongest objections to go into hospital, there was a still better opportunity of estimating the probability of natural cures. In all of these places, however, the mortality was reported as universal. In Scariff, Kildysart, Castle-Connell, Bruff, Croom, Hospital Rathkeall, Adare, Askeaton, and other small towns, I could not learn that a single recovery had taken place where medical means were wholly rejected. If considerable information, and even very extensive personal experience of this disease would allow of my forming any absolute inference, I might at this period have fairly concluded, with most medical men, that natural recovery from cholera was impossible. I had, however, some doubts remaining suggested by the many cures I had seen effected under very different, and sometimes opposite modes of treatment, and still more by a few extraordinary ones which had occurred in the hospitals in circumstances of such extreme neglect as made it quite allowable to consider them abandoned cases. At length, on the second violent irruption of the disease in Limerick, after it had been desolating the city for three months, I first witnessed the natural recovery of three individuals in one family in the neighbourhood of Garryowen. On visiting the house, I found the mother and son lying upon straw, with the usual symptoms of the complaint; an infant was stretched on his face and hands in the middle of the floor, where it had crawled from the bed, and was purging pro-

fusely. The matter ejected was bilious, as if the attack was only commencing. The mother would not allow any of them to be removed to the hospital, in consequence of her husband having died there a few days before. As they had no attendance, and even no drink, except what the humanity of their neighbours induced them occasionally to lay near the bed, I concluded that they would all shortly run into collapse and die. To my great astonishment, however, on the second day after, when standing in the ward of St. John's Hospital, I saw the three brought in; the mother and infant in light fever, the boy more severely affected, but all in a state that led me at once to predict their recovery: the latter, in fact, was the only one who required any serious treatment. I could not ascertain whether they had ever gone into perfect collapse; the mother could scarcely have done so, having all through taken care of the child. Since then I have seen two or three other instances in which the patients did well merely by slight medical assistance in the febrile stage. Indeed, it was now evident to me, that many patients survive collapse if merely supplied with diluent drinks—perhaps from a fifth to a third of all who fall into it; and that the almost universal fatality in neglected cholera arises, not from their invariably dying in the most destructive stage of the malady, but from their sinking in the subsequent fever.

The difficulty of establishing the possibility of natural recovery furnishes us with sufficient evidence of its extreme rarity; but this will be yet more clearly seen on advertent to what might be considered a law of the disease, if any rule could be called a law to which it can be justly said some exceptions have occurred:—*That without medical treatment every person attacked with it will fall into collapse, although they may not eventually die; and that such as recover do not do so by an arrest or cessation of the disease, but by struggling through, and outliving all its stages.* It hence follows, that in estimating the probable chance of recovery in all cases left to nature, we may regard them as cases of collapse, to the frightful mortality of which we have before alluded.

It shall now be shewn, by a reference to the hospital reports, that this rule or

law does not at all apply to cases actively treated; that medicine is not simply influential, as in fever, in mitigating symptoms and assisting the patient through its different stages, but that it commonly, and almost certainly, arrests the disease if resorted to in time. This is so true, that if it does not arrest it—if the progress of the disease is not absolutely interrupted—if it goes on through collapse and fever, the patient is almost sure to die. In proof of these assertions I must confine myself to the reports of the hospitals in Limerick, to which I was myself attached. Singular

to say, the reports transmitted to the London and Dublin Boards of Health, from all parts of the kingdom, are entirely useless as materials for medical reasoning, the distinction of cases as they happened to be admitted with or without a perceptible pulse having never been observed. If it was the general design to mystify the subject, and prevent the possibility of fair comparison under different modes of treatment, it could not have been more effectually done than in those loose and useless records.

THE STRAND HOSPITAL*.—*From June the 8th to June 22d, 1832.*

	Admitted.	Died.	Recovered.
In the primary stage	24	4	20, being 16 deaths in 100.
In collapse	20	17	3, being $1\frac{1}{2}$ recoveries out of 10.
Total	44	21	23

Making the mortality nearly one in two upon the whole, the cases in collapse amounting nearly to half the total admissions.

ST. MICHAEL'S HOSPITAL.—*From June 14th to July 1st, 1832.*

	Admitted.	Died.	Recovered.
In the primary stage	74	12	62, being 16 deaths in the 100.
In collapse	80	67	13, being $1\frac{1}{2}$ recoveries in 10.
Total	154	79	75

Making the mortality one in two upon the whole; the cases brought into hospital in collapse exceeding half the admissions.

From July 1st to August 1st, 1832.

	Admitted.	Died.	Recovered.
In the primary stage	119	5	114, being only 4 deaths in the 100.
In collapse	46	42	4, being only 1 recovery out of 10.
Total	165	47	118

Making the mortality less than one-third upon the whole; the cases brought in in collapse exceeding one-third.

THE NUNNERY HOSPITAL.—*From June 8th to June 22d, 1832.*

	Admitted.	Died.	Recovered.
In the primary stage	128	7	121, being 6 deaths in the 100.
In collapse	154	117	37, being little more than $2\frac{1}{2}$ recoveries out of 10.
Total	282	124	158

Making the mortality one in two and a quarter upon the whole; the cases brought in in collapse exceeding half.

ST. JOHN'S HOSPITAL.—*From June 8th to July 18th, 1832.*

	Admitted.	Died.	Recovered.
In the primary stage	419	29	390, being 7 deaths in the 100.
In collapse	264	195	69, being over $2\frac{1}{2}$ recoveries in 10.
Total	683	224	459

Making the mortality, upon the whole, one in three; the cases brought in in collapse amounting to more than a third of the admissions.

* In these reports there is no separate column for bilious diarrhœa; such cases were regarded as merely premonitory, and usually treated at the Dispensaries. The stage of rice-water discharges is called the primary stage.

ST. JOHN'S HOSPITAL.—From its re-opening, Aug. 21st, to its final closure, Sept. 13th.

	Admitted.	Died.	Recovered.
In the primary stage	59	8	51, being 16 deaths in the 100.
In collapse	61	43	18, being 3 recoveries in 10.
Total	120	51	69

Making the mortality, upon the whole, considerably less than half, or about two fifths; the cases brought into hospital in collapse, being nearly three-fifths of the total admission.

BARRINGTON'S HOSPITAL.—From Sept. 23d, 1832, to April 17th, 1833.

	Admitted.	Died.	Recovered.
In the primary stage	114	121	96
In collapse	103		

Making the mortality, upon the whole, more than half; the cases brought in in collapse having been less than half. The proportionate deaths of those admitted in the primary stage and in collapse are not recorded.

The cases running into collapse, on admission, which were yet arrested, are not given in any of these tables; they were usually classed with those in the primary stage. In the last cases admitted into St. John's (from the 28th of August) they amounted to 8, with the 8 (primary stage) which terminated fatally, making 16. This would give on the whole, cases admitted in collapse and running into collapse, 77; deaths, 51; being 26 recoveries, or nearly 4 in 10.

I have already mentioned that the perceptibility or imperceptibility of the pulse was agreed on, by the medical

gentlemen engaged in these hospitals, as the criterion by which collapse was established. The rice-water purging and vomiting were regarded as the diagnostics of confirmed cholera. All patients with perceptible pulse, however sunk or cold, were recorded in the primary stage. It was, of course, impossible to avoid occasionally admitting persons with mere premonitory diarrhoea and vomiting; but these, I believe, seldom exceeded a tenth of those registered as admissions previous to collapse, and in the last instance, in St. John's Hospital, did not amount to more than five in the hundred.

ANALYSES AND NOTICES OF BOOKS.

“L'Auteur se tue à allonger ce que le lecteur se tue à abrégier.” — D'ALEMBERT.

On the first Changes in the Ova of the Mammifera, in consequence of Impregnation, and on the Mode of Origin of the Chorion. By THOMAS WHARTON JONES, Esq.

SUCH is the title of a paper in the Philosophical Transactions for 1837, Part II. The subject is one of so much interest, that we shall copy the paper into our journal; but as about two years and a half ago, another paper by the same gentleman was communicated to the Royal Society, on the Ova of the Mammifera before impregnation, we have given it first, for the better understanding of the other, (see present No. page 689.) This we the more readily do, as in the paper communicated two years and a half ago, which was only

read, not published, is detailed a discovery of great importance in reference to the physiology of generation. We mean the discovery which Mr. Wharton Jones made in the human unimpregnated ovum as well as in that of other mammifera, of a vesicle analogous to that first described by Professor Purkinje, of Breslaw, in the immature eggs of birds and other ovipera. It appears that some time before Mr. W. J. made his discovery, the existence of such a vesicle was asserted in France by M. Coste, of Paris, from some observations he had made on the ova of the rabbit; but an examination of his work will show that he rather vaguely indicated than unequivocally demonstrated the existence of the vesicle. This indication of it, however, by M. Coste, was an inducement to Professors Purkinje and Valentine to recommence their investigations on the subject. The result was a very elaborate description of the vesicle by Valentine, though still incomplete, for he had overlooked one important point,

the spot on the side of the vesicle. This deficiency in Valentin's description was afterwards supplied by Wagner.

Hence, it appears that, like many other discoveries in science, the germinal vesicle of the mammiferous ovum was discovered about the same time in this country and on the continent. But it will be seen from Mr. Jones's Memoir, that he had investigated the subject so fully and successfully, as to be able to give such a description of it as is only to be drawn from the united labours of Coste, Valentin, and Wagner.

A comparison of dates will determine the mere matter of priority. Mr. Wharton Jones's observations were made in the beginning of Sept. 1834, and his memoir read before the Royal Society, 18th June, 1835.

We may mention that MM. Coste and Valentin received the great prizes of the French Institute for their communications on the subject.

the rate-payers half a sovereign, and ruined the patient for life.

The cases of this sort reported in the newspapers form, of course, but a small fraction of those actually occurring. For though the supporters of the new law seem to think this kind of homicide very justifiable in the abstract, yet, when it comes to individual instances, neither guardian, surgeon, nor master of workhouse, is much inclined to boast of his share in the transaction. In plain English, there is no doubt that most of these cases are hushed up. Yet those which make their appearance in the daily papers are so numerous, that if we were to insert them all, we should have little room for aught else. On the other hand, it would be a base dereliction of our duty as medical journalists to pass them over entirely: for it must never be forgotten, that these horrors cannot be perpetrated on the large scale without the subserviency of medical practitioners. In single instances, indeed, as at Bridgwater, a board may run its wild course in direct opposition to the medical officers; but, on the long run, if surgeons would imitate the plain-speaking honesty of the one in the Portsea Union, who wrote down *starvation* as the cause of a patient's illness, some mitigation of the present cruelties must take place. Unsupported by professional authority, the boards would be forced to give up their water-gruel experiments, and would no longer be able to boast that, in grinding the faces of the poor, they were backed by those who ought to be among their most zealous protectors. The case which has elicited these reflections, and which is reported in the *Times* of January 16, would formerly have been called one of the most aggravated cruelty and oppression. In the present day, however, we suppose that some milder name must be found for it; for cases of a blacker

MEDICAL GAZETTE.

Saturday, January 27, 1838.

"Licet omnibus, licet etiam nobis, dicere, item
*Artis Medice tueri: potestas modo non est in
 publicum sit, dicendi periculum non recusat.*"

CICERO.

ANOTHER DEATH FROM STARVATION.

WE had almost given this article the title of *another half-crown saved*; for the practical working of the Malthusian Act has made the loss of a life and the gain of a piece of money convertible terms. When the Poor-Law Commissioners point with triumph to the savings effected by the new system, they ought in candour to tell of the mortality from typhus fever in their workhouses; and if the Bridgwater Guardians are allowed to exult at the dexterity with which they baulked their surgeons of their fees, they should at the same time narrate the case of Charlotte Allen, where their frugality gained

dye occur so frequently, that the death of an aged woman by hunger and cold, from the refusal of all efficient relief, shews fair in comparison.

The inquest on this victim to the new boon to the poor was held at the Plough Inn, Clapham Common, on the 12th and 15th of January. Her name was Mary Stonard, and her age 61. The evidence was as follows:—

Charles Hughes, waiter at the Plough, deposed that on the 11th the deceased and another woman came into the tap-room; the former was so exhausted that she could hardly walk; her companion called for a pint of warm beer, and gave it to the poor old woman, but she was unable to take any. *“Some parings of potatoes were on the table, which the female who was with the deceased solicited to have, observing that herself and the deceased were almost starved to death. The latter at this moment fell upon the floor insensible.”*

A surgeon now attended, who adopted the almost universal practice in all cases of apparent death, or insensibility from accidents*—that is, he opened a vein; but the unfortunate woman was past all human aid.

A police constable then produced twenty-two duplicates, and a snuff-box of no value, which he found on the person of the deceased; with a few pieces of coke, and a farthing in money.

Mary Ann Stonard, daughter-in-law of the deceased, deposed to the privations which the deceased, her husband, and the witness, had suffered together for some months; the old man having long been so lame and infirm as to be incapable of work, and the occasional relief obtained from the parish of Putney having been so trifling, as scarcely to keep them from starving.

“On Thursday morning the deceased

applied for relief, but was refused, the relieving officer observing, that if she wanted any she must go to the Board of Guardians of the Wandsworth Union, at Clapham; *her mother started to go, although the distance was four miles.* A female, named Sarah Beadle, accompanied her, as she was in a very weak state from want of nourishment.”

Daniel Stonard, the husband of the deceased, a poor emaciated old man, who could not walk into the room without assistance, stated that he had long been out of employ, and that his family lived by a little washing, which his daughter and late wife occasionally obtained, but which was scarcely enough to keep them from perishing. The relief given by the parish was so scanty as to be of very little avail. On the Saturday previous to the death of his wife they obtained 1 pound of mutton, and two 2 pound loaves. *This was the first relief for several weeks.*

“A few months back himself and his wife were sent to the Board of Guardians at Clapham for relief; they waited from 9 o'clock in the morning until 6 o'clock in the evening, but obtained none.”

Relief, indeed!

He also said that the parish of Putney consented to take him and his wife into the workhouse, but that she refused, *dreading the regulations which are enforced by the new system.*

James Unwin, the relieving overseer, said that the deceased applied for relief about a month ago, when, in pursuance of instructions from the Board of Guardians, he told her he would give them an order to go into the workhouse, which she refused to accept. She was a hard-working, deserving woman.

Mr. Greenwood, a surgeon, stated that he had examined the body of the deceased, and found all the internal organs healthy. The stomach was contracted, and quite empty. The witness

* Vide MEDICAL GAZETTE, vol. ii. p. 214.

was decidedly of opinion—that Mary Stonard died from exhaustion, owing to the want of the common necessities of life, coupled with the extreme severity of the weather.

The jury returned as their verdict, “That the deceased died from exhaustion, induced by the extreme severity of the weather acting on a frame very thinly clad, fatigued, and in want of the common necessities of life.”

Few, we should hope, of those who voted for the Poor-Law Bill, imagined that we should so soon reach such an *acmé* of cruelty. Little did they think it would so soon be the glory and the shame of our legislators to make the workhouses of England so terrible, that lingering death, by starvation, was to be preferred to them. Mary Stonard's choice was a natural and a common one.

It appears that from the severity of the weather, and the consequent suspension of labour, the distress among the poor of Maidstone has been most alarming. A meeting, to take their state into consideration, was held in the town on the 15th instant, with the senior churchwarden in the chair. He observed that representations had been made to the Commissioners on the subject, and their reply was, “Fill all the workhouses, but relieve no one.” In consequence, at the last meeting of the guardians, orders had been made out for the admission of about 100 persons who had applied for relief, but with two or three exceptions, the poor had refused to accept them. In one case a gardener, whose employment was put an end to by the weather, applied for relief, and received an order for himself, his wife, and six children, to be received into the workhouse; he and his two eldest children to go to one house, and the mother and the younger children to another. “He resisted this separation as long as he could, but at last he went,

and said it was impossible to exist in the state in which they were,—they had no fire, and had been living on cold potatoes for a week! His wife, true to a mother's feelings, still refused to go into the workhouse.” Had it not been for the exertions made in behalf of this poor gardener by the worthy churchwarden, eight more inquests must have been held, and eight more items added to the long catalogue of Poor-law slaughters; while the Commissioners would have pointed with triumph to the immense savings effected in the Maidstone Union.

The triumvirate refuse to give outdoor relief, they do their utmost to make the workhouses intolerable, and then they cry out with a sneer, “Why don't you go into the workhouse, if you want relief? You'll find shelter there, and plenty of physie, and some water-gruel.”

We scarcely know where to find the counterpart of this system; but it has some resemblance to a strange story which we have read, but which we certainly never expected to find paralleled in real life, and above all in England.

The inhabitants of a certain town complained that their hospital was too full, and consequently too expensive; on which a vigorous economist undertook to relieve them. He went into the hospital, and whispered it among the patients as a great secret, that it was the intention of the governors to sacrifice the patient who was most desperately ill, for the benefit of the rest. In fact, the sick man most utterly despaired of was to be put to death, and administered in the form of *spiritus cranii humani*, &c. &c. to his fellow patients. The man who supposed himself to be intended by this description, got out of bed as well as he could, and crawled home. The frightful report still continued, and one after another made his escape, till the hospital was cleared. The satisfaction of some of the rate-

payers is said to have been extreme, but we do not recollect whether the feat was called a boon to the sick. The great difference between this story (which we used to consider as a mere extravaganza) and the present method of clearing workhouses is, that in the former case the patients were driven out by the fear of an imaginary ill; in the latter, the indigent are thrust out, or deterred from coming in, by real and tangible miseries. The wretched woman at Putney, or the hundred victims of Malthusian principles at Maidstone, were not scared away from the Union bastiles by ideal fears, nor even by reading dietaries in Poor-Law Reports; but they had heard from friends and relations who had escaped from these dens, of the meagre broth, the minute slices of bread, the "never-ending, still-beginning" doses of water-gruel; and, worse than all, of the contempt of all the charities and decencies of life—of the separation of husband from wife—of mother from child!

Let us hear, then, no more of the savings of the new system; or if Mammon cannot refrain from counting his gains, let him be reminded at each page of his balance-sheet, that even gold may be bought too dear; and that those who coin the blood of the poor for drachmas, are not better than speculators in human flesh, and must rank with kidnappers and slave-dealers.

UNIVERSITY OF LONDON.

It is amusing to learn, that, by one of those numerous happy circumstances which have characterised the rise and progress of this institution, the charter was so framed as to expire with the demise of the crown. The consequence of this was, that from the death of his late Majesty until last week (when a new charter was completed), the University had virtually become a nonentity.

Two of those originally nominated

having declined to act—namely, Mr. Sheepshanks and Lord Brougham—the places have been severally filled up by the addition to the Senate of the Bishop of Norwich and Dr. James Somerville. The latter gentleman, it may be remembered, was proposed by Mr. Warburton, as secretary, or registrar, to keep the records of the institution, but it was understood that the other members of the University could not be induced to concur in the appointment. At that time it is evident that the idea of his being made one of the Senate had not yet suggested itself. Those in the secret assure us that the nomination has been made in order to give *weight* to the medical department.

CLINICAL LECTURE

Delivered, Dec. 16th, 1837, at the Middlesex Hospital,

By DR. WATSON.

Pleurisy—Signs of Effusion—of Distension of the Pleura—Diagnosis—Treatment—Question of Paracentesis—New Auscultatory Signs consequent on the operation—Period when it should be performed—Results of the operation in several cases.

GENTLEMEN,—The case of the patient whose dead body we examined last Monday, has been one of painful interest. You expect, I am aware, that I should make it the subject of my lecture to-day; and though I have little more to say than what I have said already, and many times, in our familiar and more strictly *clinical* confabulations, it may be useful to some among you, if I review the whole case collectively, and again point out the various lessons and practical considerations, which it appears to me to furnish.

Thomas Coggs, aged 25, was admitted on the 20th of September, with the ordinary symptoms of acute pleurisy, namely, a stabbing pain beneath the left breast, shooting round at times to the spine, increased by movements of the body, and by a deep inspiration; frequent breathing; inability to lie on the painful side; thirst and fever.

The pain came on suddenly in the night of the 16th; at first it was unattended with cough.

On his admission we discovered the physical signs of effusion into the left

pleura; dulness on percussion over the lower part of that side; and what is called *œgophony* about the inferior angle of the scapula. In spite of treatment, fluid continued to be poured forth, until the whole of the pleura was distended with it, and the lung of that side rendered useless, and the heart dislocated.

The treatment consisted in venæsection, leeching and cupping the affected side, purgatives, calomel and antimony, calomel and blue pill combined with opium.

How did we know that the left pleura was full of fluid—was *distended* by it?

By the following circumstances, to which I beg your attention:—Complete dulness on percussion of the whole of that side; a total absence of the vesicular breathing; the bronchial voice and tubular respiration, along the side of the vertebral column; inaction of the left side during respiration.

But perhaps you will object that the circumstances I have just mentioned might result from some other condition of the parts within. Suppose the whole lung rendered solid by inflammation (hepatized, as they say,) so as to admit no air; this, also, would cause dulness on percussion, bronchial voice and breathing, immobility of the diseased side. True. And how, in such a case, is the diagnosis to be made? We had unequivocal additional signs to guide us. But supposing we had not; let me take this opportunity of telling you the points to be looked to in a more ambiguous case than ours.

First, then, we distinguish such cases, whereof the physical signs are much alike, by their *history*. In Cogg's we had the history of pleurisy: sharp pain without cough, without expectoration, preceded the dulness, and so on. And we had *not* the history of pneumonia—no crepitation—no rusty expectoration.

But setting that help aside also, for we may sometimes be called upon to treat a case of which we can obtain no previous history, we still may be assisted to the discovery of the true nature of the disease by attending to various particulars, some of which I will now mention:

1st. The dulness was *complete* and *uniform*. This is seldom the case when the lung has been solidified by pneumonia, still less when by tubercular deposit. The dulness is then partial; or if it extend over the whole side, it is unequal in degree in different places.

2d. The intercostal depressions were effaced; the diseased side was smooth and level: on the other you could trace the ribs, and the furrows between them, by your eye. This is a sign of distension: the intercostal muscles are stretched, and contract no longer. There was no *bulging* of the intercostal spaces; they never, I

believe, become prominent, but they are raised to the level of the bones, and the surface is even. Now when the lung itself is rendered solid by disease, it does not thrust outwards and obliterate the intervals between the ribs. This, consequently, is an important circumstance to notice in doubtful cases. It has been particularly studied by Dr. Stokes, who attributes the yielding of the intercostal spaces to the effect of the *inflammation* of the costal pleura upon the muscular tissue in contact with the inflamed membrane. It seems to be his opinion that the muscles are involved in the disease, and lose their contractile power. The same physician states that the elevation of the intercostal spaces does not occur in simple hydrothorax, independent of inflammation. But in simple hydrothorax we do not meet with a sufficient quantity of fluid to compress the whole lung, and then to exercise a centrifugal distending force; and this appears sufficient to explain the difference.

3d. The dulness on percussion did not stop short at the middle of the sternum, as it does when it results from a solid state of the lung, but extended some way beyond it, towards the right side. This is another valuable sign of distension. The mediastinum is forced out of its natural place.

4th. When one's flat hand was laid upon the patient's left side, and he spoke, no vibration of his voice, or next to none, could be felt; while on the right side, where the vesicular respiration was loud and *puerile*, and the voice natural, the thrill was strong. This would not have been so had the lung been *solid* on the diseased side; the difference would have been the other way; the thrill would have been stronger on the dull than on the resonant side. When the lung is merely rendered solid, the larger bronchial tubes are not compressed, as in a greater or less degree they are when the pleura is distended by a liquid; and the vibration occasioned by the voice is more readily conducted, as its sound is, through the solidified than through the spongy lung. Of this I had painful evidence (though I had noticed the fact before) in the person of one of my colleagues at King's College, the late Professor Burnett. He first asked me to examine his chest, during the Christmas holidays in 1834. The right side was perfectly dull, from the clavicle to the mamma; the voice heard in the same part, by means of the stethoscope, was startlingly harsh and loud; and the thrill, felt by the hand, much more manifest than on the left side, which was comparatively exempt from tubercular deposit.

I reckon this a most valuable distinctive mark, for it is within the reach of the

most ordinary observation. It requires no tact or practice, or education of the senses; but when once the attention has been called to it, is as readily perceptible by a nurse, or by the patient in the next bed, as by ourselves. If one side be morbidly dull, and the thrill is greater than on the other side which sounds healthy, the dullness denotes solidity of the lung; if the thrill be palpably less, the dullness signifies that there is liquid in the pleura. This simple test may often decide the question between the consequences of pneumonia, or of pleurisy. I observe that it is mentioned by Dr. Williams, in his lectures now in the course of publication, and his observations are generally very exact. Dr. Stokes notices, also, this vibration; but is, I apprehend, in error, when he says, that "hepatization of the lung seems to *diminish*, but not to remove it." But this thrill is not always present in the healthy state, and then we can infer nothing from its absence on the diseased side. I could discover none on either side, in Dr. Hawkins's patient, on whom paracentesis thoracis was performed yesterday. This was owing, I believe, to the feebleness and higher pitch of her natural voice. The deeper and louder the voice, the greater *ceteris paribus* will be the vibration.

I have pointed out, then, certain signs, for which we look to assist us in equivocal cases. But Coggs's case was not equivocal; there were other signs that made it plain, even if we had not known, as we did know, his history.

One of these was the situation of his heart. I have told you that the dull sound produced by striking his chest extended some way beyond the middle of the sternum, and certified that the mediastinum was pressed over in that direction; and the heart could be heard, felt, and seen to pulsate, quite on the right side of the sternum, near to, and just within the right nipple; and it could not be seen, felt, or heard, in its proper situation. This circumstance, taken in conjunction with the rest, was conclusive of the fulness of the left pleura, even to distension. The heart is, indeed, sometimes naturally placed on the right side, all the viscera being transposed; such was the case in a patient of mine who died in the hospital two years ago. But such deviations from the general rule are very rare; and the morbid signs co-existing with the displacement of the heart in Coggs, as well as his history, forbid any suspicion of its being natural.

Another conclusive circumstance was, that the left side of the thorax was found, by measurement, to be larger in circumference than the right,—nearly or quite an

inch larger. Now Dr. Stokes has ascertained that, in right-handed persons, the right side is often the larger of the two, mainly, perhaps, by reason of the greater bulk of the muscles spread upon it; and a French writer (M. Woillez) has recently put forth a similar statement as the result of numerous measurements.

That the *left* side is the largest in disease is therefore a strong circumstance, and, taken together with the other signs, quite decisive. You are to remember, that the chest may enlarge on one side under other conditions of disease—in emphysema of the lung, for instance; but then percussion and the stethoscope will furnish very different information from what they gave us in Coggs's case.

It often happens that the integuments of the distended side are œdematous; but no œdema existed in this instance.

The case, then, was clearly a case of large effusion, the result of acute pleurisy; and when the inflammation was at an end, this new phenomenon occurred. At first the patient could not lie on the left or diseased side; after the pleura became distended he could not lie on the right, or sound side. His favourite posture was *towards* his left side; not *on* that side, nor on his back, but between the two positions.

It is a circumstance also well worthy of notice, how little this state of things—this abolition of the use of one lung, and this encroachment on the space in which the other had to play, by the heart and mediastinum—interfered with our patient's respiration. Lying at rest in his bed he did not complain of, nor did he manifest, any shortness of breath or embarrassment in respiring. Probably he would, if he had got up or made exertions; but you will sometimes meet with persons who move about briskly in their ordinary way, without distress, though one pleura is full of fluid. Andral relates examples of this; and I had one here some time ago in a butcher, who could not be persuaded that any thing remained the matter with him, and insisted on going out. On the other hand, in some the dyspnoea is extreme.

On what does this difference depend? Partly, perhaps, on the suddenness or slowness of the effusion; yet that cannot be all; partly on the soundness or unsoundness of the other lung; partly, I conjecture, on the fact that some persons seem to have an excess of the power of breathing—more lung in reserve than under ordinary circumstances they want. Dr. Latham has glanced at this peculiarity in his valuable little book on Clinical Medicine.

In the present case the means em-

ployed had failed to prevent or check the effusion; and the effusion having taken place, what was next to be done?

Why, to put the patient into the state most favourable for the reabsorption of the fluid. For that purpose I kept his blood-vessels empty by low diet; I gave him diuretics, mercury till his gums were affected, blistered his side repeatedly, and rubbed upon it a combination of mercury and iodine.

But in vain. The treatment had little or no influence upon the amount of the effusion. He grew thinner and weaker; his pulse became more and more frequent, and feeble. Although he had no distinct paroxysms of hectic fever, no succession of chilliness, dry heat, and sweating, his face wore a hectic flush. All this while his breathing was tranquil.

And now was pressed upon me the question, whether my duty to the patient did not require that I should have the fluid removed by mechanical means—whether I ought not to request my colleague, Mr. Arnott, to puncture his thorax? A most grave and anxious question; and one so difficult, that even now I am fearful of leading you wrong by giving any decided opinion respecting it. But I feel bound to tell you what is passing, and has passed, through my mind on the subject; and having told you this, I must leave the matter to your own future judgment.

It has been a sort of rule with me, not to propose the operation of paracentesis thoracis in cases of pleuritic effusion, except in one or the other of the two following conditions:—1. When the effusion is increasing, and embarrassing the patient's breathing, and threatening to suffocate him, or to put a stop to the action of his heart; and, 2. When the patient is obviously losing ground from day to day, and early death, without the operation, appears inevitable, even although his respiration is not materially affected*.

Such has been my rule for these reasons:—

1. Persons who have been attacked by the disease we are now considering generally recover. It is an uncommon thing to have a patient die of *simple uncomplicated pleurisy*. The effused liquid is for the most part reabsorbed; the lung expands again, or the walls of the chest shrink in-

wards; and the ultimate state of such a patient is as good as it probably would be after a successful tapping.

2. The operation is attended with some pain (though that is trifling), always with some risk, and not seldom with considerable hazard. Not risk belonging to the operation itself, which is simple, and does not concern vital parts; but risk from its consequences. It may rekindle the inflammation; the admission of air into the pleura, which cannot, I believe, be prevented, is apt to convert the adhesive inflammation into the suppurative, or to produce decomposition of the effused liquids; and either of these possible consequences would certainly augment the danger of the patient. It is satisfactory to me to find that Dr. Stokes, in his recent most instructive work on Diseases of the Chest, expresses similar opinions. "The cases," he says, "which seem to demand the operation are those where a rapid acute effusion threatening life occurs; and next, chronic cases which resist treatment. But even in these cases, *if life is not threatened*, it seems much better not to operate, but rather give the patient the chance of the ultimate absorption of the fluid."

In our case life was threatened. Cogg was gradually but evidently sinking under the disease. He became more and more emaciated, although, after the failure of the abstinent system, I had given him tonics, and a full supply of nourishing food. His hips were beginning to get sore. I then explained to his father, that he would certainly die if the operation were not done; and that the chance of his being saved by it was small; yet it offered that small chance. And I stated the same thing, with as much plainness and delicacy as I could, to the patient himself; and I suffered them to determine. They both wished for the operation, which Mr. Arnott, as you know, performed on the 29th November, eleven weeks all but two days from the commencement of the pleurisy. A grooved needle, as recommended by Dr. Thomas Davies, was first introduced between (I think) the sixth and seventh ribs. This is to ascertain whether there is fluid within the reach of a trocar at the place of the puncture; and if so, the nature of that fluid. A collection of pus would require a larger trocar for its evacuation than a collection of thinner fluid. A serous liquid trickled out when the needle was withdrawn. Then a trocar was inserted, and upwards of five pints of a transparent but reddish liquor were let out, quite free from flakes or any puriform admixture. Air entered and escaped freely, as he breathed, during the emptying; and whenever he coughed the fluid spurted out with great force. I kept my

* Those cases of empyema are here excepted, in which a soft inelastic tumor presents itself, generally in the fore part of the chest, shewing a tendency in the confined liquid to make its own way outwards. Such a tumor should be punctured at once, for the liquid is always pus; there is no chance of its being re-absorbed; and if the swelling be left to itself, troublesome burrowing sinuses will be apt to form in the thoracic or abdominal parietes.

hand on his right side, where the heart was beating, and I observed that it did not move off in any degree to its natural place. When the fluid ceased to escape, except in minute quantity, even during the act of coughing, the opening was closed by adhesive plaster, care being first taken to expel, by desiring him to cough, as much as possible of the *air* also that had entered.

He was much troubled with cough during the operation, and immediately after it he had an opiate.

When I visited him in the evening he had had some sleep, and said he felt lighter; and seemed pleased to tell me that the moment he lay down after the operation, he found he could lie on his right side, which he was previously unable to do.

This has been noticed in other cases. What does it shew?

You must know that differences of opinion have existed as to the cause of the patient's inability, in *most* cases, (for it is not so in all,) to lie on the sound side, at an advanced period of the disorder. Some have thought that it is because the weight of the supernumerary fluid presses down the mediastinum, and so oppresses the healthier lung. Others have attributed it to the impediment which lying on the sound side offers to its own expansion: the muscles that dilate the healthy side have to lift, as it were, the weight of the body, and are, some of them, it is said, pressed upon and impeded in their action by that posture. Now the immediate effect of the removal of the fluid, in enabling the patient to lie on his right side, confirms the one of these two opinions, and completely disproves the other. The same, or very nearly the same, impediment as before to the elevation of the ribs on the healthy side would arise from lying upon that side. Doubtless the gravitation of the supernumerary fluid is the chief cause of the dyspnoea. Something may probably be due also (as Dr. Stokes suggests) to the pressure which that posture allows to be produced by the weight of the abdominal viscera upon the available portion of the diaphragm.

I was sorry to find that the heart was just where it had been before the operation. Percussion on the fore-part of the left side, which had previously given a perfectly dull sound, now gave a sound quite hollow and drum-like; much more resonant than the sound of the comparatively healthy side. The pleura evidently contained air in the place of that liquid which had occupied it in the morning.

I should have told you that on the patient's admission I could detect nothing wrong in the breathing on the right side;

it was clear, uniform—exaggerated, indeed, and puerile, as might be expected, but pure and equal every where. By degrees, however, we began to suspect that the lung contained tubercles; not only from the non-absorption of the effused fluid, but from the result of auscultation. A slight wheeze was occasionally audible at the upper part of the right lung. He coughed and expectorated somewhat; and his voice, to the ear placed upon the supra-spinal fossa, sounded unduly loud and resonant, and the respiration there was evidently tubular.

The patient remained easy, and thought himself better, for some days; but no essential improvement took place. Some remarkable modifications of the auscultatory signs, however, in consequence of the operation, it may be well to notice. The left side, as he sat up, was unnaturally resonant under percussion. There was still no vesicular breathing whatever to be heard in it. Nay, the bronchial breathing and bronchial voice, which existed between the scapula and spine, before the operation, had entirely vanished. How is this to be explained? Thus, as I believe. The bronchial breathing and voice are readily conducted to the ear laid against the side of the chest through a *solid* lung; less readily, yet still, in many cases, actually so conducted, through the *liquid* which fills the pleura and compresses the lung; but not at all, as it would seem, through *air* existing in the pleura.

You do not always, I repeat, hear it when the pleura is full of water. I could distinguish none yesterday in Dr. Hawkins's patient already referred to.

For a similar reason, there was still no vibration to be *felt* when the patient spoke.

But *those* sounds had now supervened which under the circumstances were to be expected. The whole of the fluid was not, perhaps, evacuated; some fresh fluid too, it was probable, had been poured out. I anticipated, therefore, the remarkable sounds which denote the co-existence of air and liquid in the pleural sac. Listening the instant he assumed a sitting posture, I distinctly heard, several times, a tinkle like that of a tin kettle slightly struck by a stick: I make no doubt this was caused by the dripping of liquid from the roof (as he sat) of the cavity. The voice had a strong metallic ring or echo, and so had the cough, especially just at its termination. I noticed also that a *borborygmus*, which took place in his stomach, had the same character, and seemed to the ear, placed against the side, to come from the chest. Also, when the patient jerked his body, I plainly heard the splashing of fluid in the pleura.

The circumstance of the metallic tinkling, and the ringing sound and echo heard with and after the voice or a cough, prove (what however had been proved before, by Dr. Williams, in opposition to the opinion of Laennec,) that a fistulous communication with the bronchi is not necessary to the production of such sounds, nor any communication with the external air: for the wound was closed up, and (as we afterwards saw,) no communication with the air-tubes existed. The sound acquired by the rumbling in the stomach illustrates also Dr. Williams' remark, that the metallic sounds may be conveyed from without, when the sac is closed. He states that he has known the pulsation of the heart cause a tinkling echo in pneumo-thorax.

On the 4th of December, five days from the operation, he became rather suddenly faint and sick, lost his appetite, and vomited his food: and his pulse, which had been about 120, rose to more than 130.

He rallied somewhat after this; but on the morning of the 9th he was attacked with dyspnoea—his countenance became sunk and cadaverous—some crepitation was audible in the right lung—he had “the rattles” in his throat—and at 3 o'clock, on the tenth day after the operation, he expired.

When the body was examined, the intercostal spaces were observed to be still much less distinct on the left than on the right side. One of them being punctured, there was no audible rush of air either inwards or outwards, but the gas which quietly escaped soon made itself sensible by a most disagreeable odour.

Two large oblong openings were made by removing a portion of the parietes of the thorax on each side of the sternum, which was left.

It was then seen that the mediastinum was pushed much over towards the right side, the left margin of the right lung appearing without and beyond the line of junction between the sternum and the cartilages of the ribs; and the heart lying still further over. The right lung was unadherent, but a small portion of it, as big as a French plum, at its very summit, was rendered quite solid by clustered tubercles. In various other parts of the same lung small tubercles were thinly scattered.

In the left pleural cavity, besides air, there was a considerable quantity of an opaque grey sero-purulent liquid, containing flakes and granules of lymph. The lung, with its division into lobes still evident, adhered, like a flat broad cake, to the mediastinum from one extremity to the other, and from sternum to vertebrae.

A large membraniform sheet of lymph

hung loose like a curtain at an equal distance between the ribs and the compressed lung: it was ragged and largely perforated towards the diaphragm. The inner surface of the cavity was universally smeared with pus, or puriform lymph.

The right bronchus was now firmly tied, and air was forcibly blown through the trachea. This raised and swelled out the adherent lung to a considerable extent, cracking in several places the layer of lymph spread over it; but the lung subsided again to nearly its former dimensions when the inflation was intermitted.

The adhesion of the flattened lung to the mediastinum was very firm: when it was divided by a scalpel, the cut surface appeared red, and like the section of a muscle; but it crumbled in a slight degree under pressure; and portions of it, cut off, floated upon water. No tubercles could be detected in it.

Such is the case I was desirous of bringing fully before you, as an instructive example of pleurisy and its consequences, and as a text which admitted of much practical comment. One more circumstance connected with it I may now point out—viz. that whereas, up to the time of the operation, the inflammation had been of the adhesive kind, or stage, it was afterwards converted into the suppurative. This was the effect of the admission of air into the pleura: it is a very common consequence of the access of air to an inflamed surface; we see it in the case of simple incised wounds, and under various other circumstances which I have not leisure to specify.

Did Coggs die sooner than he would have done if the operation had not been performed? I cannot tell. He could not have lived long without it. The operation was followed by no immediate ill consequence, but by some degree of comfort. Yet I think the patient's life may have been slightly abridged by it; but it afforded him a chance, and the only chance, of escape. His condition may be compared to that of a man left by a wreck on a barren rock, near an uninhabited but fertile island, surrounded by a dangerous surf; if he remain on the rock, he is infallibly destroyed by famine; if he plunge into the sea, he incurs the risk of perishing in the breakers, but he has the chance of reaching the shore, and of living.

Certainly the most important question arising out of these cases is the expediency of the operation; and if it be expedient, the time for its performance.

The advocates of the operation, even under other circumstances than those which I have already spoken of as governing my own determination, allege that if it be delayed beyond a certain period

the compressed lung becomes unable to expand, both by internal adhesion of its vesicular texture, and by the firm coating of lymph which is almost always spread over it, and too often binds it inextricably down; whereas, they say, if the liquid that presses the lung into a heap be let out *in time*, the lung will receive air and rise again, even in spite of the soft layer of lymph which may envelop it. If this be not done, and the patient should survive, he recovers (they further argue) with the loss of one lung, and subject, as long as he lives, to the inconvenience, deformity, and hazard, belonging to such a loss. The hazard is of this kind:—the other lung, called upon for a double quantity of work, will, on that account, be more than naturally prone to take on disease; and such disease occurring in it as might be harmless if the patient had both his lungs permeable and efficient, would be inevitably mortal.

If these arguments are valid (and it cannot be denied that they deserve an attentive consideration), the rules to be followed would appear to be these:—During the continuance of the inflammatory action, while there are fever and pain, and the other indications of inflammation still going on, the operation should not be done, unless suffocation, or syncope, are impending. After all signs of inflammation are over, a further short interval should be allowed, to ascertain whether the process of absorption commences. If it does, then the case should be left to nature and physis. If, on the contrary, the practitioner can satisfy himself that no diminution whatever of the effused liquid is taking place, then, while the adhesions and false membranes are yet fresh and tender, the pressure should be taken off the lung by letting out the water.

Such, I say, would seem to be the rules, supposing the advocates for an early operation are right. But I cannot venture to recommend you to adopt these rules, until more statistical evidence than we yet possess shall have been obtained of their propriety. The case now in progress, in Regent's ward, will furnish one datum towards the solution of the problem.

I imagine that after a certain space of time the lung will be quite unable to rise. How long exactly after the disease begins, it would be difficult to say. In Coggs's case, 11 weeks, all but two days, had elapsed. The effused fluid was still serous. It seems probable that the existence of tubercles in the other lung may have produced and kept up a state of the body incompatible with the absorption of the liquid.

That you may not form a more unfavourable estimate of the utility of the operation from this single case, than any sin-

gle case can warrant, I must tell you that we have had two patients in the hospital lately—one last year, one in the beginning of the present—in whom the same operation was completely successful. One of these was a patient of Dr. Wilson's, who has published the case, and a most interesting case it was. For your information I will state the heads of it, as given by him.

It occurred in a lad of nineteen. On his admission there were all the indications of copious effusion into the left pleura; the side enlarged and motionless, and dull on percussion; the intercostal spaces tense and even with the ribs; the heart beating to the right of the sternum; respiration puerile on the right side, inaudible on the left; *urgent dyspnoea*; a tendency to coma, marked by drowsiness, and blueness of the cheeks and lips: in short, the boy was on the brink of being suffocated. He had been ill about a month, and had been bled, and cupped, and brought under the influence of mercury. Dr. Wilson, with great promptitude and judgment, determined on letting out the fluid.

A grooved needle was first passed between the fifth and sixth ribs; and some serum following the puncture, a trocar was then introduced by Mr. Tuson, and nine pints of a clear fluid were drawn off. During the operation he became faintish at different times, and then the orifice of the canula was stopped for a moment by the finger. But the immediate effect of the tapping was most interesting and gratifying. While the liquid was flowing, the heart was observed gradually to move over from beneath the right mamma towards its natural situation, and his difficulty of breathing was greatly relieved. At the commencement of the operation he respired fifty times in a minute; at its conclusion thirty-eight times only. A good deal of air got in while the liquid was escaping, and for some days after the operation a splashing sound was audible on succussion of the chest, and one part of that side was unnaturally resonant, and the other part unnaturally dull, when struck, and whatever was the posture of the patient, the hollow sound was uppermost, and the dull sound was undermost; and when he sat up, or spoke, or coughed, a brazen resonance was heard by the ear applied to the scapular region. This lad got quite well, without the recurrence of a single bad symptom. He has since been here; and I understand that the left side is in a very slight degree smaller than the right.

The fluid evacuated in this case was clear and transparent. It separated, on cooling, into three parts; one of quite watery consistence, one more viscid, and a

third which constituted a soft transparent jelly-like mass of fibrin.

In this instance no injurious consequences arose from the free admission of air.

The other successful case to which I alluded happened in a patient of my own, Frederic Stevenson, thirty-seven years old. It was the more interesting, because the other lung was not healthy. He came in (last April) labouring under dyspnoea, pain in the left side, "under the heart," on coughing or taking a full breath, and inability to lie down. Three weeks before, being perfectly well, the pain seized him, and for a fortnight the orthopnoea had been present. The left side at its lower part was prominent, and one inch larger by measurement than the right. The intercostal spaces were effaced, and the sound was dull on percussion.

There were reasons, which I cannot now stop to give, for thinking that in this case the effusion was probably limited by adhesions. Four days after his admission, the pulse, which previously had been small and frequent (120), became intermittent, and rigors supervened. I was apprehensive that the heart's action might be fairly stopped, and I therefore requested Mr. Arnott to tap the chest; and fifty ounces of a lemon-coloured serum were let out, and then the wound was closed. This fluid separated also into two portions—one thinner and more serous, the other consisting of fibrinous blobs and strings.

No such separation took place in the fluid removed from Coggs' thorax. It had a distinct red tinge, but no red particles subsided on its being allowed to stand.

In Stevenson's case, the operation was done, according to his statement of dates, three weeks and four or five days after the inflammation set in. It succeeded perfectly. In two days the oppressed lung had resumed its place, for at the lower part of the left side of the chest, where no respiration was audible before the operation, large crepitation was now heard; and this gradually gave place to the healthy sound. The intercostal depressions were again visible, and the ribs moved as he breathed.

On the right side, in this man, a wheeze during expiration was heard at the upper part of the lung, and large crepitation in its lower lobe, and he expectorated mucopuriform matter, tinged with blood.

In this case, also, air freely entered and escaped during the operation, and its entrance and exit were marked by the accompaniment of the metallic sounds already spoken of.

Dr. Thomas Davies has published a tabular account of the operations of Paracentesis thoracis which he had then super-

intended. Of sixteen cases of empyema in which the operation was performed, twelve recovered; that is, the operation was successful in three-fourths of the cases—a very encouraging result. In three of the unsuccessful cases the lung could not expand after the evacuation of the fluid, in consequence of the thickness of the false membranes covering it.

The value of Dr. Davies' table would have been still greater, if it had shown in each case the time, after the beginning of the disease, at which the operation was performed; the symptoms which called for its performance; the nature of the fluid evacuated; and whether the effusion was on the right or the left side.

I have now witnessed five cases of paracentesis thoracis—two successful, and one unsuccessful, all noticed in the present lecture. A fourth occurred in a patient under Mr. Mayo's care. The operation was performed fourteen weeks after an attack of pleurisy occasioned by submersion in the Serpentine. Pus was let out in considerable quantity. The man died, exhausted, many weeks afterwards. The operation cannot therefore be called successful, for it was not followed by recovery; but it gave present relief, and prolonged apparently the patient's existence. The fifth case is still *sub judice* *. In all the five the disease was on the left side of the body.

ROYAL INSTITUTION.

Friday, Jan. 19.

Electric Induction.

THE first of the evening meetings of this useful and interesting establishment took place on Friday last, when its distinguished professor, Dr. Faraday, delighted a large assembly by an experimental illustration of the phenomena *electric induction*, and concluded by a brief statement of his new theory, which, although by himself modestly termed "a notion," until confirmed by the researches of others, will probably soon change the whole aspect of electrical science. For the full substantiation of his views, Dr. Faraday referred to his paper lately read to the Royal Society, and now in the course of being printed in its Transactions; and he now brought forward only those experiments which were most susceptible of demonstration before a public assembly. It would be doing injustice to the professor, were we to attempt to give more than

* This patient died subsequently to the delivery of the lecture. I intended to have said a word or two upon the fittest place for puncturing the chest, and upon the question of closing or keeping open the aperture; but I most not trespass any longer upon your time.

an abstract of the more prominent points of a lecture, in which experimental proof kept pace with the flowing words of the eloquent lecturer.

Electric induction is that property by which an electrified body can induce an opposite electric state in that part of other bodies to which it is approximated. Thus when a conductor charged with positive electricity is approached (but not within discharging distance) to one not electrified, the electricity of the latter, before latent, becomes developed; its negative portion being drawn towards the positive conductor, while its positive portion is repelled to the further end. A double electric state is thus induced in the uncharged conductor, without either receiving or giving any to the charged conductor, and remains so as long as the neighbouring conductor continues charged; but the moment that it is discharged or removed, the induction ceases, and the electricity returns to its balanced latent state. Now if we could divide the induced conductor into two, we should find one-half charged with positive, and the other with negative electricity; and the same result may actually be obtained by inducing electricity in two conductors in a line, with their ends in contact, and removing the further one whilst the inducing influence continues. All the common phenomena of electric excitement will be found to involve this principle of induction. Let two examples suffice. When we approach our knuckle to the charged conductor of an electrical machine, the electricity crowds towards it, being drawn there by the opposite electricity which accumulates from our body and contiguous objects into our knuckle, until we bring it within discharging distance, when the whole charge passes across the space at that point. In the Leyden phial or battery, the cumulative property of electric induction is exemplified in a higher degree. Here is an extensive conductor of tinfoil inside, communicating with the machine, separated only by the glass of the jar from an equally extensive conductor outside, in communication with the earth; the electricity excited by the machine passing into the inside of the jar, draws the opposite electricity from the earth to the outside, and the extent of the electrified surfaces and closeness of their approach give to the inductive attraction its greatest force; so that two enormous quantities of opposite electricities become heaped up close to each other, ready to discharge and equalise themselves whenever a conducting medium is presented to them. Dr. Faraday showed that where circumstances prevented induction, there was no development of electricity. Thus the interior of an electrified coal-seattle

showed no evidence of electricity, but its exterior being in inductive relation with surrounding bodies, was obviously electric.

Philosophers and mathematicians have generally sought to explain or calculate electric phenomena or effects under the hypothesis, that they are the result of attractive and repulsive forces, acting in straight lines, inversely as the square of the distance. The profound calculations of *Æpinus* and *Poisson* were founded on this notion. They considered that induction is the same law acting on distant particles, the intervening non conductor being wholly passive in regard to the electric force. Professor Faraday sought to bring this view to an *experimentum crucis*. If the forces are exerted only in right lines, electricity should not be induced in curves or round a corner. By several ingenious experiments, however, he proved that opposite electric states may be induced round a corner, and without any limitation to the law of right lines. This and other facts have brought him to the conviction, that electric induction does not depend on attractions and repulsions exerted between separated particles, but on a particular polarised state which the particles of non-conductors are capable of assuming, and which acts contiguously from particle to particle. We look with great interest to the further development of these views.

The learned professor, at the conclusion of his lecture, congratulated the members on the flourishing state of the Institution, which he added now received them with a *clean face*. He announced that a lady had just placed a thousand pounds to the credit of the Institution, the interest of which is to be applied once in seven years to a prize for the best work *On* (as we understood) *the Evidence of Design in the Works of Creation*.

METEOROLOGICAL JOURNAL.

Jan.	THERMOMETER.		BAROMETER.	
Thursday . 18	from 15	5 to 25	30.08	to 29.84
Friday . . 19	21	3	29.72	29.74
Saturday . 20	— 5	19	29.81	29.88
Sunday . . 21	5	33	29.85	29.63
Monday . . 22	29	42	29.62	29.63
Tuesday . . 23	24	37	29.68	29.61
Wednesday 24	27	19	29.64	29.68

Wind, N.E. and S.E.

Except the morning of the 18th, and afternoons of the 18th, 20th, and 22d, generally cloudy. Snow fell on the 18th and 19th.

Severity of the Frost.—In our last weekly journal we had to call the attention of the reader to the extreme degree of cold indicated by the thermometer on the morning of Monday the 18th: on the same day that this fact was published in the *MEDICAL GAZETTE*, viz. Saturday, the 20th, we had to record a degree of temperature still lower by six, viz. *five degrees below Zero!* being within 13 degree of the lowest ever, I believe, recorded in Great Britain.

CHARLES HENRY ADAMS.

WILSON & SON, Printers, 57, Skinner-st., London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF
Medicine and the Collateral Sciences.

SATURDAY, FEBRUARY 3, 1838.

LECTURES

ON

FORENSIC MEDICINE;

Delivered at the Aldersgate School of Medicine,

By SOUTHWOOD SMITH, M.D.

Physician to the London Fever Hospital.

LECTURE VII.

Apparent Death—Errors as to the fact of Death—Signs of real Death—Cessation of the Circulation and of the Respiration—Uncertain signs—Certain signs: Rigidity, Putrefaction, Loss of Muscular Contractility—Causes of Death: External Violence; Natural Disease—Diseases of the Nervous System as the Causes of Death—Diseases of the Structures of the Cranium—Diseases of the Dura Mater: Idiopathic; connected with Affections of the Ear—Gorged condition of the Blood-vessels of the Dura Mater.

Apparent death.—Suppose you are called to a case of sudden death, and required to investigate the cause of it. Your first object, of course, must be to ascertain whether the death be real or only apparent. Do not, without examination, assume that death has taken place; do not hastily and positively affirm it. You will be placed in an awkward position if, after a positive declaration that the death is real, the person should at last recover.

On a superficial view there may be every appearance of death, and yet it may be merely a case of suspended animation; or a state of the system produced by disease closely resembling that of death. Syncope, apoplexy, catalepsy, concussion, even intoxication, may so perfectly simulate death, that it may be quite impossible

to determine the true nature of the case without close observation.

Errors as to the fact of death.—Nor are the signs of real death by any means so manifest and certain in all cases, as to render attention unnecessary. Authentic cases are on record, which indisputably prove that error as to the fact of death has been committed. Without doubt, in proportion to the progress of medical science and the improvement of medical education, the number of such errors has greatly diminished; and if it be right to judge of the state of medical knowledge in Europe a century ago by a test derived from the correctness of the opinion pronounced on this subject, the state of the science at that period must be deemed even more imperfect than it is commonly supposed to have been. Bruhier, in his treatise on the Uncertainty of the Signs of Death, published in 1740, records 181 cases in which apparent was mistaken for real death. Out of this number of persons, really alive, but supposed to be dead, it is stated that fifty-two were buried alive; that four were opened before death; that fifty-three spontaneously recovered after having been laid out in their grave clothes; and that seventy-two were erroneously reported as dead. It is impossible not to suspect that this statement is exaggerated; but Zacchias, Lancisi, Philippe Peu, Guillaume Fabri, Pechlin, Kirchmann, Kornemann, Winslow, Falconet, Rigodeaux, all cite examples of the commission of the like error. In the present state of medical science this mistake is not very likely to be made; yet the apprehension of its occurrence is by no means entirely removed from the public mind. A friend of my own, a medical man, laboured for years under such a terror of it, that he bound me by a solemn promise, if I should survive him, to take effectual care to prevent his being buried alive, by making such a dissection of his body as should be incompa-

tible with the continuance of life. He died several years ago, and I performed my promise.

Signs of real death.—There are positive and unequivocal signs of real death; and no well informed medical practitioner of the present day can possibly confound this event with the state of the system in apparent death, except from culpable carelessness. I cannot suppose that you will ever experience any real difficulty in arriving at a correct conclusion on the matter, because your instructed and experienced eye will commonly teach you the truth instantaneously and irresistibly; the conviction of which will be derived from a number of sources, which it might take you some time to discriminate. Still you ought to be familiar with the most certain signs of death, and to know where to look instantly and steadily for evidence of the event, in case any doubt should arise in your own minds, or there should be doubt in the minds of others.

The first event to which you should look is the state of the circulation. If there be complete and protracted cessation of the pulse of the heart and arteries, that will afford one evidence of the fact of death,—not, indeed, sufficient, taken alone, but unequivocal and certain if it be connected with another event, the existence of which you should next endeavour to ascertain, namely, the cessation of respiration. We know from direct experiment that the respiration cannot be perfectly and continuously suspended for the space of four minutes, without producing the complete and irrevocable extinction of life; at least so complete and irrevocable, that reanimation can be effected by no means yet discovered. But respiration may appear to be suspended when it is only exceedingly slow and faint, decarbonizing just enough blood to prevent the extinction of the irritability of the heart, but not sufficient to give it any sensible action.

Whenever you are in doubt about this, uncover the thorax and abdomen, and observe intently and for a long time continuously, whether any respiratory movements take place. Accustom your eye to judge accurately of these movements, which are highly characteristic, and to the experienced eye sufficiently obvious, when they may be overlooked by ordinary observers. Certain it is, that no faith whatever can be placed in the methods vulgarly recommended for ascertaining whether the respiration has ceased—namely, by applying a mirror to the mouth, or a downy feather to the nostrils. If the mirror be warmer than the expired breath, no sign can be obtained from it, because the breath will not be condensed upon it; while the mirror may become sullied by the insensi-

ble perspiration of the hand that holds it. As to the feather—

——— “By his gates of breath
There lies a downy feather, which stirs not.
Did he suspire, that light and weightless down
Perchance must move.”

But the Prince, when, trusting to this token, he bore the crown from the pillow of his royal father, was deceived by it; and so will you be, if ever you place the slightest confidence in it.

The certain signs of death.—If in any extraordinary case circumstances should occur to excite doubts in your own minds, or if you should be unable by the ordinary indications of death to remove the doubts of others, then you must wait until you are able to obtain one or more of the certain signs of death. The certain signs of death may be included in three—namely, rigidity, putrefaction, and loss of muscular contractility.

Rigidity.—Rigidity is that general stiffness of the body which comes on in a given time after the extinction of life. Doubts have been entertained whether this phenomenon be an invariable consequent of death; but Louis has set this matter at rest; for with the view of determining it, he made careful observation, in the hospital to which he was attached, on 500 cases of death, and never in a single instance found it absent. Subsequently it has been observed to be constantly present in a wide range of the animal scale—in all the mammalia, in birds, reptiles, fishes, mollusca, crustacea, and insects.

The seat of the general rigidity which follows death is in the muscles, for on the dissection of a limb in which it is fully developed, if the common integuments, aponeuroses, ligaments, and synovial membranes be removed, the stiffness remains; but, on the contrary, if the muscles are taken away, the other tissues remaining entire, the limb becomes perfectly flexible.

Though the rigidity of death always comes on in a definite time after the extinction of life, yet the period varies according to the state of the system, the nature of the death which has proved mortal, the mode in which it has extinguished life, and the condition of the atmosphere around the dead body. In general it appears early, in proportion to the feebleness of the muscles, and the general weakness of the system; hence it comes on rapidly in persons who die of extreme old age—of protracted fevers of the typhoid type—of chronic diseases which greatly impair the nutritive functions, and exhaust the powers of life, as consumption, marasmus, scrofula, &c. On the other hand, it appears much later when death takes place rapidly in the

athletic, in a vigorous state of the system; in accidental death from external injury, as from wounds; in concussion; in the different forms of asphyxia, and more especially in that which is induced by the irrespirable gases. In this latter case it appears so tardily, that Bichat, not continuing his observation long enough, concluded that the phenomenon is not constant and invariable.

The rigidity appears first in the muscles of the trunk and neck; it extends thence to the muscles of the lower, and last of all to those of the upper extremities. It disappears in the same order. The sooner it comes on the shorter is the period of its duration; and conversely, the slower it is to appear, the longer it lasts. It disappears the most rapidly in summer, in warm and damp air, and lasts longest in winter in a cold and dry air. Its mean duration is from twenty-four to thirty-six hours; but in a case of asphyxia, in which it did not come on until sixteen hours after death, Nysten observed it to last for the space of seven days. It is often fully established before the extinction of animal heat; and the most complete paralysis of a limb does not prevent its occurrence.

The limbs, and even the trunk of the body, may become stiff, during life, from several causes; but these are so manifest that I do not suppose you can possibly be in danger of overlooking them, or of confounding the stiffness from the spastic vital contractility of the muscles in syncope, catalepsy, epilepsy, or hysteria, with the rigidity that follows death.

Putrefaction.—Of course if any degree of putrefaction be present, indicated by a blue, green, dark brown, or blackish colour of the integuments, by a softening of the tissues, and the exhalation of the peculiar odour distinctive of the putrefactive process, there can be no question of the certainty of death.

Loss of muscular contractility.—In certain extraordinary cases, in which the cessation of the heart's action, the suspension of respiration, the extinction of animal heat, the general rigidity of the body, and the commencement of the process of putrefaction, are all doubtful, it has been proposed to expose a muscle by a small incision, on a part of a limb where such a wound can do no serious injury, and then either to irritate the muscle mechanically by pricking it with any sharp-pointed body, or to apply to it the stimulus of electricity or galvanism. It is ascertained by numerous and decisive experiments that the muscles retain the power of contractility for a certain time after death. If the stimulus of electricity or galvanism be applied to a muscle a few minutes after death, it causes it to contract with

extreme violence. The forearm of a man who had been executed was bent on the arm; an electric discharge was made on the extensor muscles; several men who held the limb in a bent position were knocked down by the force of the muscular contraction, which extended the limb. From numerous experiments performed both by English and French physiologists, with a view to determine the duration of this power, it would appear that under different circumstances it may be extinguished at any period from two to thirty hours after death. Although, then, the continuance of the contractility of the muscles, on the application of stimuli, be not a certain sign of life, yet the loss of that power is a certain sign of death.

The period immediately subsequent to death may, then, be divided into four epochs. In the first the animal heat remains, and the body continues flexible. In the second the body is rigid, the animal heat sometimes remaining and sometimes not. In the third the rigidity has passed away, the body again becomes flexible, and the animal heat is always extinct. In the fourth, putrefaction commences. The duration of the first period varies under different circumstances from half an hour to twenty hours. The duration of the second period occasionally is only from two to four hours, commonly it lasts from forty-eight to seventy-two hours, and it may be protracted as long as seven days. The third period, in winter, may last from five to eight days; and a case is on record (one of asphyxia from charcoal) in which an interval of fifteen days elapsed before indications of putrefaction were manifest.

You may be required to decide on the certainty of death during any one of these four periods. In the first it may be impossible to ascertain the fact without evidence that the power of muscular contractility is lost, which evidence you cannot obtain unless you lay bare a muscle, and apply to it some powerful stimulus, which, of course, you would not do without a cogent reason. You must therefore pronounce no positive opinion until the second period come on, in which the general stiffening of the body will place the matter beyond doubt. In the third period, when the rigidity has passed away, you will be enabled to pronounce an equally decided opinion, if, together with the coldness of death, there be an extinction of muscular irritability; and in the fourth period, the presence of any degree of putrefaction will place the matter beyond all possibility of doubt.

Causes of death.—Suppose, then, the death to be real. The body has been found in some unusual situation. No history of the case

can be obtained. Or suppose that a person has died suddenly, under circumstances which have excited a suspicion of poisoning. No one is at hand able and willing to give a true account of the circumstances which preceded and accompanied the fatal event. You are called upon to ascertain the real cause of death; you are required to state the evidence on which you ground the opinion you deliver. The only sources from which, under such circumstances, you can derive the information which will enable you to form a decision, are the appearances on the exterior of the body, and the condition of its internal organs.

External violence.—The appearances on the exterior of the body will, in general, enable you to decide whether or not death has been produced by external violence. The indications of external violence of such a kind and degree as may prove mortal, I shall particularly describe when I treat of death occasioned by wounds, bruises, and so on.

Natural disease.—At present I suppose the death in question to be produced by some natural disease having its seat in the interior of the body. When natural disease is the cause of death, where are you to look for evidence of that fact? In the brain, in the spinal cord, in the heart, in the large vessels, in the lungs, in the stomach, and in the intestines. The nervous, the circulating, the respiratory, and the nutritive systems—not one of the organs belonging to either of these systems, but may be the seat of a malady that may prove rapidly mortal. When it does so, how is it to be known? By the lesion of the organ that is the seat of the malady. But does that lesion always exist? Unquestionably always. Is it always manifest after death? For the most part it is. Is the appearance uniform? With few and rare exceptions it is quite uniform. When discovered may it form the basis of a sure and positive decision? In general it may. Is it easily overlooked? Sometimes it is very easily overlooked. Easily mistaken? Yes. Can it be to any one the basis of a sound decision, who does not study it? Never. Can it mislead any one who does study it? Never.

What the knowledge of structure, and the relation of vessel to vessel, and organ to organ, is to the surgeon, that the knowledge of the pathology of natural and more especially of sudden death is to the medical witness. Without this knowledge he must almost always be proceeding in the dark; with it, he has a clear and steady light always shining upon his path; without it, he has nothing certain from which to reason and to which to refer; with it, he has a standard of comparison to which he can bring almost every case upon which

he is called to decide. If you do not know what the diseases are which produce natural, and more especially very rapid death—if you do not know the morbid changes which indicate the presence and operation of such diseases, you cannot possibly distinguish between these and the effects of poison. Before all other things, then, you must study those organic changes which indicate the existence and operation of rapidly fatal but natural diseases.

Diseases of the nervous system as the causes of death.—On the diseases of this class which have their seat in the nervous system, the labours of modern pathologists have shed so clear a light, that it is scarcely possible for any one who avails himself of the knowledge already accumulated, to fall into essential error respecting these important maladies—most of them capable of proving rapidly mortal, all of them, until comparatively a recent period, involved in profound obscurity, very imperfectly distinguished from each other, and hardly more satisfactorily distinguished from the effects of poisons and even of external violence. Independently of their value to you as medical practitioners in the daily exercise of your profession, the pathological facts disclosed and discriminated in the works of Abercrombie, Alison, Brodie, Craigie, Cheyne, Hooper, and Bright, in our own country; and of Laennec, Foville, Lallemand, Magendie, Rostan, Rocheaux, and many others on the Continent, are capable of rendering you inestimable service in the study of forensic medicine—an application of the fruits of their labour which these honoured cultivators of our science could scarcely have had in view. On the present occasion, I cannot bring out of these treasures the riches they contain. I can do little more than direct your attention to them, and show you how you may appropriate them, rather by teaching you the mode in which they were obtained, and in which you must look at and study them, and imitate the labour that produced them, than by attempting to put you in full possession of them. As the precious materials they contain were drawn with much skill and unwearying exertion from the great mine of nature, so in applying them to your own particular purpose, you must work them up with something of the like skill and labour before you can derive real advantage from them.

Diseases of the structures of the cranium.—When about to examine for a judicial purpose the condition of the great central masses of the nervous system, begin by inspecting minutely the appearances about the head, and the state of the external scalp. Blows, lacerations, or wounds, either recently inflicted or of long stand-

ing, may have left external traces of their existence and operation, which may at once disclose to you the condition of the internal organs, and decide the object of your inquiry.

On the other hand you may find, as the result of natural disease in the integuments of the head, and in the muscular and tendinous tissues, inflammation, phlegmonous, erysipelatous, rheumatic, or syphilitic; you may find, intimately connected with the morbid condition of these structures, the pericranium inflamed, thickened, and separated from the bone; the bone itself dead and exfoliating, and extensive deposits of bony matter on the external surface of the skull. It is rare that these changes take place in the structures on the exterior of the head, without being accompanied with corresponding changes in the organs contained within the skull, in the dura mater, in the other investing membranes of the brain, and in the brain itself. You may, therefore, in general, infer with tolerable certainty, the existence of disease of a given character, in the internal organs, from the presence of diseases in the external tissues.

You may find, as the result of natural disease, the bones of the skull preternaturally enlarged, as in hydrocephalus. The extent to which this enlargement sometimes takes place, you see strikingly exemplified in the specimens and drawings before you. On the contrary, you may find the very opposite state, insufficient evolution, from the too early closing of the sutures, the result of which must necessarily be pressure upon the brain, just as when there is too rapid an increase of the cerebral substance. You may find imperfect ossification, either from deficient nutrition, or from preternatural distension of the bony parietes by the accumulation of fluid within the cranium; and the deposition of bony matter may be so imperfect, over a large surface, that the membranes of the brain, and the brain itself, may protrude, forming the congenital hernia cerebri.

You may find, as the result of natural disease, the skull preternaturally thin, when fluid has accumulated to a great extent within the cranium, as in chronic hydrocephalus; when partial or circumscribed accumulations of serum have taken place; when tumors or cysts have grown from the membranes or surface of the brain; when the glandular bodies around the longitudinal sinus are preternaturally large and abundant; and above all, when there has been excessive nutrition and consequent inordinate growth of the brain. If the thinness of the skull be confined to particular spots, beneath those spots you will commonly find some cause of pres-

sure; if the thinness be general or very extensive, you may infer that a general pressure has been applied, either from an excessive development of the brain or from the progressive effusion of fluid within the cranium. Pressure, from any of these causes, in proportion to the extent of it, must of course disturb the functions of the brain; and when it reaches a certain point, must inevitably extinguish life. Instead of deficient, you may find superabundant depositions of bony matter, which may take place to such an extent that the cancellated structure of the diploë may be entirely obliterated, and the structure converted into a substance resembling ivory rather than bone. You may be pretty sure that a cranium thus diseased does not enclose a sound brain. You will commonly find associated with this condition of the bony parietes of the skull, the morbid changes hereafter to be described as peculiarly connected with epilepsy, in fatal cases of which malady the skull is not unusually hard, dense, heavy, and three times its natural thickness.

Instead of a general thickening and induration of the bony structure, you may find irregular deposits of bony matter, occupying different situations and varying in extent, perhaps from minute spicular to masses so large that they greatly diminish the capacity of the skull, and even encroach upon the natural outlets, lessening and nearly blocking up the foramina, through which nerves and vessels pass. Of this morbid condition of the cranium, I show you striking examples in these specimens and drawings. Such preternatural and extensive depositions of bony matter must necessarily produce irritation, and perhaps pressure, and may cause death, either by apoplexy or by epilepsy, most commonly by the latter form of cerebral disease.

Thus even the solid case which encloses the brain, may be the seat of diseases of sufficient magnitude to occasion death; but these diseases are commonly connected with still more formidable maladies in the internal organs.

Diseases of the dura mater.—At particular points, or over large portions of its surface, the dura mater may be preternaturally adherent to the skull,—a circumstance very constantly associated with an inflammatory condition of the membrane and of the other coverings of the brain. On the contrary, a portion of it may be completely separated from the skull. The interspace may be occupied by coagula of blood, by coagulated lymph, or by pus. This morbid condition of the dura mater may be purely the result of natural disease, the consequence of inflammation commencing in this membrane as a primary

and idiopathic affection. Inflammation of the dura mater as an idiopathic affection is certainly rare, but still you may meet with it. In a case recorded by Dr. Abercrombie, a young lady was suddenly seized with severe pain in the left temple; to this succeeded a swelling of the left upper eyelid; her look became oppressed, and her pulse irregular, varying from 96 to 120. She sometimes complained of a good deal of headache, but at other times the pain was entirely absent. Shortly after the appearance of the swelling in the eyelid, she began to have severe shiverings, followed by heat of skin and profuse perspirations, and, in two or three days afterwards, she was seized with convulsions, followed the next day by increased headache, a more oppressed look, and an excessively irregular pulse, which sometimes little above the natural standard, at other times was exceedingly rapid. On the day following the attack of convulsions there was little change, but at midnight she became incoherent, and from this time sinking rapidly into a state of extreme prostration, she died in less than three hours after the appearance of the incoherence. On inspection of the body after death, there was found between the bone and the dura mater a good deal of purulent matter. The space in which this matter had been contained was defined by an irregular elevated margin of adventitious membrane, by which the dura mater had adhered to the bone, the included space being about the size of a crown-piece; it was on the anterior part of the right hemisphere. The dura mater included within this space was depressed; its surface was in some places ulcerated, and in others black; but the membrane was quite entire, and the bone was sound. On raising the dura mater, the inner surface of this portion had the same irregular ulcerated appearance as the outer surface, and when held up to the light, the membrane at the part appeared to be in some places considerably thickened, in others very thin. The right hemisphere of the brain, over all that part of it which is usually exposed in the ordinary way of opening the head, was covered by a thin uniform layer of very thick purulent matter, spread over it with great equality; and this being removed, an extensive stratum of adventitious membrane was found under the arachnoid. It was irregular in thickness, being most remarkable on the anterior part of the hemisphere, and disappearing on the posterior part. It followed the course of the arachnoid, covering the openings of the convolutions, but not dipping between them. The pia mater betwixt the convolutions was highly vascular, but without any deposition.

In this remarkable case the inflammation of the dura mater appears to have been the primary disease, though it was afterwards complicated with extensive inflammation of the arachnoid. But in general the separation of the dura mater, and the effusion of blood, lymph, or pus, between this membrane and the skull, is the effect of external violence. Whenever you meet with this condition of the dura mater, search vigilantly for proof of violence, if the evidence of it should not be apparent. It is often the direct result of fractures, and the secondary effect of blows and injuries inflicted either recently or at some remote period; and sometimes the remoteness of the period is exceedingly great.

But though inflammation of the dura mater, as an idiopathic disease, be rare, yet it is very frequently met with in a different form, in connexion with affections of the ear and the petrous portions of the temporal bone. Caries takes place in this portion of bone, and the dura mater corresponding to it becomes detached, thickened, spongy, or ulcerated. Sometimes the disease of the bone is confined to a small spot; at other times it is very extensive. Between the dura mater and the arachnoid there is a deposition of purulent matter, or of false membrane: this deposition sometimes extends along the tentorium, and purulent matter is also found in the cells of the petrous portion, in the canals of the ear, and in the cavities of the tympanum; and sometimes it extends into the cells of the mastoid process. Oftentimes this inflammation in the bones of the skull and the membranes of the brain, seems to develop latent diseases, and leads to the formation of scrofulous tubercles, or fungoid tumors, according to the constitutional predisposition of the individual. Occasionally a large collection of pus forms under the pericranium, behind the ear, extensively denuding the bone; and while the pericranium is detached on the external surface of the skull, the dura mater is equally detached on the internal surface.

These affections often attack most insidiously; they are highly dangerous in their nature, frightfully rapid in their progress, and constantly surprise the patient with death, when he is thought to be affected merely with common ear-ache. In these cases the discharge from the ear, not relieving the pain as it commonly does, but, on the contrary, the pain increasing in violence, the patient becomes oppressed and drowsy; the drowsiness is rapidly followed by delirium, the delirium by coma, and coma by death. In other cases, a purulent discharge from the ear having subsisted for some time, it suddenly stops;

the cessation of the discharge is followed by pain the ear; this by languor and drowsiness, which in a few days terminate in coma and death. A gentleman, about 70 years of age, complained of dull uneasiness in one ear. The seat of the pain he defined by planting the points of his fingers round the ear, so as to include a space of about three inches in diameter. There was no constitutional disturbance, and not the slightest danger was apprehended: one morning he was found in a state of perfect coma, and died in the afternoon.

A young man who had been liable to suppuration of the ear and deafness, was seized suddenly with deep-seated pain in the right ear, without fever. After a violent paroxysm of pain he sunk rapidly, and died within the space of a few hours.

On examination of the head, the petrous portion of the temporal bone was found black and carious; the dura mater corresponding to it was black, sloughy, and separated from the bone. Under the dura mater was a collection of pus and coagulable lymph, amounting to several ounces, which covered the whole superior surface of the right hemisphere.

A boy, aged 14, who had been affected for two months with headache and discharge of matter from the right ear, was one day attacked with increased pain, accompanied by giddiness and vomiting. Suddenly convulsions supervened, in a paroxysm of which he died. Inspection disclosed extensive caries of the pars petrosa, effusion into the ventricles, and abscess both in the cerebrum and cerebellum.

Occasionally, but much less frequently, inflammation of the lining membrane of the cavities of the nose, of the frontal sinuses, and of the ethmoid cells, seems to communicate disease to the brain, and proves rapidly fatal. Mr. John Pearson records the case of a child who was attacked one day with a slight erysipelatous affection of the nose; the next day he was seized with delirium, and the day following with coma, in which he died. Dr. Bright mentions a case in which, after great discharge from the nose, cerebral symptoms supervened, followed rapidly by death. Suppuration had taken place in the frontal sinuses, and a cyst full of muco-purulent fluid was discovered in one of the anterior lobes of the cerebrum.

The deposition of blood, lymph, or pus, between the dura mater and the skull, as the result of natural disease, may produce death, in consequence of the inflammation which has terminated in this effusion; but in proportion to the quantity of blood effused in this situation, pressure must necessarily be made upon the brain beneath; whence this effusion may occa-

sion death, with all the symptoms, hereafter to be described, of pressure upon the brain, and in the mode in which pressure ordinarily proves fatal.

Gorged condition of the blood-vessels of the dura mater.—But without any effusion, and without any other apparent disease, you may find the vessels of the dura mater preternaturally gorged with blood. This engorgement may take place to such an extent, that on the removal of the calvaria blood may issue from every divided vessel that united the two surfaces. The vessels may be not only turgid with blood, but they may be actually enlarged. Both conditions of the blood-vessels may be purely the result of natural disease—of long-continued inflammation of the substance of the brain for example, or of long-continued obstruction to the natural course of the circulation; conditions which occur in many cases of epilepsy, and in many of apoplexy. Whenever, then, you find the blood-vessels of the dura mater permanently enlarged and turgid with blood, search diligently for the cause of this state of congestion or inflammation. You may find it connected with congestion or inflammation of the brain itself as its primary seat, or you may find it connected with primary disease of some other and perhaps some very distant organ. You may find it connected, more especially, with a state of great congestion in the lungs, as in acute bronchitis, or with a preternatural dilatation of the chambers of the heart, or with a thickening of its valves, or with some other cause which has materially and permanently obstructed the natural course of the blood. But while this condition of the blood-vessels of the dura mater may thus be purely the result of natural disease, yet it is very important to bear in mind that it may also be the consequence of violence. It is sometimes found in death from submersion, as will be fully shown hereafter; and it is often the result of the mechanical obstruction to the return of the blood from the head, by the pressure of the cord in suspension. In this latter case it is not uncommon to observe, on raising the calvaria, both the surface of the dura mater and that of the bones of the calvaria mottled with drops of fluid blood from the torn vessels.

All the morbid appearances which have now been described, in as far as they are the result of natural disease, commonly give abundant indications of their presence during life, and sufficiently prepare the medical attendant and the surrounding friends for the fatal event. But some of them attack most insidiously, and all of them are capable of proving rapidly fatal without having excited alarm, and under circumstances which induce a strong sus-

picion of poisoning, or of the intentional infliction of some sort of violence. The study of the pathological appearances induced by these causes of death, as natural diseases, is therefore highly important with reference to forensic medicine, since it will enable you at once to determine the true nature of the case, and consequently to put an end to vague conjecture, and to remove groundless suspicion.

LECTURES
ON THE
ENTOZOA,
OR
INTERNAL PARASITES OF THE
HUMAN BODY,

Being part of a Course on Morbid Anatomy,
delivered at the London Hospital,

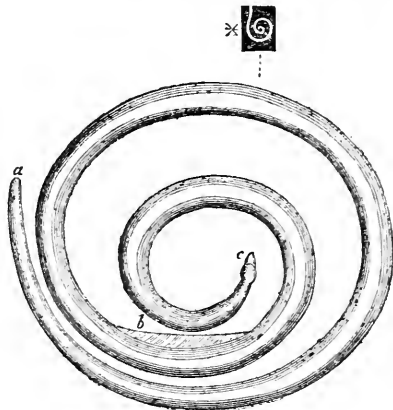
By T. B. CURLING, Esq.

LECTURE IV.

14. *Spiroptera hominis*.

THIS name is applied to a species of worm several of which were voided from the urinary bladder of a female, whose case is described by Mr. Lawrence in the second volume of the Medico-Chirurgical Transactions. A poor woman, living in the parish workhouse of St. Sepulchre, London, had been subject, since she was 24 years of age, to attacks of retentation of urine, accompanied with uneasiness in the bladder, and pains down the thighs. The employment of the catheter was attended with the discharge of large numbers of a vermiform substance, and small granular bodies regarded as ova. In the course of three or four months she passed about 1000 of these bodies, and has since, at various intervals, voided them in great abundance. The worm-like substances were of two kinds; the larger measured from four to six, and even eight inches in length, were attenuated at their extremities, contracted in the middle, and of a triangular form. They were composed of a solid parenchymatous substance without any trace of alimentary or generative organs, and they possessed so little the appearance of organized bodies, that Rudolphi and Bremser were inclined to regard them as portions of coagulable lymph moulded into a cylindrical form; an opinion, however, at variance with the fact that one of the angles or ridges constantly presented a beautiful crenated appearance. Their real nature, therefore, must be regarded as doubtful. But the animals referred by Rudolphi to the genus *Spiroptera* were some small worms transmitted to that naturalist in a separate phial, but

which were voided at the same time with the ova and larger parenchymatous bodies above described. They were of different sexes; slender, white, and highly elastic; the male measuring eight lines in length, the female ten. The head was furnished with small papillæ and a round mouth, and the body attenuated at both extremities. The tail of the male, more slender



Spiroptera hominis.

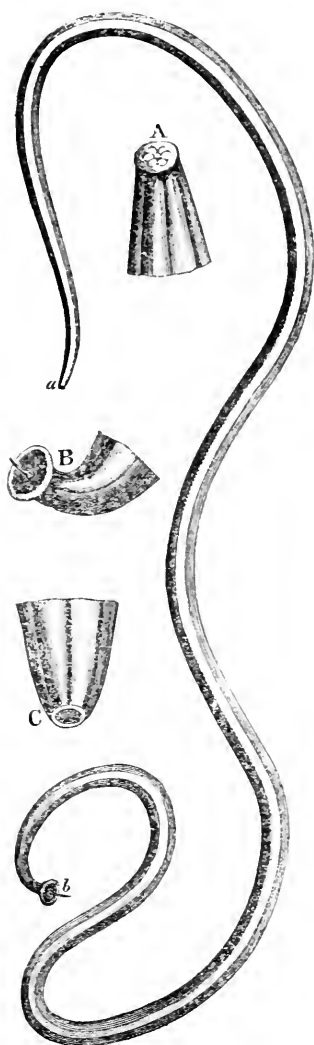
- a, Head.
b, Dermal aliform membrane.
c, Preputial sheath.

than that of the female, emitted from its apex a small tube, probably the sheath of the penis. A dermal aliform production near the caudal extremity determined the reference of this entozoon to the genus *Spiroptera*.

The escape of the worm in this anomalous case was aided by the use of turpentine, both by the mouth, and injected into the bladder.

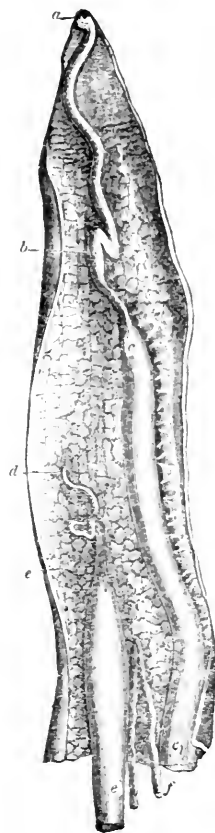
15. *Strongylus gigas*.

(from στρογγυλος, cylindrical). This is a dark-coloured worm inhabiting the kidney. As its name implies, it is of large size, sometimes attaining the length of three feet, and the thickness of the little finger. In the Hunterian collection at the College of Surgeons there is a specimen nearly of this magnitude which was found coiled up in the capsule of the left kidney, its glandular structure being completely atrophied. The male *strongylus* is less in size than the female, and is slightly attenuated at both extremities. The head is obtuse, the mouth round, and encircled by six hemispherical papillæ. The oesophagus is round, and dilates at about two inches from the mouth into the intestinal canal, a square and straight tube terminating in the anus. The body is slightly im-

*Strongylus gigas, male.*

- a*, Head.
A, Mouth and papillæ.
B, Dilated pouch and penis.
C, Anal extremity in the female.

pressed with circular striæ, and with two longitudinal impressions. The tail is incurved in the male, and terminates by a dilated pouch, from the base of which the single penis projects. In the female the caudal extremity is less attenuated and straighter, with the anus a little below the apex: the vulva is situated at a short distance from the anterior extremity, and communicates with a slender cylindrical

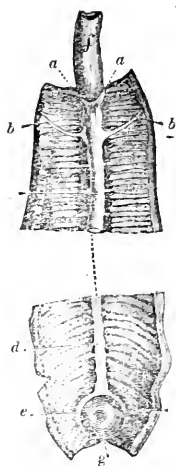


Anterior extremity of the Strongylus gigas, showing the commencement of the digestive, and the termination of the generative tube.

- a*, Mouth.—*b*, Œsophagus.—*c*, Intestinal canal.
d, Vagina.—*e*, Uterus.—*f*, Ovary.

tube or vagina, about an inch in length, which dilates into the uterus, a canal about three inches long, terminating in the ovary, which is simple, and three or four times the length of the body. A nervous system can be easily and distinctly demonstrated in the *strongylus*. It consists of two slender nervous rings, one encircling the gullet, the other the anus, connected by a single cord running in a straight line along the middle of the ventral aspect.

From the few cases on record, it would seem that the *Strongylus*, both whilst in the kidney, and after it has passed into the bladder, occasions considerable distress and disorder in the urinary organs.—The *Strongylus gigas* is met with in the wolf, dog, horse, bull, and other animals, as well as in man.

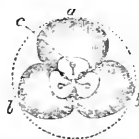


f, Esophagus.
a a, Nervous cord surrounding the gullet.
b b, Slender branches coming off from the abdominal nerve.
g, Anus.
c c, Nervous cord surrounding the anus.

Commencement and termination of the nervous system in *Strongylus gigas*.

16. *Ascaris lumbricoides*, *Lumbricus teres*, or Common round worm.

This worm, which is one of the most common infesting man, varies in length from about five or six inches to a foot, its thickness being about two lines. The body is round and elastic, and tapers towards both extremities. At the apex of the anterior is a triangular-shaped mouth, surrounded with three papillæ or tubercles, which characterize the genus. The pos-



Head and mouth of *Ascaris lumbricoides*.

a, b b, Papillæ.
c, Oral aperture.

terior extremity, at the apex of which a small black spot may frequently be observed, terminates in the male more acutely than in the female, and is also abruptly curved. The anus is situated, in both sexes, close to the extremity of the tail, in form like a transverse fissure. The tegumentary covering of the *Ascaris lumbricoides* is smooth, unctuous, and transparent, and is transversely furrowed with numerous fine striæ. The body is likewise marked with four longitudinal equidistant lines extending from the head to the tail: two lateral, and larger than the others, widen as they descend, but diminish again towards the caudal extremity. They are supposed by Cloquet, who has written an elaborate monograph on this particular worm, to



Female *Ascaris lumbricoides*.

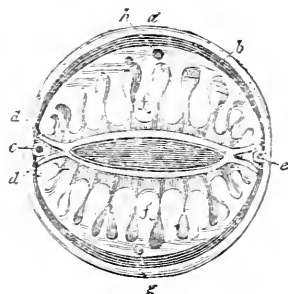
a, Head. b, Anus. d, Vulva.

denote the situation of the principal vessels of the body. The dorsal and abdominal longitudinal lines are less distinct than the preceding, and correspond to two fine cords running beneath the tegumentary covering, and connected, anteriorly, to a slender white ring, encircling the gullet,



*Digestive and generative tubes of the male
A. lumbricoides.*

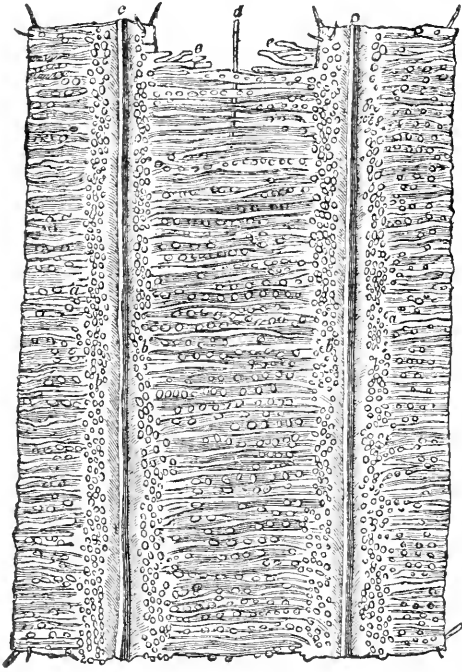
the whole constituting the nervous system of the ascaris. In the female the abdominal nerve, as it descends, forms a circle around the vulva. The muscular system is composed of two sets of fibres intimately connected with the integument; one set circular or transverse; the other longitudinal. The longitudinal are attached to the oral tubercles, which they serve to separate, and by this means open the mouth. The oesophagus is muscular, four or five lines in length, and separated by a well-marked constriction from the intestinal canal. The intestinal canal is readily distinguished by its brown or dark greenish colour, occasioned by the matter which it contains. It is a thin transparent membranous tube which proceeds nearly in a straight course to its termination at the anus. To its external surface are attached a great number of soft obtuse processes, the nature and use of which are unknown. They arise chiefly in the dorsal and ventral regions, and are continued from numerous transverse bands which pass across the body from one lateral vessel to the other. Both the obtuse processes and the transverse bands are composed of a homogeneous spongy tissue without any central cavity. This worm is oviparous, and the



Transverse section of A. lumbricoides, magnified.

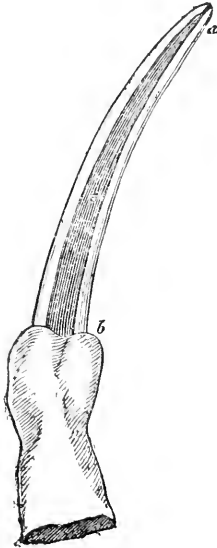
e, Lateral vessel.
f, Soft obtuse processes.

generative apparatus, in both sexes, occupies a great part of the body. In the female the vulva is situated at the junction of the anterior with the middle third of the body, and forms the entrance to a distinct vagina, which is about five or six lines in length. This leads to the uterus, which soon divides into two slender cornua or oviducts of a whitish appearance, which are many times longer than the body, and gradually diminish in size to capillary tubes, regarded as ovaries. These organs form numerous and almost inextricable coils around the alimentary canal, and one of them, unravelled by Hunter, measured more than four feet. In the male worm there is a single seminal tube, which, like the oviduct of the female, is a slender canal of remarkable whiteness,

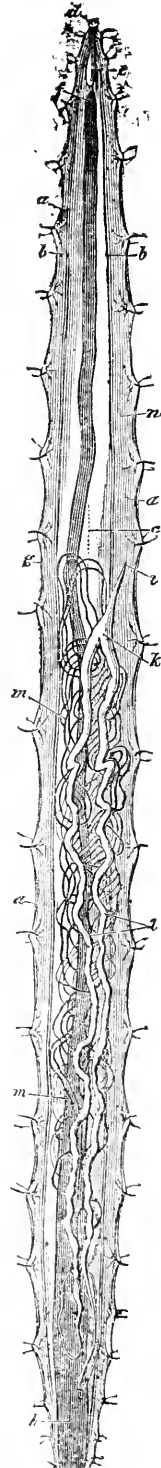


Nutritive processes and vascular canals magnified.

c, Lateral longitudinal line or blood-vessel.
d, Nervous cord.



Penis of *Aescaris lumbricoides*.



Female Organs.

a, Mouth. c, Esophagus. f, Intestinal canal. k, Uterus. l, Oviducts.

much longer than the body, being nearly three feet in length, and likewise arranged in a similar tortuous manner. It terminates in a seminal reservoir, which is about an inch in length, and communicates with the base of the penis, a slender conical process, which may be protruded or retracted, according to circumstances, by muscular fibres adapted for the purpose. At the extremity of this organ a minute pore may be observed with the aid of the microscope. The females are more numerous and more frequently met with than the males.—These worms are not only found in man, but infest likewise the horse, the hog, and the ox.

The *Ascarides lumbricoides* inhabit naturally the small intestines, although they often make their way into the large, and are voided by stool. They sometimes even pass into the stomach and œsophagus, and escape by the mouth, or nostrils; and there are some rare instances on record, in which it is stated that they had gained entrance to the larynx and trachea. Laennec, Guersent, and Cruveilhier, have met with these worms in the hepatic ducts and biliary canals. The latter pathologist is inclined to believe that they had migrated into this unusual situation after death, or during the last moments of existence. Dr. Hodgkin alludes to two cases in which they passed out at an artificial anus. Sometimes only one or two are present in the intestines, at other times they exist in considerable numbers. Guersent and Dr. Hodgkin have found between fifty and sixty of these worms in one subject; and Dr. Hooper speaks of a girl, eight years of age, who voided upwards of two hundred in one week. They are far more common in children than in adults, and are rarely found in old age. When present, they excite more or less irritation in the mucous membrane. We have a preparation in the museum, showing considerable hypertrophy of the follicles, both of Peyer and Brunner, in a portion of intestine which contained a good-sized worm. There is generally a copious secretion of mucus, effused, without doubt, for the protection of the lining membrane. The mucus is often thick and viscid, and it existed in such abundance in some cases of fever accompanied with worms, noticed by Pinel, that he was induced to apply the term of "mucous" to the disease. The accounts to be met with in the old authors, of these worms eating through the coats of the intestines, are not worthy of much credit. They sometimes, it is true, occasion ulceration, which may extend until perforation is produced, but even this is a rare occurrence. There are, however, a few cases on record, in which an abscess formed in the parietes of the

abdomen has burst, and given vent to pus, mixed up with gas and feculent matter, and likewise one or more of these worms. It would appear, from those instances in which dissection was performed, that the peritoneal tunic at the site of ulceration had contracted adhesion to the abdominal parietes, and that the subsequent extension of the ulcerative process had caused the formation of an abscess in the subcutaneous cellular tissue, which, bursting externally, made a passage for the discharge of the worm, together with the feculent contents of the canal. The *ascaris lumbricoides* is not, indeed, furnished with any apparatus which would enable it to perforate the living tissues, and is very rarely, if ever, found attached to the mucous membrane, or entangled in the follicles.

The symptoms originating in the development of these worms in the intestines are of the same inconstant and vague nature as was remarked of the other entozoa infesting the alimentary canal. It often happens that digestion is more or less impaired, the breath fetid, the appetite deranged, and that there is a sensation of sinking, or faintness, at the epigastrium; itching at the nose, occasional diarrhœa, colicky pains in the abdomen, and a degree of general emaciation. These are the symptoms most commonly observed, yet I have known individuals who have every now and then passed an *ascaris* during several years, and yet have betrayed no indication of ill health. There are, in fact, no signs or assemblage of symptoms which enable us to determine with certainty the presence of worms in the intestinal canal, unless they have been expelled from the bowels.

17. *Ascaris vermicularis*.

Before speaking of treatment, I must proceed to describe the last species of entozoa remaining to be noticed—viz. the *Ascaris vermicularis*, called by Bremser, *Oxyuris vermicularis*; the common thread-worm. This worm was known to Hippocrates, and received the name of *ασκαρίς* (from *οσκαρίζω*, to leap), on account of its supposed activity. It varies in size according to the sex; the males rarely exceeding two lines in length, and the female measuring about five. The body is proportionally slender, white, round, and highly elastic. The head is obtuse, with a transparent membrane on each side, and, according to Rudolphi, presents the three valvular papillæ characteristic of the genus *ascaris*. Bremser, and other helminthologists, have not succeeded in detecting these papillæ; and, believing that they do not exist, have referred this species of worm to the genus *oxyuris*. The

*Ascaris Vermicularis.*

* Natural size.

tail of the male is spiral and obtuse; that of the female, straight and tubulate. These worms are so exceedingly minute, that their internal organization cannot be very readily observed. It is very similar, however, to that of the *ascaris lumbricoides*.

The *ascaris vermicularis* inhabits the large intestines, especially the folds at the inferior extremity of the rectum, where it is found in large numbers. Bremser states that he has met with it in the cæcum. It is the most troublesome parasite infesting children, though it is found occasionally in adults. Bremser mentions the case of an old man, eighty years of age, who was troubled with these worms until he died. It is stated that they sometimes make their way into the vagina, where they cause intolerable itching, discharges, and even nymphomania. Children affected with them experience an itching at the anus and at the nose, which they frequently rub and scratch until they produce excoriation and bleeding. They have a repeated inclination to go to stool, with tenesmus, and the motions are slimy, and sometimes tinged with blood, and generally contain a few of the worms. Pro-lapsus ani also frequently exists, owing to the straining and forced efforts made by the patients on going to stool. The general symptoms which they give rise to are very various, and occasionally of a threatening nature. Convulsions and other cerebral symptoms, excessive appetite, nausea and vomiting, griping and purging, feverishness, and general emaciation, may likewise be occasioned by ascarides; whilst, in many cases, there is so little

deviation from health, that their existence would never be ascertained, or even suspected, if they were not detected in the stools. But, however slight may be the irritation and disturbance dependent on these and other kinds of the intestinal entozoa, it is always an important object to obtain their expulsion from the system. Their presence may not, perhaps, be indicated by unequivocal symptoms of morbid action; still they operate, often in an imperceptible manner, unfavourably on the constitution, and are the cause of a sort of *malaise*, a condition far removed from high and vigorous health, and expressed by enfeeblement of the powers both of the mind and body; thus constituting an evil, though not of a striking nature, still one which it is very desirable should be removed. Besides, too, any continued irritation, however mild and slight in its effects, tends to bring the constitution into a condition favourable for the reception of morbid impressions, which, in strong and robust health, would not be succeeded by hurtful results. Whenever, then, the existence of any of these parasites has been ascertained, whether the symptoms be mild or grave, an appropriate course of treatment should be pursued for their destruction and expulsion.

The medicines by which these objects are attained are called anthelmintics; but as treatment scarcely comes within the scope of these lectures, I shall only allude to a few of the remedies which enjoy the best reputation amongst practitioners. The expulsion of intestinal worms can be effected by the exhibition of an active purgative. For this purpose you may administer a large dose of calomel and jalap, or croton oil. There are some medicines, however, which appear to be extremely unfavourable to the life of these animals, such as the oil of turpentine, one of the most common remedies employed in these cases, and a very efficacious one, especially as its purgative action obtains the expulsion of these worms after they have been destroyed. Although turpentine had been previously resorted to in worm cases, Dr. Fenwick, of Durham, has the credit of having made its virtues more generally known, by a paper published in 1811, in the second volume of the *Medico-Chirurgical Transactions*. He was led to adopt this remedy in consequence of learning from a friend, that a sea-faring man, suffering severely from the tape-worm, having observed that whenever he drank rather freely of gin he always passed portions of the worm and experienced relief, was induced to take a wine-glass full of the *Oleum Terebinthinæ*, under the impression, that being of the same nature, but stronger than gin, it might effectually cure him;

The consequence was, that about two hours afterwards he passed, with a purgative stool, an entire tape worm; from which time the complaint did not return. Turpentine is useful in all kinds of intestinal worms. It should be given in large doses, as half an ounce or an ounce, in conjunction with or followed up by castor oil. In infants, from half a drachm to a drachm may be safely taken in milk. In cases of the small ascarides, it has been injected into the rectum with great benefit. In cases of tænia, it is generally advisable to repeat the dose of the turpentine two or three times, in order to destroy the ova of the worm, which, being left in the canal, might lead to their reappearance. Chabert's empyreumatic oil, which is strongly recommended by Rudolphi and Bremser, and is much used in Germany, especially in cases of tænia, consists of a mixture of one part of the oil of hartshorn with three of the oil of turpentine, which having stood for three days, three-fourths are distilled off in a glass vessel, by the heat of a sand bath. The bark of the pomegranate root, a remedy for worms known to Celsus, has recently been recommended to the notice of the profession as a valuable anthelmintic for those affected with tape-worms. It is a remedy very generally employed in France, and may be exhibited in the form of decoction or powder. The latter, which is considered the most efficacious, is given in doses of ℥ij. or ℥j. two or three times a day. The fresh bark is found to be more active than the dry. Some caution is necessary in its administration, since an over-dose has the effect of producing giddiness, sickness, convulsions, and purging. The cowhage, *delichos puriens*, or cowitch, a remedy highly recommended some years ago, by a practitioner of the name of Chamberlain, has been a favourite vermifuge in cases of lumbrici and ascarides in children. It is given in the form of an electuary, and is usually followed by a smart purgative. The occasional employment of an injection of cold water, a mode of treatment sanctioned by the authority of Van Swieten, has been found successful in keeping the rectum free from the ascarides vermiculares. Being so readily within reach, these worms have been attacked by other remedies in this way, as mercurial ointments, turpentine clysters, and injections of bitter medicines. There are many secret and quack medicines and recipes which have obtained a considerable reputation as anthelmintics, but they are mostly composed of the substances already mentioned.

It has been common amongst practitioners to refer too many of the diseases of infancy to the presence of worms in the

intestinal canal, and active medicines have been exhibited, under an erroneous supposition of this nature. Powerful anthelmintics, indiscriminately employed, may, however, prove more hurtful to the patient than the evils which they are intended to remove; and without the conclusive evidence afforded by the discovery of worms in the stools, we are scarcely warranted in resorting to the more irritating remedies recommended for our use in these affections. After the exhibition of vomitages, steel medicines, bitters, and other tonics, will often prove of essential service in restoring the tone of the alimentary canal, and in correcting the disposition of the organs which favours the development of these animals. It is stated that they abound in humid situations and unhealthy districts, and are common to those who take their food without salt, or who do not obtain wholesome nutriment. The diet, therefore, and other circumstances favourable to a sound state of health, should likewise be objects of care; and as the production of worms is frequently secondary to derangement of the important organs of digestion, it will often be found, when their functions are improved, and the general health properly attended to, that the parasites will disappear without any other means being employed for their removal.

ON THE FIRST CHANGES

IN THE

OVA OF THE MAMMIFERA

IN CONSEQUENCE OF IMPREGNATION,

And on the Mode of Origin of the Chorion.

By THOMAS WHARTON JONES, ESQ.

Communicated to the Royal Society by Richard Owen, Esq. F.R.S.*

PART I.—On the Changes in the Envelopes.

HAVING previously described the structure of the ovum of mammiferous animals, as it exists in the ovary before impregnation†, I now proceed to relate some facts respecting the changes which it undergoes in consequence of that act.

My observations in reference to this point are the following:—

Observation 1.—On Wednesday, the 16th, and Thursday, the 17th September,

* See Philosophical Transactions, Part II. for 1837, p. 389.

† See Lond. and Edin. Phil. Mag. vol. vii. p. 209.

1835, I examined the internal organs of generation of a rabbit, which had been impregnated on the afternoon of the Saturday preceding, and which was killed on the afternoon of Tuesday. The ovaries of both sides presented corpora lutea.

In the fallopian tube of the right side, near where it enters the horn of the uterus, I found six ova. In the same place on the left side there were only two. They differed very remarkably from the ova as they exist in the ovaries before impregnation, inasmuch as the former presented, in addition to the component parts of the ovum of the ovary, a thick gelatinous matter surrounding it, similar to what is observed in the ovum of the frog. The addition of this gelatinous envelope made the diameter of the whole body about 1-70th of an inch. Fig. 1 represents one of these ova magnified 40 diameters, and fig. 2 the ovum of the frog when recently laid, magnified 2 diameters.

I could not detect the germinal vesicle in the ova in question. The granular matter of the yolk was coherent. The application of weak vinegar to the ova rendered the yolk transparent. Dilute nitric acid made the superadded gelatinous envelope contract, but by the addition of more water it gradually expanded again.

The question which this observation suggests is, "Where do the ova acquire the additional gelatinous envelope; in the fallopian tubes or in the ovaries?" The two following observations give the answer, "In the ovaries."

Obs. 2.—March 6, 1836. Examined a female rabbit to-day, 41 hours and 40 minutes after impregnation. There were *no ova* in the horns of the uterus, nor in the fallopian tubes.

The right ovary presented on its surface a very large and prominent graafian vesicle, quite transparent, except at its most projecting point, where there was a spot of blood. I perceived nothing peculiar in the ovum contained in this vesicle. I did not detect a germinal vesicle in it.

Besides this large and prominent graafian vesicle there were on the surface of the right ovary other five prominent vesicles filled with coagulated blood. At the most projecting point of each of these there was a small whitish mamillary elevation, within which was contained the ovum, surrounded by a

transparent gelatinous substance, the same as that described in the preceding observation; only it is to be remarked, that in the fallopian tubes this gelatinous-looking substance had swelled out and acquired a greater diameter than it presented in the ovary. I did not detect a germinal vesicle in the ova forming the subject of this observation.

In the left ovary I found only one vesicle, containing the coagulated blood and the ovum surrounded by the gelatinous-looking envelope.

Obs. 3.—A rabbit, 48 hours after impregnation, presented appearances much the same as the above.

Is any trace of the gelatinous-looking envelope of the ovum to be observed before impregnation? In the ova of the rabbit, &c., before impregnation, the proligerous disc, in which the ovum is embedded, is observed to be composed of a gelatinous substance, interspersed with grains, but as yet there appears no distinctly circumscribed envelope*.

The gelatinous-looking envelope of the ovum I have just described must not be confounded with the vitellary membrane of the ovum, which was fully considered in my former paper. The former appears to be analogous to the cortical membrane surrounding the ovum of the *Ornithorhynchus paradoxus*, &c. while still in the ovary, described by Mr. Owen†. That it, and not the vitellary membrane, as I formerly imagined, forms the chorion, will be made evident by the following observations:—

I would, however, premise some remarks on the ova of the batrachian reptiles, in order to place in a more striking point of view the circumstances I am about to relate in regard to the ova of the mammifera.

Fig. 2 exhibits the ovum of the frog magnified two diameters. It is composed of a yolk, black on its surface, and whitish inside. The yolk is surrounded by a vitellary membrane, thicker than that of the bird's egg, but thinner in proportion than that of the ova of the mammifera. Outside the vitellary membrane is a gelatinous envelope, which is added in the oviduct, the two preceding

* Dr. Karl Krause, of Hanover, however, in a late number of Müller's Archiv., speaks as if the gelatinous substance really formed a well-defined envelope. From his observations on the ovum before impregnation he has been led to form much the same opinion regarding the origin of the chorion as is recorded in this memoir.

† Philosophical Transactions, 1834, p. 561.

parts being formed in the ovary. When the ova are laid, the gelatinous envelope rapidly absorbs water, and swells out to great thickness.

The ovum of the newt differs from that of the frog, inasmuch as the gelatinous-like matter which surrounds the yolk and its membrane is of an oval form, and is somewhat hardened on the surface, so as to form a kind of shell, inside which is a fluid substance, in which the yolk and its membrane can freely revolve and glide from one end to the other. The vitellary membrane is thinner in the newt than in the frog. Fig. 3 is the ovum of a newt, in which development has commenced, magnified rather more than twice.

But what I wish particularly to insist on, in regard to the ova of the batrachian reptiles, and especially of the newt, is, that when the embryo of the latter has attained a certain size, but still at an early period, the vitellary membrane gives way, and then the embryo is only contained within the cavity of the substance, which is added to the ovum in the oviduct, fig. 4.

In the case of the frog the vitellary membrane does not give way, until about the time the tadpole is ready to burst all its envelopes. With the development of the embryo the cavity circumscribed by the vitellary membrane increases to as much as one-fifth of an inch in diameter, and always retains its spherical form. There is a limpid fluid in the interior of the vitellary membrane which seems to serve the purpose of an amniotic fluid, fig. 5.

Obs. 4.—March 18 and 19, 1836. Examined a female rabbit seven days after impregnation. The right ovary presented four corpora lutea, the left ovary two. I found only one ovum in each horn of the uterus; they were about 1-50th of an inch in diameter*. Fig. 6, magnified 40 diameters.

No vitellary membrane was to be seen. The gelatinous-looking envelope constituted the only covering of the yolk, which now formed a spherical blastoderm. The cavity of the gelatinous-

looking envelope was much larger than the spherical blastoderm. The inner surface of the gelatinous coat presented what I supposed to be fragments of the vitellary membrane adhering to it.

In both ova the spherical blastoderm was irregular on one side, that on which I supposed the embryo was about to be developed. It was beginning to present the separation into layers, and had the same peculiar friable globular structure as the blastoderm of the hen's egg.

Obs. 5.—This observation, which refers to the human ovum, agrees with that just related in regard to the ovum of the rabbit.

In the spring of 1836 I examined a small human ovum sent to me to Cork, where I then was, from Glasgow, by Dr. Mackenzie. In his letter to me, dated November 29, 1835, he describes it thus: "A very small human ovum. It came along with the entire decidua from a patient of mine. It lay in the middle of one of the parietes of the decidua, rather near its upper edge, and was about the size of a marrowfat pea, before being put into spirits. The decidua covering it, towards the hydroperic cavity, was thin and semi-transparent, but the opposite portion of the decidual nidamentum was thick, and marked with foramina, as if from vessels which had penetrated and adhered to it. Having opened the nidamentum and taken out the ovum, I observed what will immediately strike you, that one side of it was bald and the other shaggy with the villi of the chorion. The bald part lay towards the hydroperic cavity. A small puncture was made through the chorion, and perhaps through the amnion, by which some fluid escaped: nothing more was attempted. The fallopian portions of the decidua measured nearly half an inch, and were both entire."

In a subsequent letter Dr. Mackenzie says, in reference to the age of this ovum, "The ovum in question I consider as three or four weeks old. The lady had missed one menstrual period, and thought herself four weeks gone."

On laying open the ovum, by carefully cutting and reversing the bald side of the chorion, the following appearances (delineated, natural size, in fig. 7) presented themselves. The whole cavity of the chorion was filled with a fine

*The reason I found but two ova is, perhaps, that from their great transparency they may have escaped notice. The gelatinous coat was so transparent that I could with difficulty see the outline of it under the glass when it was observed by transmitted light. The spherical blastoderm being opaque was the only circumstance that enabled me to detect the ova at all.

gelatinous cellular tissue, imbedded in which, towards one extremity of the ovum, was a small round body.

It was evidently the spherical blastodermis; on being taken out and examined under the microscope, it presented the same friable globular structure found in the spherical blastodermis of the rabbit in the preceding observation. There was no vitellary membrane to be seen.

From observation 4 it may be inferred, that in the progress of the development of the ovum of the rabbit the vitellary membrane gives way, as in the ova of the newt, and indeed of many of the oviparous animals; that the gelatinous coat acquired by the ovum in the ovary, and more especially circumscribed and defined after impregnation, constitutes the only covering of the spherical blastodermis after the giving way of the vitellary membrane; that this gelatinous-looking coat forms the chorion, which in the rodents at a further stage of development presents itself under the form of a thin and transparent membrane, very like the vitellary membrane of the bird's egg, situated immediately outside the non-vascular and reflected layer of the umbilical vesicle.

The conclusions to be drawn regarding the human ovum, from observation 5, are the same as above. The human ovum, as regards the spherical blastodermis, was in much the same stage as the ova of the rabbit seven days after impregnation; the vitellary membrane had disappeared, or been resolved into the gelatinous cellular tissue filling the interior of the chorion; and the embryo had not yet appeared, though the spherical blastodermis was undergoing the preparatory changes. As regards the chorion, the human ovum was more developed than that of the rabbit, but it is to be remarked that even in an after stage of development the same difference in structure continues to prevail.

PART II.—On the Changes in the Vitellus.

What I have to communicate in this second part of my memoir is of a much less definite character than that which is given in the first part. From the nature of the subject it in many cases necessarily consists of inferences rather than observed facts. It relates chiefly to the ova of the batrachian reptiles, and is added here merely for the pur-

pose of throwing some light on the changes which take place in the yolk of the ova of the mammifera, previously to the commencement of the evolution of the embryo.

In approaching this subject the first question which presents itself is: "When does the germinal vesicle of the ova of the mammifera disappear; before or after impregnation?" It is known that in birds and reptiles the germinal vesicle disappears before impregnation. In the ova of the frog, contained in the oviduct, and also in the more advanced of those contained in the ovary, no trace of the germinal vesicle is to be observed. The black blastodermis surrounds the whole yolk, with the exception of a small spot* on the opposite side to that where the primitive streak appears. In the furthest advanced ova contained in the ovary of the newt, the blastodermis was formed, and I think I perceived the place where the germinal vesicle had been. As to the ova of the mammifera, I have found many in which there was no germinal vesicle, and which certainly had not been impregnated. It is to be remarked that in such ova the vitelline grains were for the most part coherent, and formed the spherical blastodermis.

It being determined that the disappearance of the germinal vesicle is prior to impregnation, and not dependent on it, the next question which arises is, "how does the germinal vesicle disappear?" My observations on the ova of the water-newt are the only ones I have which bear upon this question. From what I have observed in them I think the mode of disappearance is the following:—

The vesicle, at first imbedded in the substance of the yolk, approaches more and more the surface of it, until it comes to lie immediately underneath the vitellary membrane. The coat of the vesicle having now become very soft and weak, gives way, and the contained fluid is

* This small spot of the ova of the frog, which is white, (from the exposure of the white yolk,) always turns to the most depending side. The germinal point is thus always uppermost. I turned a mass of spawn upside down; the white spot was exhibited by all, but in a short time the white spot had turned downwards, and the germinal surface again became uppermost. In this case, does the vitellus alone revolve, or does the vitellus and its membrane turn round together in the gelatinous substance surrounding the ovum? It appeared to me that the latter was the way in which the revolution took place.

effused on the surrounding surface of the yolk. The coat of the vesicle being of extreme tenuity cannot be seen after it has given way. The small depression in which the vesicle was situate now forms the cicatricula.

I think that the fluid contained in the germinal vesicle being effused, gives a degree of consistence to the matter composing the surface of the yolk, and thus promotes the formation of the blastoderma.

If, then, the disappearance of the germinal vesicle is not dependent on impregnation, it may be asked what is the first change which takes place in the ova in consequence of impregnation? Of all the ova, the ova of the frog are those in which such change can be most directly observed. In them *the breaking up of the surface of the yolk into crystalline forms*, described by Prevost and Dumas, is the first change I have seen.

March 17th, 1835. I examined to-day the spawn taken from a frog yesterday, part of which was impregnated and part not; that which was impregnated presented the appearance delineated in fig. 8. The unimpregnated ova presented no change. The surface of the yolk becomes every day still more broken up, the crystalline forms becoming smaller and smaller, until the surface of the black blastoderma appears under a magnifying glass like shagreen.

The blastoderma, consisting of an aggregation of clear globules, different from those of the rest of the yolk, is now fully formed; it has extended itself so as to close in the white spot. Evolution then proceeds.

The change which takes place in the yolk of the bird's egg appears to be limited to the neighbourhood of the cicatricula. In the ovum of the mammifera, there being little more than a blastoderma to be formed, the whole of the vitelline grains undergo a change, and are resolved into a spherical blastoderma, presenting the same peculiar friable and globular texture as the blastoderma of the egg of the newt, frog, bird, &c. The matter contained in the cavity of the yolk of the bird's egg seems to be a substance of the same nature as the blastoderma, and to serve for the extension of it. The blastoderma of the bird's egg being once formed by the effusion of the fluid of the vesicle of Purkinje, and animated by fecundation, probably has the power to assimilate the matter in the cavity of the yolk to its own substance, without the assistance of a fluid such as that of the vesicle of Purkinje, which was first required to promote its formation. There is no central cavity in the ova of the frog and newt, because the blastoderma is formed at once all round the ovum.



1



7



2



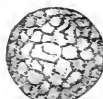
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8



6

Description of the Figures.

Fig. 1. An ovum found in the fallopian tube of a rabbit the third day after impregnation; magnified forty diameters.

Fig. 2. The ovum of the frog when recently laid; magnified two diameters.

Fig. 3. The ovum of a water newt in which development has commenced; magnified rather more than twice.

Fig. 4. A diagram showing the embryo of the newt after the vitellary membrane has given way, contained only within the cavity of the substance which is added to the ovum in the oviduct.

Fig. 5. A diagram showing the embryo of the frog still surrounded by the vitellary membrane, as well as the gelatinous substance which is added to the ovum in the oviduct.

Fig. 6. An ovum found in the horn of the uterus of a rabbit seven days after impregnation; magnified forty diameters.

Fig. 7. A human ovum aborted at the third or fourth week; natural size.

Fig. 8. This exhibits the breaking up into crystalline forms, observed on the surface of the frog's ovum after impregnation; magnified about six diameters.

RECOLLECTIONS OF CHOLERA;

ITS NATURE AND TREATMENT.

BY WILLIAM GRIFFIN, M.D.

Limerick.

Mortality of Cholera.

No. V.

THE inferences from the foregoing reports is so much more favourable to medical science than the aggregate amount of deaths would lead one to anticipate, that doubts might possibly be entertained of their accuracy; but independent of their agreement in demonstrating the vast proportion of cures which may be effected in the early stage, as well as the usual incurability of the cases admitted in collapse, I can directly answer for the general correctness of those kept in the hospitals with which I was connected.

It appears by these reports, that in no hospital was the mortality over 16 in

the 100, or less than 4 in the primary stage. I think it probable that the small proportion of four deaths in the 100, was more dependent upon the admission of a greater number of light cases than upon any essential difference or increased success in the treatment; as both rates of mortality occurred in my own practice, without any important change of remedies or management. When I come to speak of these, however, by and by, it will be necessary to recollect that the results, such as they appear, have been tested in 1665 cases; a number which, I believe, it will be readily admitted must set all question on the subject at rest.

As the number of cases brought into hospital shrunk, cold, and pulseless, is seldom less than one-third, generally amounts to half, and sometimes exceeds two-thirds of the total admissions, the average rate of mortality in collapse, on the whole, as given in those reports, appears frightful. The apparent failure of the treatment made me particularly anxious to compare the results with those in other hospitals throughout the kingdom; but all my inquiries for this purpose ended in disappointment. There was, considering the importance of the subject, a strange deficiency, or disingenuous reserve, observable in a great proportion of the published statements from medical men, or (what was equally injurious to science) a propensity to draw general and absolute conclusions from a very moderate experience. To the latter, indeed, we would in charity attribute many of those individual pretensions to success with remedies, new neither in this nor in other countries. The probable mortality, however, may perhaps be gathered pretty correctly from the following statements:—

Dr. Christison, speaking of thirty-seven cases of collapse treated at Edinburgh and Leith by saline injection of the veins, says that not more than two or three of these would, at all events, have recovered, according to the previous experience of the medical men there; which is estimating the recoveries from collapse, under ordinary treatment, below one in ten. Dr. Colclough, staff-surgeon, in his report to the Army Medical Board, says, he has seen no recovery in any military patient, from collapse *with total loss of pulse*; but believes such recoveries may have taken place, though rarely, in the general cholera

hospitals, which are filled with impoverished, poorly-fed patients, among whom the disease is usually of a milder type.

Mr. Annesley observes, "that most of the fatal cases were nearly moribund on their admission into hospital; on the other hand, all the cases that recovered were brought for assistance *before the circulation had completely ceased*, and when blood could be freely drawn." Surgeon Gordon Satara says, "*after the pulse has failed at the wrist*, and the extremities have become cold, I consider the patient as almost irrecoverable." Surgeon Corbyn, Camp Eritch, states it as his opinion, "that unless remedies are resorted to within six hours after the attack (i.e. before collapse has set in), the case is almost hopeless." In the circular of the London Board of Health, dated August 9th, it is said of the stage of confirmed collapse, "that there is but little reason for hope;" a statement amounting, Dr. Hardwicke Shute conceives, "to an admission, founded on the most extensive observation which the records of the disease in this country afford, that a very great majority of the patients (say 49 in 50, or at least 18 in 20) perish." Dr. Venables says, in a letter from Whittlesea, published in the MEDICAL GAZETTE, "The fully formed collapse, or even the advanced approaches to it, seem as unmanageable here as it has proved in most other places. I have, however, witnessed *one recovery* and *one convalescence*, under these circumstances." Dr. Miller, in a communication to the MEDICAL GAZETTE, asserts, "*that all cases of collapse die*.* A report from a medical gentleman in Liverpool, who was connected with an extensive hospital there, states—"Of *fifty-four cases of collapse*, we have saved but *three*, and even in these there was some little heat of surface." A physician in Dublin, to whose valuable observations on the disease, forwarded to me during its prevalence there, I must always refer with pleasure, says, "I would go so far as to assert, that in the young and robust, if not unusually bad cases, the recoveries from collapse may be rated as *one in three*; while in the old, enfeebled, and dissipated, they may not be *one in twenty*. Suppose the mean—there would be nearly *one in*

twelve, which is not far from what is generally admitted."

Allowing for differences that may exist in the severity of the disease, and in the proportions of the young or the old attacked at any particular place, there appears to be a very general similarity in the results of cases in collapse. If we look to the only accurately detailed reports that could be offered, the mortality varies from about nine to seven in ten; or, in larger numbers, from 90 to 70 in 100. These would appear to be the natural results; and sad as they are, it is a question, with our present experience, whether active medical treatment may not be found to increase rather than diminish the fatality. In Limerick, in proportion as medical men lost confidence in all the various remedies suggested in the treatment of collapse, and trusted more to the efforts of the constitution, the recoveries seemed to increase.

With respect to those differences in the type of cholera which have given it the character of mild or severe at particular periods or places, it is necessary to give some general idea of the extent to which they might possibly influence the rate of mortality. Looking merely to the reports, they may appear to be so great as to render any just comparisons between one mode of treatment and another exceedingly difficult; but a more intimate knowledge of the disease will convince us, that the lighter mortality at all times depends, not so much on the greater curability of cases in collapse, as on the disease becoming more prolonged, and thus allowing more time for treatment, or removal to hospital, previous to the extinction of the pulse. Patients in the advanced or collapsed cases die nearly in the same proportion as before, but a greater number are admitted in the primary stage, and there are consequently less deaths upon the whole. There is no disease so true to itself as cholera—so constant in its characteristic symptoms—so steady in its amount of destructiveness. It is violent and severe at its onset in every place, not simply because it is less under the influence of medicine at any particular stage than afterwards, but that it runs its course so rapidly, there is no opportunity of treating it in any but the last. The malignity or fatality of cholera, in fact, consists principally in

* MED. GAZ. July 31st, p. 543.

the speed with which it has passed through the primary stage, for on this depends the usually hopeless state in which patients are brought into hospital, or fall under treatment. Hence it is, looking to the aggregate mortality merely, we can never form a just estimate of the influence of treatment. A report from one hospital, for instance, may give only 26 deaths in 100, while another gives more than double that number; and yet, on strict investiga-

tion, it may be found that the treatment in the latter has been the more successful of the two. For instance, if in one hospital the cases admitted in collapse amounted to three-fourths of the total amount, and in another only to one-fourth, the former would have double the number of deaths, even though the treatment was precisely twice as successful, as the following statement will show:—

First Hospital.

	Admissions.	Cured.	Died.
In the primary stage	25	24	1, or 4 in 100.
In collapse	75	15	60, or 2 recoveries in 10 cases.
Total	100	39	61, or nearly two-thirds of the whole.

Second Hospital.

	Admissions.	Cured.	Died.
In the primary stage	75	67	8, or 8 in 100.
In collapse	25	2½	22½, only 1 recovery in 10.
Total	100	69½	30½, or nearly one-third of the whole.

There is another source of fallacy in the general reports of hospitals: a number of patients in the premonitory stage of the disease, that is, not in cholera at all, are included in them to a greater or less extent. To point out this, it is not necessary to contrast one hospital with another, but simply to contrast the reports of two different periods in the same hospital. It will scarcely be believed they are records of the same disease, the results sometimes prove so different. In Dublin, during three weeks commencing the 1st of April, there were 121 cases, and 66 deaths; while in the three weeks subsequent to the 16th of May there were 784 cases, and only 69 deaths. In Cork, from July 16th to August 9th, there were 121

cases, and 69 deaths; while from the 11th to the 25th May there were 710 cases, and only 93 deaths. In the Limerick general reports, from June 11th to 18th, there were 435 cases, and 208 deaths; while from July 1st to the 17th, out of 481 cases there were only 121 deaths. In Barrington's Hospital in the same city, from 11th July to August 17th, there were only 79 deaths in 396 cases; while from August 26th to September 27th, 139 died out of 200 admitted; and from September 23d to April 17th, 121 died out of 217. The contrasts will appear still more striking in the daily reports. The following are from the Dublin hospitals in April and May:—

	Cases.	Died.
Dublin, May 19	97	10
20	28	3
21	38	5
22	46	2
23	40	5
24	40	4
25	90	7
Total	379	36

	Cases.	Died.
April 8	14	11
10	5	5
12	9	2
14	6	7
15	6	2
16	8	2
17	10	9
Total	58	38

The medical reader need scarcely be told that of the 379 patients admitted in

the week in May, no more, probably, than 72 could have been in cholera. It

was not always in the power of the medical gentleman in attendance to prevent the admission of patients not in cholera; frequent impositions were practised by the poor for the sake of procuring clothes, which were given gratuitously to the convalescents, and those complaining of mere diarrhœa could not be safely rejected when cholera was very prevalent, or frequently of the rapid type.

This peculiar character of the disease, its rapidity, invariably marks its commencement every where, but seldom lasts more than from three or four days to a week: the complaint then assumes the protracted form; there is a greater opportunity for treatment; more cases are brought into hospital with perceptible pulse, and there are consequently more recoveries. The proportion of patients admitted into hospital in collapse in this country has, as I have already mentioned, never been less than one-third of the whole, and has seldom exceeded two-thirds, except in villages or hamlets where the poor were prejudiced against all medical treatment, and permitted their friends to be removed to hospital in the last stage only for the sake of obtaining coffins. The proportion of deaths in real cholera has been at all times as nearly as possible the same, varying with the variation in the number admitted in collapse. For as, under the best management, in a large number of cases the proportion of those admitted in the primary stage, who ran into collapse and died in despite of the treatment, nearly equalled the recoveries from collapse, the aggregate of the mortality necessarily corresponded with the aggregate of cases admitted in collapse. The greatest mortality which occurred in Limerick during the prevalence of cholera, as I have before stated, took place on its second incursion in Barrington's hospital, on the evening of the 26th of August, when, out of 73 cases admitted on that and the two following nights, 52 expired. These cases were mostly of the rapid type, and I believe more than three-fourths were brought in in deep collapse. In Paris, where the mortality was so terrific, the cases of collapse appear to have been immense; they were for the most part, as M. Magendie called them, "cadaverized cases." On the 28th of March, says the report*,

nine men labouring under intense cholera were admitted into the Hôtel-Dieu. One died in a few hours, seven more the next day, and one only recovered. A woman was next admitted about four in the afternoon, and expired before midnight. On the 29th, 23 new cases, 14 men and 9 women, were lying in the St. Martin and St. Monica wards; 8 of the men and 1 of the women died the same day, and the remaining 6 men on the day after. On the 30th, 40 new cases were received; 10 died the same day, and almost all the rest on the ensuing day. At midnight there were altogether 78 cholera patients lying in the Hôtel-Dieu; and on the evening of the following day only 16 were in life: these expired in the course of the subsequent days. At this period, of 27 females only 19 died; while among 51 men there were 43 deaths. "On the 3rd of April," the report continues, "when it became necessary to distribute the patients in all the wards on the left bank of the Seine, 388 patients had been treated in the wards of St. Martin and St. Monica, and next day 281 deaths were to be divided among the twelve physicians in attendance. Some days after, one-half at least of the residual 107 had expired, and threescore only quitted the hospital recovered."

This terrific mortality occurring under the treatment of the most celebrated physicians and physiologists in Europe, might very well put to shame the ridiculous and silly, or yet worse, the dishonest pretensions, of practitioners in this country. Such results in such circumstances ought rather to have made candid and thinking men distrust the extent of their experience of the disease, even if it led to a more favourable estimate of the influence of remedies. On my own mind, after witnessing the progress of nearly a thousand cases of cholera, the impression now is, that the mortality in Paris would not have been considerably less under any medical treatment which has been suggested or tried in this country; but that it might possibly have been somewhat less if the cases had been left wholly to nature. The patients were almost all brought into hospital in a state in which medicine is almost wholly uninfluential; the bodies corpse-like, the pulse extinct, and the violent and anxious efforts that were made to rescue them, as it were, from the verge of death, must, it is to be

* Archives Générales de Médecine, April 1832.

feared, have often precipitated them into it.

In estimating the mortality under different modes of treatment, besides the considerations to which I have adverted, it is necessary to attend to the ages, habits, and station in life of the patients. The deaths appear to be least at the middle period of life, amount to considerably more in infancy, and to most of all in old age, as the following tables declare:—

ST. MICHAEL'S HOSPITAL, LIMERICK.—*Report for the Month ending June 30th, 1832.*

Admitted at 45 years of age and upwards, 103, of whom 24 recovered and 79 died.
Under 45 years and over 15 years, 186, of whom 100 recovered and 86 died.
Under 15 years, 39, of whom 19 recovered and 20 died.

Report for the Month ending July 31st, 1832.

Admitted at 45 years and upwards, 29, of whom 15 recovered and 14 died.
Under 45 years and over 15 years, 115, of whom 90 recovered and 25 died.
Under 15 years, 21, of whom 17 recovered and 4 died.

ST. JOHN'S CHOLERA HOSPITAL, LIMERICK.—*Report from 28th August to September 10th, 1832.*

Admitted at 45 years and upwards, 19, of whom 8 recovered and 11 died.
Under 45 years and over 15, 54, of whom 37 recovered, and 17 died.
Under 15 years, 37, of whom 18 recovered and 19 died.

It was remarked in Paris, that not only were a greater number of men admitted into the hospital than women, but that a much greater number died in proportion. The observation would not appear to be at all applicable to the disease in this country. At its commencement, indeed, the very reverse was the fact with respect to the admissions—women constituting the great majority. After some length of time, the numbers of males and females in all the hospitals bore some near proportion to one another. The following is a summary of the report of the Nunnery Hospital, in St. Mary's parish, Limerick:—

Total number admitted	282	..	Died	124
————— of Males	102	..	—	56
————— of Females	180	..	—	68

It is evident the cholera varies very much in the selection of its prey at different periods; sometimes striking men

chiefly, sometimes women, sometimes laying society waste by families. In the first irruption in Limerick the sufferers were chiefly the middle-aged; the infants attacked were very few, and those few almost all died. On its second visitation the admissions took place in families; the children were in abundance, and many recovered.

It has been long observed of typhus fever, that although the rich are less liable to be attacked by it than the poor, it is much more mortal among them. This is as strictly true of cholera. The wealthy and well-fed are little liable to be affected; but when once seized with it, very generally die. I have by me a list of nearly 150 respectable persons who died in Limerick, which I should suppose is much more than half the number of that class who suffered with the disease.

Persons in advanced age, or of broken or dissipated habits, seldom recovered from cholera. In drunkards, it almost always assumed the rapid type. The mortality was also greater among those admitted in diarrhoea with little disturbance of stomach or cramps, but with a failing pulse, than where the vomiting was violent and the cramps distressing.

In conclusion, it may be assumed, from the foregoing observations—

1. That, as it appears the disease, if abandoned to nature, almost invariably runs into pulseless collapse, the mortality in all such cases may be rated at the usual average in that state.

2. That in patients in whom the pulse has ceased at the wrist, no known remedy possesses any decided influence in bringing about recovery; the mortality sometimes amounting to nineteen deaths in twenty cases; and under the most successful treatment, furnished in authentic reports of large hospitals, never proving less than seven in ten.

3. That the mortality in cholera cases, under judicious treatment, when commenced before the pulse had ceased at the wrist, varies from 4 to 16 in the 100.

4. That cases of cholera occurring when it is not epidemic, even though presenting all the characters of the Asiatic disease, are more protracted and manageable. They do not always run into collapse, even when neglected; and sometimes recover without assistance when they do.

5. That the severity of the epidemic

itself is very variable, differing materially, not only on different nights, but in different places on the same nights: so that to form just comparisons of the mortality, or of the influence of remedies in different hospitals, or in different wards of the same hospitals, it is essential the cases compared should correspond both as to time and locality. When this cannot be strictly done, the probable errors may be at least partially corrected by increasing the number compared; so that the un-noted varieties in the nature of the cases in the several hospitals may, in some degree, mutually compensate one another.

REMARKS ON PHRENOLOGISTS

AND

DR. ELLIOTSON,

BEING A POSTSCRIPT TO AN INTRODUCTORY LECTURE, BY R. GRAVES, M.D.*

SINCE my introductory lecture was delivered I have had leisure to peruse the second part of Dr. Elliotson's Human Physiology with the attention it deserves: as Dr. Elliotson has devoted so much of the volume to phrenology, I feel it right to add the following remarks:—

It appears certain, if we believe Dr. Elliotson, that Spurzheim's labours, and those of his pupils and followers, have done much to impede the progress of phrenology; of Spurzheim he says—"In his fondness for changing his names, his arrangement, and his numbering of the organs, he introduced confusion without advancing knowledge. To prove his speculative spirit, I may mention that instead of giving the origin of any of his asserted discoveries, as Gall did, and adding a host of examples, he tells us, in regard to the organ of inhabiteness, only that a gentleman much attached to his house had a particular spot of his head much hotter than any other; and, in regard to the organs of hope, marvellousness, conscientiousness, size, weight, order, time, he neither tells us how he discovered them, nor adduces a single proof. He changed even the situation

and extent of organs in his last plate. The space allotted by him to marvellousness was originally between wit, imitation, hope, and ideality; now it is more than twice its former size, and placed between these four, and veneration: covetiveness was placed by Gall, and admitted by Dr. Spurzheim, before cunning and ideality; now it is over cunning, and between ideality and cautiousness. Ideality, in his first edition, was chiefly above covetiveness and before circumspection; now it is above constructiveness, and a large organ stands between it and covetiveness."

Mr. Combe, we are told, drives away his readers in disgust; and Mr. Carmichael is said to be guilty of great mis-statement and great injustice towards Gall! When phrenologists use such bitter terms towards each other, we need not expect from them much courtesy to those who are not of their sect. Is it not refreshing to find a single Elliotson thus attacking a whole phalanx of giants?—"Few anatomists and physiologists have any idea of the errors, as to facts, and of the poverty of argument displayed by Cuvier, Tiedemann, Pinel, Esquirol, Richerand, Carus, Rudolphi, Serres, &c."

Sir David Brewster, too, is summoned before the judgment-seat, and receives from Dr. Elliotson a mild rebuke, calling both his honesty and ability into question. At p. 582 his sentence is thus recorded: "and yet Sir David Brewster would wish to be considered a philosopher in all his intellectual and moral doings." The reader will at once perceive that this philosopher is no phrenologist.

Sometimes Dr. Elliotson indulges in sarcasm against some unnamed person, as where, at p. 397, he says, "though Sydenham resided in Pall-Mall, he was never employed by the court; yet the names of the court physicians of his day are no more known than the names of the court footmen*." Removed as we are in Ireland from the seat of government and of court favour, we can

* The lecture itself shall appear in its proper place hereafter, in Dr. Graves' Course on Clinical Medicine.

* This sarcasm of Dr. Elliotson is not a fortunate one, for it so happens that the court physician of Sydenham's day was the celebrated Talbot, the very man who taught Sydenham the right method of administering bark; Talbot is highly praised in Madame de Sevigné's Letters for his cure of the Dauphin. Ray, in his *Historia Plantarum*, gives him the greatest praise.—Vide *Lancet*, Dec. 23, 1837: Dr. Sigmond's Lecture on Bark.

only guess at Dr. Elliotson's meaning; and we therefore cautiously interpret this sentence as meant to make little of the present court physician, Sir James Clarke. If this surmise be right, it may be well to remark, that Dr. Clarke's book on Climate, and his work on Phthisis, are extremely prized in Dublin, and that all the profession here were pleased to find the management of the Queen's health entrusted to the care of so able a physician; but perhaps Dr. Clarke is not a phrenologist.

As for the late Dr. Fletcher, Dr. Marshall Hall, and Sir C. Bell, they have been spared by Dr. Elliotson; he merely says, "The views of all these gentlemen appear to me equally confined and erroneous" (p. 496.)

In two different parts of this physiological work, Dr. Elliotson presents us with a list of his own discoveries in the practice of physic and pathology. These extraneous matters are very ingeniously introduced, and apparently for the purpose of self-defence; for his hand is against every man, and *he* imagines every man's hand to be against him. In truth, he writes like one who thinks himself an object of peculiar persecution, and one who believes that his merits in the cause of science have been sufficiently great to ensure the enmity of the whole profession. He talks of the inevitable opposition that awaits new truths on the part of mankind in general, but of himself he complacently remarks (p. 686), "I have never yet declared an opinion upon a new truth that I have been obliged to retract." He instances Aristotle, Galileo, Bacon, Newton, and Harvey, as men whose opinions drew on them jealousy, or even hatred—and then he speaks of the stethoscope, prussic acid, quinine, and the glanders!

These discoveries of Dr. Elliotson have drawn down upon his head the ill-will of his brethren, and have excited an opposition *we* should have but little anticipated. Had he acted wisely, or had he not been really fond of opposition and censure, he might have easily diverted contemporary malice into other channels; for in truth he could have found but little difficulty in disclaiming an apparent originality, when he learned that thereby he incurred so much odium. Thus, what could have been easier than to refer to the twelfth volume of the Dublin Medical Journal, in which Dr.

Eck's paper affords ample proof that Dr. Elliotson was anticipated, by at least ten years, with respect to the discovery of *glanders* in the human subject? Dr. Barker, of Dublin, published on, and recommended the quinine, before Dr. Elliotson; and the *stethoscope* was used diligently in the Meath hospital immediately after the publication of Laennec's work; yet no one here ever dreamed of having deserved praise, or having incurred censure, in consequence of using either quinine or the stethoscope.

MENSTRUATION AT AN ADVANCED AGE.

To the Editor of the Medical Gazette.

SIR,

AMONG the selections from foreign journals, in the British and Foreign Quarterly Review of this month, two cases are reported of menstruation occurring in aged females. Should you deem the following worthy of a place in your valuable and extensively circulated journal, you will greatly oblige

Yours respectfully,

ARCHIBALD BROWN, M.D.

21, College-Street, Glasgow,
18th Jan. 1835.

In the summer of 1835 I was consulted by Mrs. S., aged 56: the following are a few of the most prominent symptoms. Great emaciation, expression anxious and disturbed; skin harsh, dry, and jaundiced. Pulse 96, weak and irritable; tongue loaded with a brownish-coloured coating; bowels costive, stools whitish; urine scanty, and tinged with bile; hepatic region full, and slightly tender on pressure; liver is felt to be considerably enlarged; is distressed with occasional severe lumbar pains shooting down the posterior aspect of thigh, and downward and forward embracing the pelvic viscera. Catamenia ceased to flow fourteen years ago. Complaint, of seven months' duration, ascribed to cold and over-exertion.

The treatment adopted consisted of active purgatives; occasional use of the hip-bath; anodyne friction to lumbar region; and friction with nitro-muriatic lotion to the hepatic. This was followed by an alterative combined with a tonic and a light nutritious diet. Three weeks after commencing the alterative, I found

her, on calling, much excited, and on inquiring into the cause of it she told me "that during the night she felt as if something gave way within her, which was followed by a discharge similar to her usual courses." Having satisfied myself as to the appearance of the discharge, I enjoined rest, and stopped the medicaments for a few days. The quantity passed at that time, she mentioned, amounted to about the same as that discharged at the usual periods during the last year of menstruation. After this, there was a marked abatement of all her previous ailments, and a few weeks' continuance of the treatment above mentioned completed her recovery.

Catamenia continued to flow regularly every four weeks, with little variation as to quantity, for eleven months, and again subsided. I visited her the other day: her health continues good, but she has had no return of catamenia.

TRANSPOSITION OF THE VISCERA.

To the Editor of the Medical Gazette.

SIR,

By inserting the following case, in which the heart was situate on the right side, and an immense tumor occupied the anterior mediastinum, you would much oblige

Your obedient servant,

THOMAS FALCON.

Bradford, Jan. 20, 1838.

John Boardman first came under my observation in the spring of 1834, for a pulmonic affection, at which period the respiratory murmur was dull, accompanied by a harassing cough and dyspnoea. From these symptoms he rallied in the summer of 1834, so as to be enabled to follow his usual employment. This reprieve was, however, very temporary. In the beginning of September of the same year, he was seized with symptoms in their nature most appalling. Unable to respire except in a semi-erect position; his pulse weak, irregular, and intermittent; with a dull pain in the dorsal region, and also following the course of the axillary nerves.

The sensation of oppression in the precordial region he compared to the pressure of a sack of flour on the chest; he, however, referred his chief pain to

the epigastric region, which was very tender on pressure. He was unable to retain food upon his stomach, except of a farinaceous nature, and only in very small quantity. The urgency of the symptoms at this period indicated the presence of gastritis of an acute character. Under this conviction he was treated by leeches and counter-irritants to the epigastric region, and the other remedial means which are found serviceable in gastritis. The vomiting and pain were, however, in nowise relieved thereby.

Oct. 15th.—The paroxysms of embarrassment in his breathing are now truly urgent and distressing. The heart's pulsation cannot be felt on any portion of the left side of the chest, neither can the respiratory murmur be heard except upon the posterior part of the chest, near the vertebræ. He now informs me that the heart has always pulsated on the right side of the chest. Upon applying the hand to the part indicated, the heart's impulse is felt feeble and irregular. On applying the ear, the bellows murmur is constant and distinct.

These being the most prominent symptoms of the case, I would add, that the man lingered till April 1835, when he died. Upon opening the cavity of the chest, I found an immense tumor, weighing nine pounds, of a dark brown medullary substance, easily lacerable by the fingers, and surrounded by a fibrous capsule. Having taken its origin in the anterior mediastinum, it appeared gradually to have increased in size to the time of his death. The two-lobed lung was so compressed as not to be more than two inches in thickness; it pressed upon the bronchi above, as also upon the aortic arch, extending considerably into the right lung. The heart had evidently always been situate on the right side, and not pushed to its present situation by the tumor; for the aorta passed downwards on the right side of the vertebral column, the brachio-cephalic artery passed to the left, whilst the single carotid and subclavian passed to the right.

Although the continued gastralgia and vomiting were most probably the result of the pressure of the tumor upon the vagi nerves, yet the stomach was not found inflamed, an effect which experimentalists have produced by dividing these nerves. The lumbar glands

were enlarged. The thoracic and abdominal viscera were transposed.

The man had suffered amputation of the right testicle for fungoid disease, ten years previous to his death. The nature of the tumor in the chest was evidently fungus hæmatodes, affecting the remains of the thymus gland.

TRANSPORT OF THE WOUNDED.

To the Editor of the Medical Gazette.

SIR,

I SHALL feel myself obliged by your giving a place, in the next number of your journal, to the following short article.—I am, sir,

Your very obedient servant,

F. C. CHERRY,
Vet. Surg. 2d Life Guards.

Regent's Park Barracks,
Jan. 29, 1838.

IN the MEDICAL GAZETTE of the 20th January is an article on "Transport of the Wounded," by Mr. J. Rutherford Alcock, wherein my name is brought conspicuously forward.

I have not the honour of knowing Mr. Alcock, but I have attentively read his paper, and am at a loss to know what its real object is.

With the complaint he makes as to difficulties occasioned to medical officers by the regulations of the British service, I have nothing to do whatever; neither with the improvements "beneficial in the extreme," effected among "ward-masters, clerks, orderlies, &c." under the sanction of, not by, the inspector general.

But with regard to transport by wheel carriages, I must beg to say, that the time and attention necessarily devoted to the duties of hospitals and the sick, appear to have laid him open to misinformation and misconception in regard to transport and its organization; and to these carts of mine in particular.

I found my claim to entertain and express opinions on the subject of military transport, from very long experience, under circumstances extremely varied; and on industry in collecting information, under circumstances more favourable than falls to the lot of many to encounter.

These I reduced to a practical working state, and printed the result, more than ten years ago. The book is in the

hands of heads of department, and many others interested in its details, with experience competent to enable them to decide on its merits.

[In compliance with Mr. Cherry's request we have inserted this note. We leave the party attacked to answer it if he thinks proper; but as referring to Mr. Alcock's communication, he professes to be "at a loss to know what its real object is," we beg to inform him that one very distinct object was, to point out certain imperfections in Mr. Cherry's carts for transporting the wounded; and we know that Mr. Alcock (who has done this without any degree of disrespect or ill-nature) has excellent opportunities of judging.]
—ED. GAZ.

IRISH COLLEGE OF SURGEONS AND APOTHECARIES' HALL.

To the Editor of the Medical Gazette.

SIR,

A LETTER signed "A Professor in the School of the Apothecaries' Hall of Ireland," having lately appeared in your columns, I take advantage of your impartial intimation appended to it to offer a very few observations, rather in the way of comment than reply. The condition which you very fairly require of being furnished with my name, I have complied with in the accompanying billet.

The great bulk of the *soi-disant* Professor's letter being purely of a personal nature, does not, in my opinion, demand either notice or reply. When a crisis is at hand involving the interests of a numerous and important profession, and the welfare of the community, I, for one, am little disposed to hair-split the letters of an official oath for the purpose of proving or disproving the claims to the epithet of renegade, of a gentleman who has thought proper to unite himself in intimate relation with the bitter and avowed enemies of a College whose "reputation, honour, and dignity," he has solemnly sworn, "to the utmost of his power, to endeavour to promote." I, for one, would never think of sifting out the two grains of reasonable apology from the two bushels of wordy chaff in which the professor has conveyed his excuses for entering into a servitude which he obviously despises, nor would I have the cruelty to inter-

rupt the prosing lullaby with which he has sought to quiet his conscience, were it not that this same lullaby prosing, as it is, contains a *refrain* which has now become the watch-word of the professor's present taskmasters.

"That we may the better understand the question," says the professor, "I will state the case as it is. The apothecary is compelled, as well by the wants of society, as by the position in which he is placed by others rather than by himself, to act in the double capacity of the prescriber, and the dispenser of medicine." Here, sir, comprised in these few words, you have the real object of the *professor's* lengthy epistle, and now permit me to "state the case as it *really* is." The medical profession in Ireland has been hitherto entirely composed of physicians and surgeons, who are in the habit of practising only as prescribers or consultants, no class equivalent to the general practitioner or apothecary of England having ever been sanctioned by law or custom in this country. To meet the wants of the public, and to serve those physicians and surgeons practising as I have described, a body of apothecaries (in the true acceptance of that name) was created by Act of Parliament, in the year 1791, for the express purpose of preventing "*injury to the fair trader, the dis-appointment of the physician, and the imminent hazard of the lives of his Majesty's faithful and loyal subjects throughout the realm.*" Let it be here expressly remembered that the apothecary was not, either by the spirit or letter of the act, required to learn or authorised to practise medicine as the English apothecary was by the act of 1815; but was simply and purely required to "be well skilled in the preparing and compounding of medicines," and was in all respects identical with the chemist and druggist of England of the present day. That body, which was originally created under the sanction of the physicians and surgeons of Dublin, which was even furnished in its puny infancy with a local habitation by that College of Surgeons against which it is now struggling in ungrateful insubordination, differed in no respect except its incorporation from the druggists and chemists of England. Its members were educated solely with a view to the making them expert and skilful compounders of medicine, being required to serve an apprenticeship to the *mystery* of an apothecary,

and to be examined, at its termination, in the knowledge of the pharmaceutical art. For many years they professedly confined themselves to the duties of their respectable and useful occupation, to the great advantage of the community, and the comfort and satisfaction of the medical profession. That profession, however, having somewhat incautiously permitted their employment about the sick, in what they unwisely considered the menial offices of applying leeches, and administering clysters, &c., advantage was taken of the opportunity, and the apothecaries commenced at first to dabble in practice over their counters, and, at a still more recent period, to insinuate themselves by all arts into the confidence of such patients as they were allowed to approach under the circumstances first alluded to, and of course, *pari passu*, to remove from the same confidence the proper medical adviser. These steps were taken silently, and by slow—very slow gradations; but within the last year or two, having gained, as they supposed, a sufficiently strong position, the apothecaries have thrown off the mask, and having hired into their service, as teachers, certain physicians and surgeons (whose poverty, I hope, not their will, consented,) they have boldly set themselves forward, not merely as general practitioners, but as the sole licensers and instructors of that class of the medical profession.

In this very hasty sketch, sir, you will no doubt recognise an identity between the Irish apothecary and the English chemist and druggist. Let not the name of apothecary, by its different signification in England, blind your readers, nor prevent them from sympathising heartily with their brethren in this neglected country. Their cause is our cause, with this difference, that in England the druggist has not as yet ventured from behind his counter, while, in Ireland, he has already issued forth into practice, and even shews his inclination to be a licenser of medical practitioners. This difference may not long exist; the public in England, as here, know little of the real merits of their medical advisers, the value of education is by them little understood, but low charges, and mean servile and cringing habits, are comprehensible by all, and unhappily acceptable to too many.

This, sir, is "the real state of the case," and it is to relieve themselves from the cruel oppression and hardship

to which such a state of things subjects them, that members of the College of Surgeons in Ireland have determined upon praying the Legislature to remove from them the trammels of the Apothecaries' act, and of their own charter, and enable them to practise their profession with some honest advantage to themselves and safety to the public, by permitting them to supply their own medicines instead of trusting to the tender mercies of rival practitioners. This is what the College asks for; it does so in the conviction that it would be vain to seek the more desirable remedy of obliging the druggists to confine themselves to their proper business of preparing and compounding medicines, and I leave it with confidence to the profession to decide whether, in what the College is doing, it is desirous, as the professor has accused, it of throwing unjust odium upon any class of individuals, or of degrading itself and its members from the station they have hitherto held.

The professor threatens you with future communications respecting the Charter of the College, &c.: should they be merely requiems to his conscience, my time, I hope, will be better employed than in disturbing their anodyne effects; but in case of their containing calumnies against the College, to which I am proud to belong, I shall again trespass upon your kindness for an opportunity of exposing them.

I have the honour to be,

A MEMBER OF THE ROYAL COLLEGE
OF SURGEONS IN IRELAND.

Dublin, Jan. 23, 1838.

ANALYSES AND NOTICES OF BOOKS.

“L'Auteur se tue à allonger ce que le lecteur se tue à abrégier.”—D'ALEMBERT.

THE DISCOVERIES OF SIR CHARLES BELL.

Idea of a New Anatomy of the Brain.

By C. BELL. 1811.

On the Nerves, &c. &c. By C. BELL.

Transactions of Royal Society. 1821.

Ditto, ditto. 1822.

Journal de Physiologie. Par M. MAGENDIE. 1821 and 1822.

Anatomical and Physiological Commentaries. By HERBERT MAYO. Parts 1 and 2. 1822 and 1823.

In the course of our last volume we had occasion to bring forward the sub-

ject of Sir Charles Bell's observations on the nervous system, in illustration of the degree of confusion which occasionally arises in the history of discoveries in medical science. The question of originality has of late been agitated in a new quarter—between Mr. Whewell, the learned author of the *History of the Inductive Sciences*, on the one hand, and the writer of a critique on that work in the *Edinburgh Review* on the other. We will first present (partly in a chronological form) the principal features of the writings of the several authors who are most interested in the question, and will then show what appear to us to be the just conclusions to be drawn from the evidence they afford.

1811. “*Idea of a new Anatomy of the Brain*, by C. Bell.”—This was a small work which the author did not publish, but merely distributed among his most intimate friends, containing, as he has often said, the fundamental principles of his system. We extract the following as the most important passages:—“In opposition to these opinions (the prevalent ones of the day,) I have to offer reasons for believing that the cerebrum and cerebellum are different in function as in form; that the parts of the cerebrum have different functions, and that the nerves, which we trace in the body, are not simple nerves possessing various powers, but bundles of different nerves whose filaments are united for the convenience of distribution, but which are distinct in office as they are in origin from the brain.—That the external organs of the senses have the matter of the nerves adapted to receive certain impressions, while the corresponding organs of the brain are put in activity by the external excitement; that the idea or perception is according to the part of the brain to which the nerve is attached, and that each organ has a certain limited number of changes to be wrought upon it by the external impression.—That the nerves of sense, the nerves of motion, and the vital nerves, are distinct through their whole course, though they seem sometimes united in one bundle, and that they depend for their attributes on the organs of the brain to which they are severally attached.”

Speaking of the experiments to prove these points, and the impossibility of making them on the brain itself, he says, “I took this view of the subject.

The medulla spinalis has a central division, and also a distinction into anterior and posterior filaments corresponding with the anterior and posterior portions of the brain. Further, we can trace down the crura of the cerebrum into the interior fasciculus of the spinal marrow, and the crura of the cerebellum into the posterior. I thought that I might have an opportunity of touching the cerebellum, as it were, through the posterior portion of the spinal marrow, and the cerebrum by the anterior portion. To this end I made experiments, which, though they were not conclusive, encouraged me in the view I had taken.

I found that injury done to the anterior portion of the spinal marrow convulsed the muscles more certainly than injury of the posterior portion, but I found it difficult to make the experiment without injuring both portions. Next, considering that the spinal nerves have a double root, and being of opinion that the properties of the nerves are derived from their connexion with the nerves of the brain, I thought that I had an opportunity of putting my opinion to the test of experiment, and of proving that nerves of different endowments were in the same cord, and held together by the same sheath. In laying bare the roots of the spinal nerves, I found that I could cut across the posterior fasciculus, which took its origin from the posterior portion of the spinal marrow, without convulsing the muscles of the back; but that on touching the anterior fasciculus with the point of the knife, the muscles of the back were immediately convulsed. Such were my reasons for concluding that the cerebrum and cerebellum were parts distinct in function, and that every nerve possessing a double function obtained that by having a double root."

Then, having pointed out the distribution of different nerves in the same organ for the performance of different functions, as the 1st and 5th in the nose; the 5th and 7th in the head and face; the 3rd and 5th in the orbit; the 5th and 9th in the tongue—"no part is sufficiently supplied by one single nerve, unless that nerve be a nerve of the spinal marrow, and have a double root, a connexion (however remotely) with both the cerebrum and cerebellum." "From the crura cerebri, or their prolongation in the anterior fasciculi of the spinal marrow, go off the nerves

of motion; but with these nerves of motion, which are passing outwards, are nerves going inwards—nerves from the surfaces of the body, nerves of touch, and nerves of peculiar sensibility having their seat in the body and viscera."

1821.—A paper "On the Nerves," &c. read before the Royal Society, July 12, 1821, and published in their Transactions a few months after:—"No organ which possesses only one property or endowment, has more than one nerve, however exquisite the sense or action may be; but if two nerves, coming from different sources, are directed to one part, this is the sign of a double function performed by it. If a part, or organ, have many distinct nerves, we may be certain that, instead of having a mere accumulation of nervous power, it possesses distinct powers, or enters into different combinations, in proportion to the number of its nerves." "When an animal is endowed with mere sensation and loco-motion, where there is no central organ of circulation, and no organ of respiration but what is generally diffused over the frame, the nerves are extremely simple; they consist of two cords running in the length of the body, with branches going off laterally to the several divisions of the frame." "There is the same uniform and symmetrical system of nerves in the human body as in the leech or worm, although obscured by a variety of superadded nerves," which "do not destroy but obscure that system; and accordingly, when we separate certain nerves, the original system of simple constitution is presented even in the human body. The nerves of the spine, the tenth or sub-occipital nerve, and the fifth or trigeminus, of the system of Willis, constitute this original and symmetrical system. All these nerves agree in these essential circumstances: they have all double origins; they have all ganglia on one of their roots; they go out laterally to certain divisions of the body; they do not interfere to unite the divisions of the frame; they are all muscular nerves, ordering the voluntary motions of the frame; they are all exquisitely sensible, and the sources of the common sensibility of the surfaces of the body." Then having spoken of the general characters of the respiratory superadded system of nerves, he passes to the relation of the

well-known experiments on the nerves of the face, from his conclusions on which we extract the following:—“The portio dura is the principal muscular nerve of the face; it supplies the muscles of the cheek, the lips, the nostrils, and the eyelids; that is, it is a nerve which orders all those actions which are in the remotest connexion with the act of respiration.” “These facts regarding the lips are of difficult explanation, until we consider what is necessary to the simple act of feeding. When a horse gathers the oats from the hand, or from the ground, he must feel the food, which is the office of the branches of the fifth; he must move his lips under the direction of that feeling, or he cannot gather it. It was accordingly discovered by experiment, that whether the seventh or the fifth were cut, if the operation were done on both sides of the face, the creature was deprived of the power of feeding, but from different causes; for in the first experiment it was owing to the loss of motion, and in the second to the loss of sensation. I am unable to decide whether or not the muscular branches of the fifth nerve go exclusively to the muscles of the jaws. I have found in an individual, that when the cheeks and lips were twisted by paralysis, he possessed the power of holding with his lips, in a manner that indicated a power independent of the seventh. But that the whole sensibility of the head and face depends on the fifth pair there is every possible proof.” “The fifth nerve, the nerve of mastication and sensation.” “The disease of the bone at one time affecting the fifth nerve, and producing excessive pain without paralysis; at another affecting the seventh nerve, and inducing paralysis without pain, are phenomena now accounted for.”

It is unnecessary to quote further from the paper read before the Royal Society, in May 1822, where Sir C. Bell endeavoured to prove the fact of branches of respiratory nerves being distributed to all the muscles of the trunk concerned in respiration, as he had in the previous one seemed to shew they were sent to the corresponding muscles of the face. He considered all the respiratory nerves, viz. the fourth, the portio dura, the glosso-pharyngeus, the vagus, the accessorius, the phrenic, and the external respiratory, and pro-

bably the intercostal and lumbar in part, to arise from a separate column of the spinal axis, between the anterior and posterior, and to be superadded to the nerves of sensation and voluntary motion, with which the muscles were supplied for common purposes. It is only to be observed, that the object of these papers was solely the illustration of the supposed system of respiratory nerves, and not the exposition of the whole of his system; and that they were the two first of a series which he had intended to present in regular order, when he was somewhat diverted by publications of which we shall now take notice.

1821.—M. Magendie, in his *Journal de Physiologie* for October. “He (Mr. C. Bell) has undertaken to show that the nerves have different functions according as they arise from such or such a part of the brain, or of the spinal marrow.” He then gives an abstract of the paper above quoted from, as *verbally* detailed to him by Mr. John Shaw, who visited Paris at that time, and says, “We have repeated these experiments (on the nerves of the face) at the Veterinary School of Alfort, with MM. Shaw and Dupuy, and the result which we have obtained agrees perfectly with what we have related, with the exception, however, of the influence of the division of the infra-orbital on mastication, an influence which was not evident to me.”—p. 387.

1822, July.—M. Magendie, *Journal de Physiologie*, vol. ii. p. 276. Having detailed very briefly his first experiments on the roots of the spinal marrow, “it is sufficient for me to be able now to advance as positive, that the anterior and posterior roots of the nerves arising from the spinal marrow have different functions; that the posterior appear more particularly destined to sensibility, while the anterior seem more especially connected with motion.” And in October of the same year, in the next number of his journal, having related additional experiments with *nuxvomica*, galvanism, &c.: “These facts are confirmatory, therefore, of those which I have announced, only they seem to establish that sensation is not exclusively in the posterior roots, nor motion in the anterior.” In the period between the publication of these two papers, Mr. John Shaw had sent him Sir C. Bell’s brochure of 1811, and had marked against the passage we have quoted,

about the experiments on the roots of the nerves. He says, "I thought I was the first who had dreamed of cutting the roots of the spinal nerves;" but adds, "we see, by this quotation from a work which I could not have known, since it had never been published, that Mr. Bell, led by his ingenious ideas on the nervous system, was very near discovering the functions of the spinal roots; however, the fact that the anterior are destined to motion, while the posterior belong more particularly to sensation, appears to have escaped him: it is, therefore, to the having established this fact in a positive manner, that I ought to limit my pretensions."

In 1825 M. Magendie expressed the same opinion in the "*Anatomie des Systèmes Nerveux*," by himself and Desmoulins. "We obtain, in fact, contractions by galvanizing each root. But the contractions are generally stronger and more complete with the anterior than with the posterior roots;" "prickings and pinchings (of the anterior roots) produce more strong and convulsive contractions, while there are scarcely any signs of pain. The isolation of the two properties in each of the two orders of roots, is, therefore, not absolute."—Vol. ii. p. 778. And even as late as 1833, when the third edition of his "*Précis Élémentaire de Physiologie*" was published, after Müller's experiments, he speaks, scarcely more positively.

1822, October.—Mr. Mayo, *Anatomical and Physiological Commentaries*, No. 1. "I infer, from the preceding experiments, that in the ass, the portio dura is a simple nerve of voluntary motion; and that the frontal, infra-orbital, and inferior maxillary, are nerves of sensation only, to which office that branch of the fifth which joins the portio-dura probably contributes; and from the preceding anatomical details, that other branches of the third division of the fifth are voluntary nerves to the pterygoid, the masseter, the temporal, and buccinator muscles."—Page 112.

1822, October and December.—Mr. John Shaw, *Medical and Physical Journal*. After pointing out the analogies between the fifth and spinal nerves, in structure, arrangement, and pathology, in a very lucid manner, "I cut a branch of the fifth upon the face; the sensibility of the corresponding side of the lip was destroyed;

but little paralysis ensued, excepting of certain actions of the orbicularis oris. I cut the nerve nearer the brain, and at a point previous to its having given off the branches to the muscles; then the jaw fell, and the muscles of that side were powerless. I varied the experiment by irritating the nerve, where it lies in the spheno-palatine fissure, immediately after an animal was killed; the jaws then came together with much force, indeed, so as to nip my assistant's finger severely."

1823, July.—Mr. Mayo, *Commentaries*, No. 2, corrected his former observation on the motor nerve of the buccinator, finding that "pinching the branch of the fifth, which penetrates that muscle, produces no action in it," page 8; "by this analogy (of the fifth with the spinal) I was led to conjecture that the double roots of the spinal nerves have functions corresponding with those of the fifth." "When I was engaged in experiments to determine the fact, M. Magendie's were published, which establish the justness of my conjecture."—Page 10.

1831.—Müller, in the "*Annales des Sciences Naturelles*," and 1834, "*Physiologie des Menschen*," p. 627. A series of experiments completely determining the entire isolation of motor power in the anterior, and sensitive in the posterior roots, the experiments of Federa, Schoeps, and others, having left the question in the same degree of uncertainty as those of Magendie.

The several passages we have now quoted have been selected as least capable of misinterpretation, and as including in the least space possible the principal features of each work. We have only room for the conclusion, which we believe they fairly lead to, viz. that to Sir Charles Bell is entirely due the honour of having proved (what had been imagined previously on insufficient grounds), that no one nervous filament is capable of performing more than one function; and for having first suggested and established the fact, to use Mr. Whewell's words, "that the two offices of conducting the motive impressions from the central seat of the will to the muscles, and of propagating impressions from the surface of the body and the external organs of sense to the sentient mind, reside in two distinct portions of the nervous sub-

stance." We believe that no one previously ignorant of the subject, but capable of judging of it, could read Sir Charles Bell's works published before any others, without being convinced of these important facts; and that he had carried his investigations almost to completion, is sufficiently proved by the subject being nearly perfected in two years after his first paper was read at the Royal Society.

Of those by whom facts supporting the doctrines were confirmed, improved, and supplied, we would accord the first honour to Mr. Mayo, for his experimental proofs of the facial branches of the fifth being purely sensitive, and his anatomical proofs of the motor power of its non-ganglionic portion; and, when the respiratory system is satisfactorily proved to be imaginary, for his arguments against it. The next place we think belongs to Müller, for the first clear experimental proof of the complete isolation of the motor and sensitive powers in the anterior and posterior roots. Next after Müller, credit is due to Magendie, for his approximations to the experimental proofs of this isolation, and of the absence of motor power in the infra-orbital nerve; nor must we omit our tribute to Mr. John Shaw, for his experimental proof of the motor power of the non-ganglionic portion of the fifth, and the numerous very important dissections and cases which he supplied.

MEDICAL GAZETTE.

Saturday, February 3, 1838.

—
 "Licet omnibus, licet etiam mihi, dignitatem
Artis Medicæ tueri; potestas modo veniendi in
 publicum sit, dicendi periculum non recuso."
 CICERO.

LAPSE OF LEGACIES FROM INSANITY.

WE have had, in our recent numbers, to call the attention of our readers to an unfair attempt judicially to place the person and liberty of a fellow-creature under the control of others, and his property, consequently, out of his own management. The attempt, as we have shewn, happily proved abortive; and

we should have been glad if another case we then cited, (and probably many more might have been adduced,) had been equally well adjudged. We shall, at all times, seek to expose these efforts to possess the control of the property of others by such means; for we consider it our province, and our duty, to watch every legal proceeding connected with insanity—indeed, the insane are mainly dependent on the medical profession for their comfort and protection. Insanity is, perhaps, the greatest misfortune to which society is liable, and the most distressing that can befall a family; for should the leading member of it be thus afflicted, all his exertions for its support are paralysed, and he becomes a burden on its resources; an object of incessant care and solicitude; and his children may eventually lose his property, from his inability adequately to dispose of it.

When we offered some remarks in a former number*, and an "Abstract from the recent Will Act," it did not at the time strike us, that the consideration of the Wills of Lunatics constituted no part of that act of legislation. The want of due arrangement for such has occasionally been a source of great injustice with regard to the descendants of lunatics; and we will now point out the relative advantages or situations of the sane and the insane, in reference to the provisions of the new Will Act, and take the liberty to suggest some amendments for the consideration of the legislature, to supply the existing deficiencies.

And we may without impropriety premise these, by some observations of the Commissioners appointed to inquire into the law of Property, who, referring to the subject of *Lapse*, say "the rule that gifts lapse, if the person to whom they

are made dies in the lifetime of the testator, sometimes operates with great hardship, and defeats, in many cases, the intention of the testator." And that the framers of the new Will Act, and the legislature, by whom that act was sanctioned, considered the lapse of a gift by will to a *child*, from the death of that child in the lifetime of the testator, to be, in all cases where such child left a family, a great hardship, and contrary to the probable intention of the testators, is obvious, since that Act provides, "that in all wills executed after the 1st of January, 1838, *no lapse shall take place in such cases; but that the gift shall take effect as if the child had survived the testator, unless a contrary intention shall appear upon the face of the will.*"

Thus far the public generally is protected; and every person not labouring under disability, can, and probably will, re-execute his will after the 1st of January, 1838, so as to take advantage of the Act. But how can the lunatic re-execute *his* will, so that his family, also, may obtain a similar or any advantage from the Act? His will having been made while in a state of sanity, contained dispositions of his property, suitable to the then existing state of his family; but many may, from subsequent circumstances, be deprived of his intended bounty. For instance, where the testator has, by his will, divided his property among his children, giving to each an equal share, his probable intention at the making of the will was, that the share of each child should be a vested interest, and descend to the issue of that child. Should the testator subsequently become insane, and, after this event, should one of his children die during his lifetime, leaving a family, then the intended gift to that child lapses, and the grandchildren lose their parent's share; for the testator, who, when

he made his will, never contemplated the event of his not having, at any time, the power to alter it, is now deprived of that power, and cannot meet the contingency which has occurred. Under such circumstances his probable and implied intentions are frustrated, by a cause which is, of itself, a sufficient affliction to his family, without the addition which the loss of the property necessarily carries with it.

With reference to cases similar to the above, the Commissioners, in the Report before quoted, state, that "it is true that the event of death might always be provided for,"—that is, it might at the time of making the will: "but it is found in practice that such provision is very rarely made. A testator does not contemplate that the immediate objects of his bounty, and especially his children, will die before him; he does not like to encumber his will with provisions which appear to be unnecessary; and he imagines that, if the event should happen, he shall be able to alter his will. His legal advisers think the chance that such an event will happen, without being provided for, is too slight to compensate the trouble of inserting clauses to meet it; and, in truth, it would often be difficult to determine how far such provisions should be carried.

It must be allowed, that a future disability from *insanity* is one that is not likely to occur to the testator at the making of his will, and can hardly be suggested by the legal advisers.

But if the lapse of gifts by will to children, as alluded to, was a hardship requiring the interposition of an act of parliament, to provide a remedy for the injury which might be sustained; even although such injury might arise from procrastination, indifference, or neglect, on the part of the testator; it would appear much more imperative on the legislature, to give relief in

situations which arise from a misfortune, over which no human power has any control. This might, perhaps, be best obtained by some provision to the following effect:—To provide, by an amendment in the new Will Act, or otherwise, as may seem expedient to the legislature, that in all cases where persons of unsound mind, *living on the 1st January, 1838*, shall have duly made and executed their wills, at some period previous to such unsoundness of mind, such wills shall be taken and construed as if they had been duly re-executed on the 1st January, 1838, by testators in full possession of their mental powers; so as to bring those wills also within the provisions of the Act. Without this, should any child of such testators die during their lifetime, leaving a family, the issue of that child will lose their grandfather's bounty, because they have had the misfortune to lose the care and protection of their parent; a much greater hardship by the way than any that the Law Commissioners, in their Report on Wills, have alluded to.

As cases may occur where the provision suggested may not afford sufficient protection, it would appear that a more comprehensive and effectual remedy would be afforded by providing, that where a testator has become insane subsequently to the execution of his will, and has continued so to the time of his death, his will shall be considered as speaking and taking effect from the time of the *commencement of his insanity*; a period always readily determinable by legal inquiry. As from that time the capability of the testator to provide for contingencies ceases, as completely as though he were absolutely no longer in existence, and as he is, in both a moral and legal sense, from that period, *dead*:—the property should be used to administer to his comforts, &c.; and at his actual *death*, his property

should be distributed as though it had been coeval with his moral or legal death. By these means any contingencies occurring subsequently to the latter event, will have been provided for by *himself*. Thus, should a child of an insane person have died (leaving a family) at any time subsequent to the commencement of the insanity of the parent, the legislature having so provided for the due fulfilment of the *sane intentions* of the testator, by amending the new Will Act, or otherwise, the issue of such child will be insured that proportion or share of the testator's property, which he meant by his will should devolve upon them. Provision, however, should be made for such as may come into existence between the events of the legal, and the actual, death. But if the law be left as it is at present, lunacy—the most afflicting misfortune of our common nature—will remain, still, to be very often a doubly dreadful blow to a family; for a portion of the unfortunate sufferer's descendants may thereby be bereft of his intended bounty towards them, and that, from a deficiency of our legal code which might be removed.

It is difficult, and often improper, to legislate retrospectively, and nothing but the necessity of distributing justice impartially to all classes and conditions of the community, perhaps, can warrant it. But to accomplish this, is the object and duty of legislation:—and if so, why are lapses in the wills of *lunatics* permitted still to take place, when, in those of sound minds, the legislature has declared lapses to be unjust, and has accordingly provided that they may not arise? This is not legislating impartially for all conditions of the community—not extending an equal guardianship over the interests of all positions of society.

The law very properly takes the persons and properties of lunatics under

its especial care; but has left the children of lunatics, as far as that property (when a will has been made prior to mental alienation) is concerned, to the common chances of survivorship, and absolutely unprotected by any law whatever; and further, the children have no means of protecting themselves, since the life of a lunatic is not insurable. They have, therefore, no hope for an amendment of their cruel and inequitable position, but from the future wisdom and provisions of the legislature.

It may be said, that we have no proof that these lunatics would have altered or re-executed their wills had they retained their mental powers: on the other hand, we have no better proof that they would not have done so; and both probability and experience teach us that the omission, on the part of the testator, to alter his will on the death of a child, leaving a family, arises almost always from procrastination, neglect, or ignorance, and very rarely from design; and the legislature having decided that it is unjust that such gifts by will to children should lapse by death, it would be an act of inconsistency to leave the issue of the children of one class of persons to their fate—perhaps to ruin, while those of all other classes are taken under the protection of its wisdom and care. This situation of the issue of children of lunatics is one of peculiar hardship as to the construction of the existing law in their case; for the lunatic, as far as regards his inability to alter or re-execute his will, in their favour, is taken to be as incapable as if he had died immediately upon the making of that will, when the objects of his bounty were alive; but as regards the causing a lapse, or forfeiture of their interests under the will, he is taken to have survived them.

We shall only add, in conclusion, that the hardship above alluded to is

not imaginary, but exists in the case of one of our brethren, whose family suffers from the "lapse" we have described*.

INQUEST

INVOLVING "A VERY NICE POINT."

A REMARKABLE inquest was held at Norwood, in Surrey, on the 26th of January, on the body of a boy named Henry Bailey. The gist of the case may be comprised in a small compass. The boy, whose age is not stated, had been in Lambeth Workhouse, where he was flogged by a Mr. Rowe with such severity, that the body, when viewed by the jury, "bore evident marks of blows, the back, thighs, legs, and arms, being nearly covered with black marks: there was also a bruise on the forehead."

Bailey had been removed to the House of Industry at Norwood, for the infant poor of the parish of Lambeth, on the 13th of January, and died there on the 19th. Now as it appeared on post-mortem examination that there was disease of the lungs, it would be difficult to say that this punishment had absolutely caused the death of the child, but it is at any rate obvious that it must have accelerated it. This plain fact did not escape Mr. W. Street, a surgeon of Norwood, whose evidence concludes thus:—

"There were also marks of a buckle on the hip, as if deceased had been struck with a strap that had a buckle at the end of it. The blows, in the state of health in which deceased was, would have caused much constitutional disturbance, but would not of themselves have caused death."

On this the following curious dialogue ensued:—

* There are one or two sentences of the above taken from a little brochure lately sent to us, but which, having been mislaid as this is passing the press, we are unable to designate the quotations in the usual form, by inverted commas.—ED. GAZ.

"The Coroner.—That is not the question. Do you mean to say that those blows would have produced death sooner than it would otherwise have taken place?"

"Mr. Street.—They might have hurried him into fever, and rendered him less able to bear up against the complaint under which he laboured."

"Coroner.—Suppose a person stood before us charged with having inflicted those blows, would you take upon you to say that their effect would be to accelerate the death of the deceased?"

"Mr. Street (after some consideration).—*That is a very nice point, and I would rather not answer it.*"

"The Coroner.—It is a nice point, and it is for that reason that I am anxious to obtain your opinion, as well as that of the other gentleman who examined the body."

"Mr. Street.—I repeat, that it is a very nice and delicate point, and, if pressed, I must say that, under all circumstances, I would prefer answering in the negative."

Surely this is sad stuff. The severe whipping which would cause "much constitutional disturbance" in a child labouring under disease of the lungs, must inevitably hasten death; and the witness, having confessed that the blows would have caused this, was not justified in endeavouring to back out of the necessary consequence. Perhaps he did not know, while giving his evidence, that the chief perpetrator of the deed was beyond all human censure, having died on the very morning of Bailey's removal to the house at Norwood. But though this will account for the witness's finding "a nice and delicate point" in his own imagination, it will not excuse it. The really nice and delicate points to decide would have been—1st, whether such a punishment would not have endangered the life of a healthy child; and, supposing this question to be decided in the negative, 2dly, whether the inflictors knew that the child was seriously ill when they so misused it? If they did, it is clear

that they were guilty of manslaughter, not to say of a graver crime. If they did not, under what kind of inspection, medical or other, are the children in Lambeth Workhouse?

These are really difficult questions; but when Mr. Street thinks it "a very nice point" to decide whether the death of a patient with pulmonary disease is hastened by his being whipped till his back, thighs, legs, and arms, are nearly covered with black marks—till there is a bruise on his forehead, and the mark of a buckle on his hip—we must apply an old adage to him, and say, *notum in scirpo querit*.

Another practitioner was now called, whose evidence, in the brief report before us, appears only to be the echo or faint reflection of Mr. Street's. Like him, he thought the disease of the lungs sufficient to account for death; and, like him, he could not tell whether the blows had accelerated it.

If any one wishes to know the cause of the disease of the lungs under which, with the help of the floggings, the poor child sank, he will not find it a very "nice and delicate point" to determine. The deceased, with his father and brothers, being homeless, had been received into Lambeth Workhouse, but were turned out again to shift as they could, and slept several nights in a stable. Those who do not practise the art mentioned in the adage above quoted, of finding knots in bulrushes, will allow, we suppose, that sleeping in a stable in January, and such a January as that of 1838, might *possibly* produce inflammation of the lungs. The jury said that, after the opinions of the surgeons, they could not go much further; they did not, however, appear quite satisfied. They at length returned a verdict, "that the deceased died of disease of the lungs."

This inquiry, if it can be called one, was badly managed in many particu-

bars. A juror wished to examine the father and brother, who, however, were not called; and the medical evidence will, we fear, be cited by those who are apt to say on points of forensic medicine, that "they manage these things better in France." In such a case as this, it was imperatively required that the professional witnesses should not only be free from all local partiality or bias, but from all suspicion of it; and therefore the services of some distinguished London surgeon should have been procured. Mr. Watmore, indeed, the clerk to the Lambeth Board of Guardians, stated not only that this inquiry was instituted at their particular desire, but that they had directed a post-mortem examination by two most eminent surgeons. Neither their names nor their opinions, however, are to be found in the report.

CLINICAL LECTURE

ON

SYPHILIS.

Delivered at St. Bartholomew's Hospital.

BY WM. LAWRENCE, ESQ. F.R.S. &c.

The Venereal Poison does not always produce the same effects—Case of Acute Phagedæna—Case of Sloughing Chancre—Remarks upon these Diseases—Effect of Intemperance and Licentiousness—Treatment to be employed.

I CALLED your attention, gentlemen, in the last lecture, to four cases of primary syphilitic ulcers, then in the hospital. All the sores possessed strongly marked characters, and no two were alike. We thus learn that the venereal poison, whatever be its nature, does not produce one unvarying set of effects. Further evidence of similar nature might be adduced in corroboration of the same conclusion. Thus, the first effect of the venereal poison may be an ulcer quite superficial, or a mere excoriation without loss of substance. Many consider that such appearances are not syphilitic: I regard them just as much so as the other forms. They occur from promiscuous sexual intercourse, and are not seen in married life where the parties strictly observe the marriage vow, nor in such as live chastely. They give rise to secondary syphilitic symptoms; and thus it happens that we see cases of the latter

where the patient says that he has had no venereal disease. I was consulted, not long since, by a gentleman with an attack of acute syphilitic iritis, of which the characters were so well marked, that I immediately told him he had venereal disease of the eye. He said this could not be, for he had never contracted a venereal affection. On further examination I found that he had had what he called a rash over his body; that it had lasted three or four months, and that there were still some remains to be seen; these were small patches of scaly syphilitic eruption. It appeared further, that some time previously to the rash he had had a slight excoriation in the penis, which did not heal completely for a few weeks. He had consulted a medical practitioner respecting the latter, who told him that it was a mere excoriation without any venereal character, and not requiring any particular treatment. The crisis in this case required active treatment, and the organ was not completely restored until the use of mercury, carried to the extent of affecting the glands, had been persevered in for four weeks. If the character of the primary affection had here been inferred from those of the iritis and eruption, we should have argued that it must have been decidedly syphilitic.

Whether this gentleman would have escaped without secondary symptoms, if the excoriation, as it was called, had been treated with mercury, must be uncertain—I think he would have had a better chance. If, however, he should hereafter experience further secondary symptoms, such as sore throat, affection of the periosteum, disease of the nose, he will not estimate very highly the knowledge and discrimination of the person who pronounced the primary complaint not to be venereal.

[No further symptoms have ensued in this case: at the end of eight months from the treatment of the iritis, the gentleman remains perfectly well.]

I placed before you, in the last lecture, a case of chronic phagedæna; that is, of an eating ulcer which proceeds slowly; which had lasted eight weeks without causing serious or extensive damage. Phagedænic ulceration may proceed more rapidly; it may destroy the glands in eight days. I will present you with an example of acute phagedæna. A gentleman, twenty-two years of age, observed an unpleasant appearance on the glands ten days after a suspicious connexion. For this he consulted a medical practitioner on a Thursday, and did not see him again till the following Sunday, pursuing his ordinary avocations, and living as usual in the meantime, the weather being very hot. I saw him on the following day (Monday), when his medical attendant informed me that

the complaint had consisted on the Thursday of a black spot; that it had increased fearfully by Sunday, when he recommended that I should be immediately consulted. I found a phagedenic excoriation, which had destroyed one-fourth of the glans on its left side, bounded anteriorly by the frenum. This alarming sore had an irregular surface, with sharp ragged edge; it was of a dirty yellowish and grayish colour, intermixed with bloody spots; and it secreted an offensive ichor, with strong and characteristic fetor. The glans was swelled and bright red. The prepuce could be easily withdrawn so as to expose the sore, which was painful. There was slight inflammatory tumefaction of the penis generally, which might be owing partly to the excitement and partly to exertion in hot weather. Eighteen leeches were applied to the penis, and afterwards bread poultice; black wash to the sore. The bowels were cleared by an active aperient, and subsequently he was ordered to rub in a drachm of the strong mercurial ointment every night, and to take two grains of calomel with one-third of a grain of opium every eight hours. He was confined to bed. The circumstances of his domestic position, the painful nature of the complaint, which compelled him to observe perfect rest, and the severe constitutional disturbance, aggravated by high atmospherical temperature, prevented the use of the frictions; the calomel and opium were therefore given every four or six hours. On Wednesday evening he was in a high fever, the small upper apartment which he occupied being as hot as an oven. I bled him largely from the arm, and found it necessary to substitute an evaporating lotion to the penis for the poultice, of which he could not bear the weight. Two days after he was removed to another residence, where he had a cool and commodious apartment. The action of the mercury was slow in this instance; it did not cause swelling of the gums nor ulceration, but it rendered them white, as if they had been covered by a thin adventitious stratum, and it caused moderate pytalism. The progress of the complaint seemed checked in the first twenty-four hours; but it was not decidedly arrested until about the seventh day. In the meantime it extended deeply, by a sloughing process between the glans and the corpus cavernosum penis; it destroyed the frenum, and shewed itself to a small extent on the right side of that fold, but it did not enlarge beyond its original boundary on the left of the glans, and it left the prepuce entire. When the slough separated it had gone so deeply as to open the urethra to the extent of about an inch on its upper and left side. When the surface

had been cleared the sore healed rapidly; and, as the prepuce was entire, while the contraction of the cicatrix lessened the apparent loss of substance in the glans, not only was the part again capable of all its functions, but its form had suffered very little damage. The mercurial influence was kept up until cicatrization was complete—a period of about four weeks. This is an example of primary sore spreading rapidly by destructive ulceration, which would, probably, have destroyed the glans in four or five days from the time I first saw it, if left to itself. The treatment demonstrates, unequivocally, the powerful influence of mercury over such affections.

[No secondary symptoms have followed in this case; the patient has continued perfectly well.]

I proceed to explain to you the nature and treatment of another form of primary syphilis—*sloughing chancre*; an affection in some respects more formidable than the preceding, the acute phagedenic sore. A lad of sixteen, who had recently left the country, contracted syphilis soon after he arrived in town. He immediately consulted a general practitioner, who observed nothing alarming in the case. This was on a Tuesday, and he was seen again on the following Saturday. In the meantime he had been going into the city daily, from a distance of three miles, and returning, had been actively employed in business during the day, and living in his usual manner, in the family with which he resided; the weather being very warm. The penis was now swelled, red, and painful; the orifice of the prepuce contracted, and there was a degree of feverish disturbance, rendering him incapable of further exertion. Depletion by resection and leeching, with rest in the horizontal position, and other corresponding antiphlogistic measures, were immediately resorted to, and followed up effectively. The local mischief, however, seemed to advance, and loss of the penis was feared. I saw the patient on Wednesday, when he was in a state of the greatest discomfort, from severe bodily and mental suffering. The penis was swollen generally, the prepuce more particularly, while a thin, bloody, and offensive discharge, with the peculiar fetor characterizing these sloughing sores, issued abundantly from the contracted orifice of the latter. The part was so extremely painful that he could not bear the slightest touch. He was enfeebled by the depletion he had undergone, and by the entire want of rest, caused partly by constant pain, partly by mental anxiety and depression, with apprehension that he might lose the member entirely. I divided the prepuce in its whole length

along the upper surface of the penis, finding that the most marked relief is experienced in such cases when the inflamed and ulcerated glans is liberated from the pressure which is caused by the swollen prepuce. Free bleeding ensued, as it usually does, from the edges of the incision. The relief thus afforded by unloading the vessels immediately engaged in the inflammatory mischief, is an important source of additional benefit in this mode of proceeding. One half of the glans had perished, and was converted into a dark greyish slough, separated from the living parts by a distinct line. Poppy fomentation and soft bread poultice constituted the local treatment, and the muriate of morphine was given at night to procure rest. On the following day he was much better; the division of the prepuce had been followed by immediate and effectual relief to the part, and the opiate had procured a good night. The slough was separating. In two days' time the separation had advanced considerably, leaving a rather foul and ragged surface, to which lint, dipped in the balsam of Peru, was applied. In two days more, the surface had become quite clean, and the balsam was left off, as its application caused pain. Soon after, the mortified part became entirely detached; although the slough extended deeply between the glans and the body of the penis, the urethra escaped. The sore now healed rapidly. The measures of general treatment, after the prepuce had been divided, consisted in a nutritious diet, with porter and wine, and the sulphate of quinine, in two-grain doses, three times a day.

[No secondary symptoms have ensued in this case.]

This and other analogous cases show that the venereal poison may cause, not only inflammation, increased and altered discharge, thickening and various modifications of ulceration, but also loss of vitality more or less considerable. The mortified part is separated by the same process as in other cases, the surface left after such separation being perfectly healthy, and usually granulating and cicatrizing rapidly. An ingenious person has written a book to prove "the non-existence of the venereal virus." We will not dispute about words; but we may be permitted to ask, if there is no venereal poison, how it happened that the first and sole amour of a healthy youth, aged sixteen, should have caused the serious mutilation of which I have given you the history?

In respect to the division of the prepuce—a strikingly beneficial part of the treatment in these and other similar conditions of the glans and prepuce—fear has been entertained that the discharges from the

diseased surfaces might contaminate the edges of the incision. This apprehension is altogether unfounded; in a large number of cases, where the proceeding has been adopted in this hospital, no such result has ever been noticed.

I have seen lately a greater number than usual of these serious phagedenic and sloughing diseases; some having occurred in private practice, where they are met with more rarely than in the wards of an hospital. These private cases are particularly interesting in elucidating the natural history of the disease; we can trace out the history more satisfactorily, and can ascertain that the distinguishing features of the complaint are not produced by causes which may be supposed to exert an influence in many hospital cases—such as intemperance, neglect, and perseverance in profligate licentiousness, after disease has been contracted.

I was consulted not long ago by a patient, 64 years of age, who said that he wished to have my opinion respecting an appearance on the penis. As he was preparing to shew the complaint, I anticipated the nature of the affection from a most offensive smell proceeding from the part. He drew back the prepuce without difficulty, and exposed a slough of the glans as large as a shilling, partly yellowish, partly black, discharging a thin fetid ichor. He did not relate how the affair had occurred, and it was not expedient to question him on the subject. I found that he had shewn it in the morning to his medical attendant in the country, and had been induced by his report to come to town, where I saw him in the evening. There was little or no swelling of the penis, and not much pain. I directed him to return home immediately; to go to bed, and remain there; to apply as many leeches as he could place on the penis; to use poppy fomentation and bread poultice, and take opening medicine. I told him that swelling and pain might be expected, and that a division of the prepuce would then be necessary. I did not see him again till the end of two days, when he was considerably worse. The penis and prepuce were swelled, but not considerably; the latter could only be withdrawn with difficulty. He had been in great pain for the last twenty-four hours, and had not slept during the preceding night. I divided the prepuce in its whole length. There was free bleeding, especially from one artery; and twenty ounces of blood flowed from the incision. Half the glans had mortified. The parts were now covered with the charcoal poultice, which is an excellent application, both for correcting fetor, and improving the condition of sloughing sores. The division of the pre-

puce at once relieved the pain, and it arrested the sloughing process. The latter effect has always ensued, within my observation, when the glans has been completely denuded in this way. The debility and the mental depression necessarily attendant on such an affection, and the treatment, required a nutritious and generous diet, with tonics and restoratives; these means restored and maintained strength. The slough, which was dense and hard, separated slowly; a month elapsed before it had completely come away, while in the preceding instance of a younger subject, the mortified part was loose and soft, and completely detached, in a fourth part of the time. Here, too, as I have generally observed, the slough extended deeply under the glans; it also destroyed a portion of the urethra.

[No farther unpleasant effect has occurred in this case; several months have now elapsed since the cicatrization was completed.]

I attended another case of sloughing chancre some years ago. A gentleman, 22 years of age, had connexion with a female on Oct. 24. He had had none for a considerable time previously, and none subsequently before I saw him. On November 4, he observed a small whitish place on the glans, and consulted a surgeon, who touched it with caustic, and prescribed opening medicine, and three blue pills a day. On the 5th the sore place was larger, very painful, and produced a foul discharge. He was worse on the 6th, when three grains of calomel, and half a grain of opium, were ordered every six hours. He consulted me on the 7th, having taken four of the doses last mentioned, and feeling much worse. As soon as he opened his clothes to shew the complaint, I recognized the peculiar factor of sloughing sores. The prepuce was bright red, and slightly œdematous; the glans, equally red, protruded about half way at the orifice of the prepuce. The latter could be withdrawn with a little difficulty, so as to expose a slough equal in extent of surface to a shilling, of which about two-thirds were on the glans, the remaining one-third on the root of the prepuce. The slough was black at one point; yellowish and dark red in other parts; it had the pulpy glutinous appearance of sloughing phagedæna, and it projected above the level of the sound parts; it discharged a thin, red, and extremely offensive ichor. The neighbouring internal surface of the prepuce was highly inflamed. The whole pain was very painful. The mouth was not affected by the mercury.

The mercury to be left off; leeches to the penis, followed by poppy fomen-

tation and bread poultice; the fomentation to be injected under the foreskin. Aperient medicine. To keep in bed.

In the evening, the leeches having drawn much blood with considerable relief, the pain was relieved, the discharge less red and offensive, and thicker. The bowels had been freely opened; the pulse full, and rather hard. I took away sixteen ounces of blood from the arm, and ordered a saline draught, with thirty drops of tinctura opii, if there should be such pain as to prevent rest.

8th.—The blood presented a thin buffy coat; passed a good night without the opiate. The discharge is no longer fetid; the prepuce is still inflamed and swollen; the glans and penis very tender to the touch, and occasionally painful.

Twelve leeches (which occasioned free bleeding); an opiate draught at night.

9th.—The penis less swelled and painful; the discharge no longer sanious nor fetid, but a thin pus.

An opening draught, and afterwards a saline draught every six hours; an opiate at night.

10th.—Occasional severe pain during the night, with intervals of ease; the penis still red, and very tender to the touch. I slit open the prepuce, of which the internal surface was inflamed and partially ulcerated; the sore of the penis was larger, presenting an excavation that would hold a filbert; a loose black slough lay in it; the discharge puriform, not discoloured, but very fetid.

Frequent tepid ablation; poultice of bread and water.

11th.—Much easier since the division of the prepuce; the lining of the latter and the glans present several ulcerations, with ash-coloured surface. As these did not exist originally, they must be referred to the active inflammation of the affected parts, excited by the primary affection. In the same way a syphilitic sore behind the corona glandis not unfrequently causes inflammation, excoriation, and fetid puriform discharge of the glans and prepuce. The original sore is large and deep, the slough having separated.

14th.—The process of granulation is advancing, and the ulcerative excavation is lessened. The secondary ulcerations, and the incision of the prepuce, are healing; all swelling has subsided, and the pain is at an end.

18th.—The sore is cicatrizing rapidly.

Poultice discontinued; dry lint; a draught of infusion of cascarrilla, with

dilute sulphuric acid, three times daily.

Dec. 12th.—The sore healed soon after the last date, and the patient went out of town. The parts are now in a perfectly sound state, the loss of substance in the glans appearing much less than at the time; and the general health and strength are perfect.

This case occurred several years ago. I have seen the subject of it from time to time, and know that no constitutional symptoms ensued.

The cases now related shew you that the venereal poison may cause mortification—either simple loss of vitality, or the state called sloughing phagedena—as its primary effect; and that high inflammation of the surrounding parts, particularly the glans and prepuce, follows more or less quickly as a secondary consequence. The treatment consists in depletion, general or local, or both, with other antiphlogistic means; and in division of the prepuce when it has become inflamed. The propriety of the latter measure is indicated by general swelling, redness, and severe pain of the penis, with fetid ichorous or sanious discharge from the orifice of the prepuce. The latter part will soon slough, if local relief is not procured in the manner now indicated. The division of the prepuce, however, is of great advantage in checking the progress of sloughing chancre on the glans, even where the part is not considerably swelled, nor in the state of phymosis, as you see from the foregoing cases. In this rapidly destructive and formidable complaint, mercury is unnecessary; its free exhibition would probably be most injurious. I do not think it necessary to detain you on the minor points of treatment, such as the occasional use of anodynes, the variations of local treatment according to the state of the ulcer, the diet, the administration of tonics and restoratives when the progress of disease has been checked; the particular indications in each case will direct your conduct in these matters.

ON THE MICROSCOPIC STRUCTURE OF THE BRAIN,

AND OF THE TISSUES INTERMIXED WITH IT.

By DR. LUIGI CALORI.

AFTER describing the three principal forms of the ultimate structure of the brain observed by anatomists, viz. the tubular of Ehrenberg, the fibrous or cylin-

driical of Alex. Monro and Fontana, and the globular of Della Torre, he mentions his own observations, which have led him to conclude that all these forms may be produced and rendered apparent by attention to certain particulars. The most uniform appearance is the globular; but the shape varies much with the mode of preparing the brain for examination, and with the degree of illumination to which it is subjected under the microscope, as well as with the magnifying power; nevertheless, the globular is most probably the real form of the nervous matter.

The tissues that are intermingled with the nervous substance and give it support, are very fine and transparent cellular substance, and the blood-vessels pointed out by Prochaska, of which Calori asserts that no one has hitherto given a full and perfect description. That the vessels of the medullary and cortical portions are not identical, is clear from their difference of calibre and distribution, and hence Gall was wrong in asserting that the latter was the source of the former. These vessels form a remarkably fine network, quite disproportionate to the size of the primary vessel, and similar to the distribution of the vessels on the surface of the allantoïd membrane. The nervous matter is united to these vessels, within whose ramifications it is collected; and, if really globular, it approaches most closely to the adipose tissue of Malpighi in its general appearance.—*Bullettino delle Scienze Med. de Bologna*; and *Brit. and For. Med. Rev.*

CASE OF LIVER ABSCESS.

MR. J. C. S. was seized on the 6th of August, in Canton, with inflammatory symptoms, for which he was leeches on the 10th or 12th, and bled from the arm on the 14th or 15th, and had this treatment afterwards followed up by a succession of leeching and blistering, and the administration of calomel every night, until the severity of the symptoms gave way. The disease was so far got under before his leaving Canton, that he was considered out of danger by his medical attendants, and was recommended by them to go to Macao for the benefit of a purer atmosphere, where he arrived on the 1st of September, labouring under a relapse of all his former symptoms, but of an aggravated and more strongly marked character. He complained of much acute tenderness over the whole region of the liver; so much so, as to be scarcely able to bear any degree of pressure of the hand upon any part of it. An attempt even to take a deep inspiration caused very severe pain in the right side. His respiration was short, quick,

and attended with cough; tongue coated, mouth parched; quick and sharp pulse; anxiety of countenance, and great general prostration; symptoms clearly indicating that the inflammatory process had exceeded the bounds which admit of a termination of active disease by resolution.

The application of leeches to the seat of pain, which was had recourse to repeatedly, and carried as far each time as his reduced state would admit of, afforded only temporary relief. His bowels were carefully attended to and kept open by means of emollient clysters, with occasional small doses of calomel, and rhubarb and castor-oil. Counter-irritation by means of blisters and the tartar emetic ointment was kept up; the nitro-muriatic bath was tried, and persevered in for some time; notwithstanding all which, no decided benefit was produced.

The above treatment was pursued until the 13th, when a sudden change for the better, in the character of the symptoms, took place. He felt himself all at once relieved, and was sensible of something having given way within him. On examining his motions next day, a very considerable quantity of purulent matter was discerned in them, and in those he passed for several days after, which sufficiently warranted the opinion that had been held, of an abscess having formed in the liver. For ten or twelve days after this, he improved considerably, when another return of the symptoms took place. The same remedies were employed as before, together with anodyne fomentations, with the same want of success; he got daily worse; and serious apprehensions regarding his recovery were entertained, when, on the 4th of October, he experienced another sudden change for the better. But this abscess being higher situated in the organ than the former one, burst into the *thorax* instead of the *colon*, and the matter was discharged by expectoration. Ever since he has continued to get better; and nothing further was required than a careful attention to the state of the bowels, keeping them open by mild aperients and emollient clysters, improving the strength generally by demulcent tonics and a strictly regulated diet, and allaying nervous irritability and procuring sleep by means of night-draughts containing the acetate of morphia.

A few days ago he felt some uneasiness in the right side: the cupping glasses were had recourse to, but as he could not endure them, leeches were applied in their stead, and with a very good effect. He is now recovering rapidly*.

ROYAL INSTITUTION.

Friday, Jan. 26, 1838.

THIS evening a lecture was delivered by Dr. FARADAY,

On the Nature of Fatty Bodies, and the Application of Stearine to the Manufacture of Candles.

The lecturer began by stating that all fats, whether animal or vegetable, are composed of the ultimate elements—carbon, hydrogen, and oxygen—though in somewhat different proportions; in all, however, the carbon greatly predominates. But their proximate principles are oleine and stearine; the oleine, or oil, being separable by pressure from the stearine, which is a solid. The lecturer then went on to give a very brief outline of the way in which soap is made—by the application of an alkali (as potass, or soda) to fat, or oil. Before Chevreul, who, in 1813, directed his attention to the subject, it was generally believed that, by the application of an acid, the fatty body thus converted into soap would be restored to its former condition: but his experiments have shown that the qualities are then essentially different from what they were before the alkali was applied.

He next directed the attention of his audience to the process of candle-making; where, instead of potass, or soda (which would be too expensive), quick-lime is employed, and when mixed and stirred with the fat, produces exactly the same effect as an alkali. He explained each step of the process with great clearness, illustrated his remarks as he proceeded by numerous specimens of the materials in their different states, and concluded by displaying two sets of moulds, showing how the wicks are fastened, and how the tallow is used when finally prepared. He stated, that wicks should be soaked in acid for some time before they are used; but the acid used must not be sulphuric, which rots them. Great care must be taken to pour the tallow into the moulds at a proper temperature; for if it is too hot, it is very difficult (in some cases impossible) to extract the candles unbroken; and even if they come out whole, they are inferior in appearance to others. For the sake of facilitating the process of extracting the candles from the moulds, different substances have been employed in small quantities: of these Dr. F. confined his remarks to arsenic, which was first employed by a Parisian; and though the art was attempted to be concealed, it found its way into England a few years ago, where

* Dr. Colledge, in the Quarterly Journal of the Calcutta Med. and Phys. Society, April 1837.

it has since been almost universally employed. At first it was used only in the proportion of 1 10th of a grain, which, however, has since increased to (it is said) $4\frac{1}{2}$ grains per candle. As the arsenic is not dissolved in the tallow, it sinks down in the mould when the liquid is poured in, so that much the greatest quantity is to be found at the top of the candle. Dr. F. showed specimens of candles containing different quantities of this matter, and said that he had been unable to light one of them, in consequence of the quantity contained at the top; but upon cutting it off, the lower part of the candle was lighted with perfect ease. Where it exists in any quantity, if the candle is blown out, a very disagreeable smell is easily perceived. He stated, that one of the best methods of ascertaining the presence of arsenic is to melt the tallow, put a few pieces of it into a retort, along with some pieces of zinc, and pour in a small quantity of sulphuric acid. If the hydrogen thus generated be collected and lighted, the colour of the flame will betray the presence of the arsenic; or (as all may not be familiar with its natural colour) let a white plate be held over the flame for a few minutes, and a deposit of the arsenic will soon be perceptible. It is, however, satisfactory to know, that owing to the late inquiries of the Westminster Medical Society, the practice has been discontinued.

ACCIDENTS ADMITTED AT THE LONDON HOSPITAL.

Surgeon, Mr. ANDREWS.

Assistant-Surgeon, Mr. ADAMS.

Jan.	Sex.	Age.	Case.
2.	M.	28	Injured spine.
	F.	25	Contused hip and eye.
	F.	25	Contused knee.
	F.	48	Fractured ribs.
	F.	38	Contusions.
3.	F.	37	Sprained ankle.
	M.	60	Dislocated humerus.
	M.	43	Fractured ribs.
4.	M.	36	Fractured skull, laceration of brain, rupture of spinous artery (a).
	M.	56	Fractured ribs.
	M.	44	Lacerated foot.
	M.	48	Lacerated foot.
	M.	10	Fractured tibia.
	F.	22	Contused hip.
	F.	66	Injured shoulder.
5.	F.	13	Contused hip and abdomen.
	F.	6	Burn.
	M.	24	Injured eye (b).
	M.	8	Injured hand.
	M.	17	Fractured humerus.
	M.	40	Sprained ankle.
6.	F.	12	Contused hip.
	F.	50	Fractured ribs.

7. M. 16 Lacerated forearm.

8. M. 34 Lacerated hand.

In-patients 25

Out-patients 34

Total 59

(a.) The subject of this case was a sailor, who had fallen on board a ship from a considerable height, and was taken up insensible, in which state he was brought to the hospital. At the time of his admission he was labouring under the apparent symptoms of an extreme degree of concussion of the brain, and a large quantity of blood had escaped from the right ear. His insensibility continued almost complete up to the period of his death, which occurred on the fifth day after the accident. On the third day after his admission a transient gleam of returning sensibility manifested itself, as he spoke to the nurse, asking for food, and took some milk and bread which stood near him; but he soon relapsed into his former insensible state. He was, however, at intervals restless, drawing his arms forcibly away when any examination of his pulse was attempted. The pulse was small and weak; the pupils, almost insensible to light, neither contracted nor dilated; his urine and feces flowed involuntarily from him; he had no apparent paralysis of the extremities. The only external appearance of injury was a slight tumefaction over the right temple, and this scarcely perceptible; but as the symptoms indicated rather a general injury to the brain, no incision was made. The treatment adopted was antiphlogistic; and mercury was exhibited by the mouth and by inunction. He sank gradually.

Post-mortem.—A fissure on the right side of the skull, across a branch of the spinous artery, extending down over the petrous portion of the temporal bone; blood effused on the dura mater on this side. Extensive laceration of the substance of the left middle lobe of the brain, at the basis.

(b.) This patient was admitted with a violent purulent inflammation of the left eye, which he attributed to some lime having been blown into it as he walked. It was, however, soon apparent that the disease was not attributable to this supposed cause, and that the case was to be regarded as one of violent gonorrhœal ophthalmia. He had been the subject of gonorrhœa three weeks. All the symptoms it is unnecessary to detail; but it may be observed, that sloughing of the cornea had occurred at the upper portion, and the iris had protruded. A slight haziness of the cornea subsequently took place. The case is alluded to as a proof of the efficacy of a combination of the antiphlogistic and stimulating plan of treatment, as the dis-

ease is completely arrested. The treatment consisted in the exhibition of mercury, so as to affect the mouth—in the free scarification and excision of the chemosed conjunctiva—and the injection into the eye every hour of a solution of nitrate of silver, in the proportion of three grains to the ounce.

Surgeon, MR. SCOTT.

Assistant-Surgeon, MR. HAMILTON.

- | | | |
|-----|-------|---|
| 9. | M. 45 | Injured elbow. |
| | M. 24 | Hernia (reduced.) |
| | M. 17 | Dislocation of the humerus, with fracture. |
| | F. 4 | Burn (died Jan. 10th.) |
| 10. | M. 60 | Lacerated scalp. |
| | F. 70 | Fractured femur. |
| | M. 50 | Concussion. |
| | M. 62 | Fractured neck of the femur. |
| | F. 6 | Burn (died Jan. 12th.) |
| 11. | M. 28 | Fractured fibula. |
| | M. 35 | Cut head and concussion. |
| | M. 25 | Lacerated foot. |
| | M. 18 | Concussion. |
| | M. 7 | Burn. |
| | F. 4 | Scald (died Jan. 15th.) |
| | F. 2 | Burn (died Jan 13th.) |
| | F. 7 | Burn (died Jan 13th.) |
| | F. 26 | Burn. |
| | F. 11 | Burn (died Jan. 14th.) |
| 12. | M. 25 | Wounded leg. |
| | M. 23 | Contused back. |
| 13. | M. 57 | Fractured ribs. |
| 14. | M. 43 | Fractured right clavicle, and dislocation of the left do. |
| 15. | M. 22 | Contused hip. |
| | M. 18 | Injured testicle. |
| | M. 30 | Injured ankle. |
| 16. | M. 39 | Injured ankle. |
| | F. 28 | Injured ankle. |

Out-patients 49

In-patients..... 28

Total 77

Amongst the accidents treated as out-patients were 13 cases of fracture, making 19 during the week.

PLASTER CASTS IN FRACTURES.

To the Editor of the Medical Gazette.

SIR,

Nothing could be further from my intention and wishes than to despoil a professional brother of that reputation to which his skill and industry entitle him; and had I seen or heard of Mr. Beaumont's pamphlet, should have felt equal pleasure in acknowledging that I had been led to employ the plaster casts in consequence of

his suggestions, as I had in referring to the paper published by Messrs. Bond and Gale in the *Lancet*, which was to me a satisfactory authority, as I know the former gentleman to be an ingenious man and good practical surgeon.

It is probable that Messrs. Bond and Gale had seen the ill-timed and injudicious critique on Mr. Beaumont's pamphlet in the journal edited by Dr. Ryan, as they refer to a notice which they "recollect reading some time before in one of the medical periodicals, wherein the method" of treating fractures by means of plaster casts was "condemned as impracticable and injurious."

Messrs. Bond and Gale's paper is dated July 1831, and in the following month a letter from Dr. John Browne, of Dublin, appeared in the same journal, saying, that Professor Kirby had employed successfully the plaster casts ten years before the appearance of the paper sent forth by those gentlemen, but it does not appear that Professor Kirby has at any time published an account of the method.

With whom this method of treating fractures originated, it would be difficult at this time to decide. I have been informed within a few days that an account of it was published many years ago in the *Medical Repository*, and it is long since that M. Lallemand introduced it from Montpelier into the Parisian hospitals. There can be no doubt that the practice has been handed down by tradition from a very remote period, but Mr. Beaumont is perhaps the first gentleman who has written a pamphlet expressly on the subject, for which our thanks are due to him. For my own part, I disclaim all title to originality in this matter, and can only express my grateful acknowledgments to the inventors and revivers of the method in question, which I am sure will be found in the fullest manner to answer the expectations of those practitioners who may be induced to employ it; that it will eventually supersede the ordinary splint and bandage, and be found, though rough in its exterior, to be the really "soft and easy method of treating fractures."

I am, sir,

Your obedient servant,

W. SWEETING.

Bridport, 13th Jan. 1838.

In the paper I transmitted to you on the 24th ult. reference is made to a case then under treatment. In the present week the plaster cast has been removed, and perfect union of the fractured bones found to have taken place. The case was in the beginning very unpromising, in consequence of the bones having been a long time without an approach to union, but it has turned out extremely well.

OF

DRUGS ON SALE IN THE ENGLISH MARKET,

With their Prices and several Duties.

(From the Official Returns.)

	PRICE.			DUTY.			DUTY PAID	
	£	s.	d.	£	s.	d.	In 1837.	In 1836.
Aloes, Barbadoes, D.P. c	12	0	0	to 30	0	0		
Hepatic (dry) BD. c	5	0	0	14	0	0		
Cape, BD. c	2	0	0	2	2	0		
Aniseed, Oil of, German, D.P. lb	0	9	6	0	9	6		
E. I. lb	0	7	0	0	7	6		
Asafoetida, B.D. c	2	10	0	5	0	0		
Balsam, Canada, D.P. lb	0	1	3	0	1	4		
Copaiba, BD. lb	0	6	0	—	—	—		
Peru, BD. lb	0	4	3	—	—	—		
Benzoin (best) BD. c	25	0	0	50	0	0		
Camphor, unrefined, BD. c	9	0	0	—	—	—		
Cantharides, D.P. lb	0	5	6	—	—	—		
Carraway, Oil of, D.P. lb	0	9	0	—	—	—		
Cascarilla or Eleutheria Bark, D.P. c.	1	15	0	—	—	—		
Cassia, Oil of, BD. lb	0	7	0	—	—	—		
Castor Oil, East India, BD. lb	0	0	6	0	0	10		
West I. (bottle) D.P. 1½ lb	0	2	3	—	—	—		
Castoreum, American, BD. lb	1	15	0	—	—	—		
D.P. Hudson's Bay lb	1	0	0	1	4	0		
Russian, lb				none				
Catechu, BD. c	1	8	0	1	10	0		
Cinchona Bark, Pale (Crown) lb	0	2	0	0	3	6		
BD. Red lb	0	3	0	0	6	0		
Yellow lb	0	2	0	—	—	—		
Colocynth, Turkey lb	0	2	6	0	4	0		
D.P. Mogadore lb	0	3	0	—	—	—		
Calumba Root, BD. c	1	4	0	2	5	0		
Cubebs, BD. c	5	0	0	—	—	—		
Gamboge, BD. c	5	0	0	15	0	0		
Gentian, D.P. c	1	4	0	—	—	—		
Guaiacum, D.P. lb	0	1	0	0	1	8		
Gum Arabic, Turkey, fine, D.P. c	8	0	0	9	0	0		
Do. seconds, D.P. c	5	0	0	7	0	0		
Barbary, brown, BD. c	3	0	0	3	3	0		
Do. white, D.P. c	4	15	0	—	—	—		
E. I. fine yellow, BD. c	3	0	0	3	10	0		
Do. dark brown, B.D. c	1	15	0	2	5	0		
Senegal garblings, D.P. c	4	15	0	5	0	0		
Tragacanth, D.P. c	8	0	0	12	0	0		
Iceland Moss (Lichen), D.P. lb	0	0	2½	0	0	3		
Ipecacuanha Root, B.D. lb	0	1	9	0	2	0		
Jalap, BD. lb	0	2	2	—	—	—		
Manna, flaky, BD. lb	0	4	0	0	5	6		
Sicilian, BD. lb	0	1	7	—	—	—		
Musk, China, BD. oz	1	0	0	1	8	0		
Myrrh, East India, BD. c	5	0	0	14	0	0		
Turkey, BD. c	2	0	0	11	0	0		
Nux Vomica, BD. lb	0	8	0	0	9	0		
Opium, Turkey, BD. lb	0	12	6	0	13	0		
Peppermint, Oil of, F. BD. lb	1	1	0	—	—	—		
Quicksilver, BD. lb	0	3	6	—	—	—		
Rhubarb, East India, BD. lb	0	2	6	0	3	3		
Dutch, trimmed, D.P. lb	0	3	6	0	4	0		
Russian, BD. lb	0	8	3	—	—	—		
Saffron, French, BD. lb	0	18	0	—	—	—		
Spanish lb	0	19	0	—	—	—		
Sarsaparilla, Honduras, BD. lb	0	1	0	0	1	9		
Lisbon, BD. lb	0	2	0	—	—	—		
Scammony, Smyrna, D.P. lb	—	—	—	—	—	—		
Aleppo lb	0	12	0	0	15	0		
Senna, East India, BD. lb	0	0	3	0	0	4		
Alexandria, D.P. lb	0	1	6	—	—	—		
Smyrna, D.P. lb	0	1	0	0	1	3		
Tripoli, D.P. lb	0	1	0	0	1	3		

‡‡‡ BD. In Bond. — c. Cwt. — B. P. British Possessions. — F. Foreign. — D. P. Duty paid.

ATMOSPHERIC TEMPERATURE.

WEEKLY ACCOUNT OF BURIALS,

To the Editor of the Medical Gazette.

SIR,

On Friday night, the 19th ultimo, and during the following day, the thermometer indicated the lowest degree of temperature which has probably been recollected in England for many years past. At ten o'clock on the evening in question the mercury stood at 3° below zero, and brandy exposed to the air froze, in the course of the night, into a solid amorphous mass. Some spirit of ammonia, confined in a common phial, deposited a large quantity of crystalline flakes, which were afterwards perfectly insoluble in the same medium. But the most remarkable fact is perhaps connected with the great change of temperature which the atmosphere suddenly underwent. On Saturday, at nine o'clock in the morning, Fahrenheit's thermometer, placed in a northern aspect, sank 4° below 0, and on the Sunday morning following, at the same hour and in the same situation, the mercury had risen to 3° above freezing, thus indicating, in the space of four-and-twenty hours, a variation of atmospheric temperature amounting to thirty nine degrees.

It may be observed, that the thermometer with which these observations were made was graduated (as I believe Fahrenheit's usually are) four degrees only below zero; but the mercury, in the latter instance of intense frigidity which I have recorded, had retreated a small distance into the bulb; and I presume, had the instrument been capable of exhibiting it with accuracy, that it would have indicated a considerably greater degree of cold than that above mentioned.—I am, sir,

Your obedient servant,

R. H. ALLNATT, M.D.

Wallingford, Jan. 23, 1833.

From BILLS OF MORTALITY, Jan. 30, 1838.

Age and Debility . . .	51	Inflammation . . .	19
Apoplexy . . .	1	Bowels & Stomach . .	1
Asthma . . .	16	Brain . . .	4
Childbirth . . .	2	Lungs and Pleura . .	15
Consumption . . .	33	Insanity . . .	1
Convulsions . . .	22	Liver, diseased . .	3
Croup . . .	2	Measles . . .	6
Denitition or Teething .	4	Mortification . . .	3
Dropsy . . .	6	Paralysis . . .	3
Dropsy in the Brain . .	1	Scrofula . . .	2
Dropsy in the Chest . .	2	Small-pox . . .	5
Fever . . .	15	Sore Throat and . .	
Fever, Scarlet . . .	1	Quinsey . . .	1
Fever, Typhus . . .	5	Unknown Causes . .	36
Heart, diseased . . .	2		
Hooping Cough . . .	7	Casualties . . .	5

Decrease of Burials, as compared with }
the preceding week . . . } 9

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.

Jan.	THERMOMETER.	BAROMETER.
Thursday . 25	from 19 5 to 29	29.54 to 29.42
Friday . . 26	21 31	29.35 29.33
Saturday . 27	24 31	29.26 Stat.
Sunday . . 28	23 33	29.35 29.43
Monday . . 29	27 43	29.43 29.52
Tuesday . . 30	28 37	29.55 29.61
Wednesday 31	29 35	29.68 29.84

Wind N.E.

Except the afternoon of the 29th, cloudy; a little rain on the morning of the 29th and two following days; a heavy fall of sleet on the evening of the 28th, which, with the snow that fell at the commencement of the frost, almost entirely disappeared before the evening of the following day, since which the frost has been scarcely perceptible.

CHARLES HENRY ADAMS.

NOTICE.

The communications of Mr. Gorham, Dr. Munk, Mr. Thurnam, and several others, are unavoidably postponed.

ERRATA.

In Mr. Curling's third lecture on the Entozoa, for "στομα, stomach," read "στομα, mouth."

In Dr. Watson's Clinical Lecture, in our last number, p. 700, 2d column, line 37, for "it," read "these bronchial sounds;" p. 701, 2d column, line 12 from the bottom, the first "or" should be "and;" p. 703, the last sentence of the note should have been printed as the last sentence of the lecture.

WILSON & SON, Printers, 57, Skinner-st., London.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, Jan. 25th, 1838.

John Thomas Darvill, 27, Mornington-place, Hampstead-road.—Fred. Chapman, Kingston-on-Thames.—Henry Gny, London.—Frederick Hyett, Cheltenham.—Augustus Maybury, Kenmare, Ireland.

Thursday, 1st February.

Albert Richard Smith, Chertsey, Surrey.—Henry Northover Pink, Broadwindsor, Dorset.—George Johnson Gossling, Macclesfield.—John Francis, Ilchester, Somerset.—Morgan Williams, Llan-dilo.—William Alfred Plues, Ripon.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, FEBRUARY 10, 1838.

LECTURES

ON THE

PHYSIOLOGY AND DISEASES OF THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL
AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of
1836 and 1837,

At the Anatomical School, Kinnerton-Street,
near St George's Hospital,

By CHARLES J. B. WILLIAMS, M.D. F.R.S.

LECTURE XIII.

Diseases of the Air Tubes (continued)—Chronic Bronchitis: Pathology; Course and Signs; Treatment—Croup: Pathology and Signs; Treatment—Whooping-cough: Signs and Treatment. Diseases of Secretion—Pituitous Catarrh: Pathology and Signs; Treatment—Dry Catarrh: Pathology and Signs; Treatment: Alkaline Attenuants—Bronchial Hæmorrhage.

CHRONIC bronchitis is not separated by any distinct line from the acute disease; but in many cases it presents very distinguishing characters. The two forms of inflammation pass by insensible gradations into each other, and are often conjoined; for although acute bronchitis frequently exists alone, chronic bronchitis is rarely free from occasional admixture of acute inflammation, and neither is the long duration of the disease always a proof that it is not acute, for I have known several cases of acute bronchitis in which attack succeeded to attack for many weeks, and never lost the acute character. The expectorated matter gives us some proofs of the state of the membrane from which it comes; and by the heterogeneous charac-

ter often presented by that in chronic bronchitis, we may form a notion of the various pathological conditions simultaneously affecting different parts of bronchial tubes. I think that we may say, however, that there is generally some opaque matter in the expectoration of chronic bronchitis, and that the tendency of chronic inflammation is to make the mucous membrane secrete, instead of glairy mucus, more or less of the opaque matter, which we have classed generally under the head Albuminous Sputa, whether that be muco-purulent, purulent, fibrinous, or caseous; whether these occur separately, or, as is more usual, variously combined, of various degrees of consistence and colour, and variously mixed with a thinner and more transparent fluid of a mucous or serous quality.

Chronic bronchitis presents itself in many forms and modes of origin. We cannot stay to enumerate these; but let us see how it arises out of the acute disease. An acute attack of bronchitis has lasted long enough to injure the membrane; or its effects have not been controlled by treatment; or stimulating causes have kept up a general or local irritation at the stage in which the inflamed bronchial membrane was relieving itself by an unusual secretion: in either of these cases, although the sputa have become partially opaque and *concocted*, and the usual mitigation of the fever and other symptoms have accompanied this change, yet the complaint then becomes stationary, with a lower febrile and inflammatory character, but with unsubdued and more paroxysmal cough, often with dyspnoea, soreness and wandering pains in the chest, and more or less general ill health, and continuing disorder in other functions. The sputa become diffuent, and mixed with some masses, *concocted* and opaque, of a yellowish or greenish colour, often obviously purulent, sometimes of a dirty grey or

brown colour, and in some parts still thin and transparent. More rarely, an albuminous matter, moulded by the shape of the bronchial ramifications, is expectorated by a violent and suffocating cough. When the expectoration is purulent, there is usually a great deal of prostration, and some loss of flesh; occasionally we find evening hectic, night sweats, and other of the common symptoms of tubercular consumption; but the physical signs are wanting.

The chest, in simple chronic bronchitis, still expands equally, and sounds well on percussion. The respiration and cough are heard with various rhonchi, mucous, sonorous, sibilant, and clicking, which are continually shifting and changing. There is no bronchial or cavernous respiration; no permanent absence of respiration in a part; no unusual resonance of the voice; and in spite of the continuance of the copious and puriform expectoration, on listening, day after day, we still find no signs of a cavity, no cavernous rhonchus or pectoriloquy. Under these circumstances, whatever be the general symptoms, we may pretty confidently pronounce that the disease is not tuberculous consumption, but simple chronic bronchitis. It is not, however, always very easy to get this perfect degree of negative evidence, and it requires much experience in auscultation, or repeated examinations, to enable one to report it with confidence. In such, and all doubtful cases, we should take into consideration also, the history of the attack, the constitution of the patient, and such of the general symptoms as may serve to throw light on the prevailing tendencies of the system. The more profuse the expectoration, especially if it be very purulent, or otherwise albuminous, the less likely is the case to be one of phthisis, if no physical signs of this disease are found, and it may be the more readily inferred that the sputa only proceed from a diseased membrane. But there are other difficulties to be considered on this point of diagnosis, which we had better postpone till we treat of phthisis.

Chronic bronchitis is often a very obstinate disease. In its milder kinds it constitutes the winter cough of old people, and may continue for many years without much injuring the constitution of the patient; but in its severer forms, when the expectoration continues to be profuse, or of a purulent nature; or when the membranes are extensively affected, and dyspnoea is the result; or when the cough is very harassing, or has a convulsive character, then the health and the strength of the patient may suffer much; in fact, those of weakly frames often fall victims to it, and this either from the disease

itself only, or also from the alterations which it causes in the structures of the air-tubes and lungs. Of these we shall speak hereafter.

The treatment of chronic bronchitis must vary much, according to the nature of the case, as indicated by the cough, expectoration, and state of the circulation. Bloodletting is not often necessary, unless to relieve a temporary exacerbation or congestion, when leeches under the clavicles, or to the top of the sternum, or cupping between the shoulders, will generally suffice. The most generally useful class of remedies are, counter-irritants, conjoined with mild alterative tonics. Friction of the chest with an oily liniment containing various proportions of tartar emetic, tincture of campharides, the essential oils, ammonia, acetic acid, or a diluted mineral acid, according to the degree of effect desired; or a succession of mild blisters; or, in less severe cases, wearing an ample pitch or mercurial plaster, with a small proportion of blistering fly in it, will furnish a choice of means of counter-irritation, applicable to every case. The methods by friction are preferable to the use of plasters, for they promote in some measure the respiratory movements, whereas plasters, unless they are supple and carefully applied, may somewhat restrain the expansion of the chest. To avoid this the patient should be desired to take long deep inspirations when the plaster is first applied; and if its material be rigid, it should have long cuts in it from middle to margin, corresponding with the intercostal spaces from the sternum to the sides. When we come to speak of the alterations of structure induced by inflammatory affections, you will perceive the importance of promoting in every way the free and equal expansion of the chest. With the plan of external counter-irritation it is generally expedient to join such internal remedies as may seem best calculated to improve the condition of the diseased membrane and of the functions generally. These will vary surprisingly in different cases, and although all these cases may suffer from the same local disease—chronic bronchitis—yet they may be best relieved by the most opposite remedies. Mild tonics, such as calumba and cascarrilla, with nitric acid, sarsaparilla, taraxacum, are very commonly useful to improve the condition of the secretions and other functions; and where the expectoration is profuse, and even purulent, without much vascular excitement, the mineral acids and metallic astringents in some cases, in others myrrh, copaiba, the balsam of Peru, benzoic acid, and the like, prove occasionally useful. Many of these are safe and beneficial only

when combined with external counter-irritation; which, like a safety-valve, lets off in another way any undue stimulation which they may cause. I have found that even steel medicines, particularly that most valuable one the iodide of iron, have been borne, and have sometimes proved very salutary in improving the general health and strength. Of late years much has been said for and against the direct application of remedies, particularly the vapour of iodine and chlorine, by inhalation. I have not much experience of this kind of treatment, but in several cases in which I have seen it tried, an unfavourable effect seemed to arise from the effort necessary in using the inhaling apparatus, the tubes being too small, and the patients complaining of the operation fatiguing them. I should suggest, as a substitute for this plan, the diffusion of iodine, or chlorine, combined with watery vapour, either in the apartment of the patient, or, what would be more practicable, in a small room, or closet, cleared for the purpose, in which he could spend from half an hour to an hour twice a day. Iodine, or chlorine, may be readily dispersed in any quantity through the room, by placing a few grains of the former, or a solution of the chloride of soda, or lime, in a saucer floating on hot water. The quantity should be determined by the effect on the patient; always keeping it below that which causes much coughing or acceleration of the pulse.

When all these means fail, a change of air (especially to a warm sea-coast residence, where the patient can use moderate but regular exercise in the open air) often proves beneficial; and to convalescents, repeated changes, such as those obtained in a sea-voyage or gentle land travelling, in warm weather, sometimes prove very salutary. Besides the several symptoms of improvement which I need not enumerate, and the recovery of looks and strength, we should watch for the physical signs, in the more perfect expansion of the chest, the diminution of the rhonchi, the equal respiratory murmur, and the restoration of the expectoration to a simple scanty mucus.

We must just notice some specific affections of the air-passages, in which inflammation is a prominent part, although the accompanying phenomena, or effects, differ from those of common inflammation.

Croup, and membraniferous or plastic inflammation of the air-passages, are characterized by the solid albuminous matter which they throw out; and their dangerous nature arises from the obstruction which this effusion opposes to the passage of the air. There are two very distinct affections of this kind; one decidedly

inflammatory, of a very acute character, and tending rapidly to the formation of a tough fibrinous matter, which resembles the lymph effused by inflamed serous membranes, but does not appear even to become organized: this is the true croup; a complaint almost exclusively affecting children. The other affection is more of the character of a cynanche, is less acute, and is more tardily accompanied by the effusion of a soft pellicle, like those which form on the tonsils in a kindred affection; indeed the tonsils are generally simultaneously affected in the same way. This is the *diphtherite* of Brétonneau; it is a much more chronic affection than croup: it prevails epidemically, is by some considered to be contagious, and affects adults more than children. I do not think that I have ever seen more than two or three instances of this complaint, so that what I have to say will refer chiefly to croup.

This *croup*, stridulous character of the sound of the air in passing the trachea, particularly in inspiration, which is the most forcible act, the indistinctness of the pulmonary murmur, owing to the little air that enters the chest, which nevertheless sounds well on percussion, and the concave state of the intercostal spaces at each inspiration, showing that there would be room in the chest if the air could get in; these are distinctive signs of unmixed croup. But it very commonly happens that there is wheezing in the chest also, from the simultaneous affection of the bronchi with either the plastic or common inflammation. The sonorous inspiration of croup is audible through the stethoscope, or ear, applied to the throat or upper part of the chest, before it can be heard by the ear unapplied: we should, therefore, use this mode of examination with children liable to croup, whenever they show any signs of indisposition. Spasmodic croup is a distinct affection; but there is probably a good deal of spasm accompanying this disease, both in the early stage, where the inflammation must greatly augment the irritability of the muscular fibres of the air-tubes, and in the subsequent period, when the albuminous coating, becoming detached, acts as a foreign body, continually irritating them. The temporary aggravations of the dyspnoea favour this view. It was remarked by Dr. Cheyne that the solid effusion found in the trachea and bronchi after death, is never sufficient nearly to close these tubes; it fact it is commonly in a hollow or tubular form, with a pretty large opening within; it has therefore been suggested as probable that a spasm added to the constriction.

I have nothing new to offer you on the subject of the treatment of croup. At its

earliest onset, when the croupy breathing is only audible to the ear applied, bleeding and the warm bath, or an emetic, followed by a brisk mercurial cathartic, may sometimes arrest the disease; but when the disease has established itself in its well-known form, that is, after the albuminous exudation has taken place, these remedies lose their effect, and are ill borne. Then the chief resource is mercury in large doses, or tartar emetic watched closely in its effects; and the success of these measures will depend on there being strength enough to battle against the stifling influence of the disease until their operation reaches the system, and causes the absorption or removal of the effused matter. Sometimes violent and almost suffocating fits of coughing are excited by the retained pellicle. An emetic will sometimes assist in its expulsion. In the less inflammatory affection, diphtheritis, the removal of the albuminous concretions is sometimes accelerated, and a healthy action promoted, by touching the fauces and epiglottis with diluted muriatic acid, or a pretty strong solution of nitrate of silver; and stimulant inhalations have been recommended with the same view. Dr. Cheyne, Mr. Porter, and other of the best authorities, agree that bronchotomy offers little chance of relief in croup, for the disease generally involves the bronchi as well as the trachea.

The peculiarity of *Pertussis*, or *Whooping-cough*, lies in the nervous, or rather the muscular, relations of respiration. The affection is at first distinctly inflammatory, and the cough is not unlike that of common bronchitis; but it soon assumes a convulsive character, the fits of coughing being very violent, uncontrollable, shaking the whole frame, and often ending with retching. There may be a *whooping* sound in the inspiration during the cough, but not necessarily; and this, as we have before noticed, depends on an undue irritability of the bronchial and contractile laryngeal muscles, so that they do not relax as usual during the act of taking breath. During this *whoop*, or sonorous back draught, the ear applied to the chest hears little or no natural respiratory murmur; but, during the intervals of the cough, this murmur may be as distinct as usual. This does not necessarily imply, however, as Laennec supposed, that there is a spasm in the bronchi; the upper part of the tube being narrowed, is sufficient to weaken much the sound of vesicular respiration, as it is observed in oedema or spasm of the glottis, and where aneurisms or other tumors press on the windpipe.

I have little to say on the treatment of

pertussis in addition to what you already know, or may find in modern works. In the first stage it is almost purely inflammatory; a bronchitis, in fact, but generally combined with more headache and general disorder than usual. In the second stage it is inflammatory or congestive, and nervous; and at last it becomes entirely nervous; but even in this stage it is often complicated with alterations in the membrane, and even in the structure of the lungs, which the previous inflammation, conjoined with the mechanical violence of long-continued fits of coughing, has produced. Moderate depletions and other antiphlogistics, in the first stage; counter-irritants, occasional emetics, and sedatives to the muscular mobility, in the second; and antispasmodics and nervous tonics in the third, are the chief heads of the treatment. Stimulant and opiate embrocations rubbed over the chest twice a day, are often very useful in allaying the violence of the cough; and I believe that they effect this partly by diminishing the excitability of the respiratory muscles. Of internal medicines for this purpose, I know of none so effectual as belladonna; but, to have its due effect, it should be administered in larger doses than those usually recommended. I have often given a quarter of a grain of the extract three times a day, to a child of two years, half a grain to one of four, and a whole grain to one of eight years of age; and increased these quantities to double, and more when they ceased to relieve. These doses generally cause some dilatation of the pupil; and I conceive that the remedial agency of the drug depends on the same power to diminish irritability in the bronchial and laryngeal muscles, which is here evinced with regard to the iris. In some cases there have been some feeling of heat and dryness in the throat, giddiness and pain over the eyes; but these symptoms lead to no bad consequences, and soon pass off when the medicine is discontinued. In fact, I have known more than one instance of a large quantity of the extract of belladonna being swallowed in mistake (in one case upwards of a drachm), without any other bad effects than the temporary production of the symptoms described as those of *poisoning* by belladonna, and which entirely passed away in a day or two. Sir Benjamin Brodie has informed me that he has repeatedly given large doses to animals; and although they manifested for a time many ugly symptoms of nervous disturbance, they all ultimately recovered. I tell you this, that you may not be alarmed should untoward symptoms sometimes manifest themselves during the exhibition

of belladonna. They occasionally occur when it is given in very moderate doses, and even during the application of a belladonna plaster externally; but they do no further harm. In many cases I have found the belladonna to diminish signally the violence and number of the paroxysms of whooping-cough; but as it loses some of its efficacy by continuance, I have found it best to intermit its use for a few days, and then to resume it again. In the more violent cases we are obliged to resort to opium, which, if more powerful as a narcotic, also may do more harm than belladonna. The preparations of morphia are perhaps the best form of opiate, and they should be combined with ipecacuanha. In the latter stages a change of air is almost a specific; any kind of change, and although only to the distance of a few miles, will sometimes entirely remove a cough that has baffled all medicines.

In the convulsive coughs of adults, which resemble whooping-cough, I have several times found the pilula aloës et asafetide twice a day, with a galbanum or pitch plaster to the chest, cause most effectual relief. Where the complaint is more obstinate, and the nervous system takes up the habit (as in chorea and other convulsive diseases), the metallic salts, subnitrate of bismuth, nitrate of silver, or ammoniacet of copper, will sometimes succeed in restoring the balance; and they may generally be aided by the shower bath, country air, and exercise, and other means which diminish the mobility of the nervous system.

We have seen how remarkably inflammation alters the secretion of the bronchial membrane; but it may also be altered without there being any distinct signs of inflammation. Thus in cases of what in this country is generally called humoral asthma, the attack and its removal are too sudden to be ascribed to inflammation; and after death there are not found any appearances that betoken an inflammatory character; nay, the membranes are often paler and thinner than usual. Although such affections may originate in, or be occasionally complicated with, bronchitis in some of its forms, yet they are not essentially so, and may with more propriety be called diseases of secretion. The most remarkable of these are the *pituitous* and *dry catarrhs* of Laennec.

Pituitous catarrh, bronchorrhœa, or bronchial flux, is remarkable for the quantity of thin liquid that is discharged by expectoration. This liquid does not appear to differ materially from the natural secretion, but in being thinner, and it is probably this, diluted with a considerable ad-

dition of the most watery part of the blood. This suggests that a laxity of the bronchial vessels may be its immediate pathological cause; and this view is confirmed by the fact that we find it to occur chiefly in those of a relaxed habit of body, who usually drink much liquid, and perspire profusely. I have seen several cases in which a discharge of this kind was plainly vicarious with the exhalation from the skin, sudden changes of temperature, irregularities of diet, disorder of the digestive functions, which depressed the cutaneous circulation, being sufficient to excite it. It is often combined or alternated with a similar discharge or phlegmorrhæa from the nasal membrane, a sort of chronic cold in the head. Sometimes it is connected with organic disease of the heart, which causes congestion in the lungs. It generally comes on without fever, with sneezing, cough, and asthmatic or wheezing breathing. On applying our ear to the chest, we hear all sorts of sonorous and whistling rhonchi, which, as the attack proceeds, become mucous or bubbling; and very little of the natural respiratory murmur is heard. The sound on percussion is generally pretty good; but in severe cases this is also impaired by the profuse quantity of liquid, which from the submucous and subcrepitant rhonchi may be known to extend even to the smaller bronchial tubes. Coming on, as the attack does, suddenly, the dyspnoea is sometimes extreme; but the strength of the respiratory forces being also unimpaired, they generally, by dint of violent coughing, get rid of the fluid as fast as it is secreted, and it comes up clear and frothy, sometimes to the amount of a pint or more.

Although not generally dangerous, this complaint often causes much suffering and discomfort; and if it occur frequently it causes much weakness and exhaustion. This affection is sometimes very obstinate in regard to treatment, especially when it has become established in the habit. The most powerful means are those of diet and regimen. A well-regulated, nourishing, but not stimulating diet, with a limited quantity of liquid; a bracing, but not too cold air; and above all, regular and pretty active exercise, with clothing always sufficient to protect the surface from transitions of temperature, and to favour an equable moderate perspiration, will often do more than medicine. Of remedial agents, mild tonics, the mineral acids, or some metallic salts, such as the sulphate of zinc, in small doses, and the milder preparations of steel, are sometimes beneficial. In other cases some of those medicines supposed to act especially on the mucous membranes prove serviceable, such as ipecacuanha, the balsams of copaiba and Peru. During

the paroxysm, as well as in the stage of effusion in humid bronchitis, I have known the ethereal tincture of the *lobelia inflata* very materially relieve the dyspnoea, and shorten its duration. It is, however, a very uncertain medicine, sometimes causing vertigo and nausea in the dose of ten minims, in others giving relief only in the dose of a drachm. There are many other remedies that now and then appear to do good, such as emetics, opium, blisters, &c., but I have no time to enter into further details on the empirical part of the subject.

The other affection which exemplifies the altered secretion of the bronchial membrane without distinct inflammation, is the *dry catarrh* of Laennec. This term, *dry catarrh*, is a very bad one; and but that the word *catarrh* is now used almost synonymously with cough, it would be totally inapplicable. *Catarrh* means a *flowing down*; and what can be greater nonsense than *dry flowing*? The symptoms of this affection are those rather of asthma than of bronchitis. They vary according to the extent of the affection. In its slightest degrees, it is presented by those individuals who, every morning on waking, feel their breath rather short until they have coughed up a little tough semi-transparent mucus. In its severer degrees, that is, when more of the bronchial membrane is affected, the shortness of breathing may amount to a regular fit of asthma, accompanied by cough; and this may last, more or less, for hours, and even days, and be at last relieved by the expectoration of the scanty tough expectoration just mentioned. There is little or no fever or sign of inflammation present; only sometimes a sense of constriction and heat, or rather of stuffing in the chest; but there is often much gastric disorder; the tongue slightly furred; the uvula relaxed; the tonsils congested; digestion imperfect; the liver inactive; the bowels torpid, or liable to extremes; the hæmorrhoidal veins swelled; and the urine turbid. Excesses in diet, the sudden removal of cutaneous eruptions, suppressed gout, and sudden checks given to perspiration, or any other free secretion, occasionally excite this affection. These causes operating on systems not much disposed to inflammatory reaction, such as those of a torpid habit of body, destroy the balance of the capillary system, and occasion an undue distension or congestion of certain parts of it. This congestion is accompanied by a disorder of the functions of the part, and in the bronchial membrane, especially by a derangement of its secretion. The same kind of passive congestion is sometimes more directly occasioned by organic diseases of the heart, particularly those in

which there is some obstruction in the left ventricle; and here we frequently have the symptoms of dry catarrh.

The physical sign of this disease is a more or less complete suspension of the respiratory sound in the part affected, whilst the chest, at that point, still sounds quite well on percussion. This suspension is caused by the tumefaction of the bronchial membrane, which either of itself, or assisted by the scanty thick mucus before named, obstructs the passage of air in ordinary respiration. Sometimes, during coughing, or violent efforts of respiration, a wheezing or sibilant sound announces that the obstruction is not quite complete; and there will generally be some of the tubes which will give these sounds during common breathing. These signs, and the tough scanty expectoration, characterise this affection; to which I would give the more pathological name, *bronchial congestion*. Coming on, as the attack sometimes does, suddenly, continuing for a few hours or longer, and then as suddenly ceasing, it bears none of the characters of inflammation; but the swelled state of the membrane and its dark colour without other change, which I have seen exhibited after death, imply a full state of the blood-vessels referable to the class of congestions, which may be produced and endure for an indefinite time, and have not the same tendency to definite terminations that inflammatory injections have. It may, doubtless, originate sometimes in inflammatory affections of the same part; but according to my experience it is more commonly the result of disorders of the digestive or other organs which tend to injure the tone of some, or other part of the capillary system. Thus, these will, in some persons, locate this congestion in the capillaries of the face, harming nothing but their beauty; in others the encephalic vessels suffer, whence habitual headaches of an obstinate character arise; in others some part of the alimentary canal is the seat, whence indigestion, hæmorrhoids, or some disorder of the alvine function, ensues. So, too, the urinary or the genital systems may become the place of this congestion; or it may fall on the bronchial membrane, and induce the affection under consideration; and the local determination of the morbid vascular condition is, in individual cases, fixed on particular parts or organs in consequence of prior weaknesses or tendencies, or other circumstances which we cannot now stop to enumerate.

Occasionally bronchial congestion is conjoined with what might seem to be its opposite, pituitous catarrh; but according to the view which we have taken of that affection, the pathological causes of the

two do not differ very widely, the same circumstances causing a loss of tone in the capillaries, being capable of producing a relaxation of their exhalant properties, or a dilatation of their caliber. Or what is more usually the case, some parts of the membrane are affected with one, and some with the other; and the result is the expectoration of much thin glairy fluid, with the little pellets of tough mucus in it. So, too, by a modification in the properties of the congested vessels, they may be excited or relaxed, and relieve themselves by the exhalation of their watery contents; and we accordingly, sometimes, find an attack of catarrh or asthma, at first quite dry, and devoid of any but the tough expectoration, suddenly relieved by a copious discharge of thin frothy fluid. This happens more commonly where the congestion results, mechanically, from disease of the heart.

Depending, as this affection generally does, on constitutional causes, it will require measures which may act on the system, as well as those which may improve the condition of the affected membrane. A due management of the diet, avoiding all acid, rich and irritating articles of food; regulating the secretions by mild aperients and alteratives; and subsequently employing tonics, which, by maintaining the balance, may increase the general tone of the circulation, generally constitute the most important part of the treatment. With regard to the measures addressed to the congested membrane, it is not found that the ordinary remedies for bronchitis are of much avail. Blood-letting produces little impression. Dry cupping, and other means of derivation, may be of more use. Stimulating and bracing applications to the whole surface of the chest, pitch plasters, flying blisters, and stimulating embrocations, are occasionally of temporary advantage.

There are, however, means of increasing the flow of the bronchial secretion, which, as temporary remedies, are of more avail than any hitherto named. We have before noticed the property which alkalies have of determining to the bronchial surface, and we have now to notice in addition their attenuant or dissolving power, which diminishes its tenacity, augments its quantity, and thus facilitates its expulsion. These remedies thus not only remove the obstructing mucus already secreted, but by favouring its looser flow, they tend to unload and reduce the congested membrane, and thus to relieve the dyspnoea that arises from its tumefaction. For the knowledge of these remedies I am indebted to Lænnec, who seemed, however, at a loss to account for their efficacy. I am far from wishing to extol chemical medicines in

general; but I cannot but think that we may bring chemistry to our aid in the present instance, to explain the action of alkaline attenuants. We know that we can, by the administration of alkaline medicines, render the urine alkaline, and increase the alkaline qualities of the blood. Now there is no solvent of mucus more effectual than alkalies, and it is easy to perceive that an alkaline state of the bronchial secretion can scarcely be compatible with the formation of tough solid mucus. I have found these remedies very effectual, and I am in the habit of giving either the liquor potassæ (℥ xx. to xxx.), carbonate of soda (gr. xv. to xxx.) or carbonate of ammonia (gr. iij. to vj.), according to the character of the case, three or four times a day, with squill, ipecacuanha, or colchicum, and some narcotic, according to the general state of the system and the prevalence of particular symptoms.

I need not detain you about some other bronchial discharges of rare occurrence. Bronchial hæmorrhage may occur to a considerable amount, independent of any permanent disease; and the fluid blood in the tubes will occasion a mucous rhonchus. In character it resembles epistaxis, hæmorrhoidal flux, and other bloody discharges from mucous membranes; but in this case, besides the weakness resulting from loss of blood, when this is considerable, there may be some danger from the interruption which it might cause to the function of respiration. The treatment, as in other hæmorrhages, will depend on whether the hæmorrhage is sthenic or asthenic—active or passive. These points are fully treated of by authors.

PRACTICAL OBSERVATIONS

ON

DELIRIUM CUM TREMORE.

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“*Scire vere est scire per causas.*”

It ought, in the present advanced state of medical science, to be altogether unnecessary to insist on the immense importance, as directing our therapeutic means, of an accurate knowledge of the causes by which any given train of symptoms is produced, or of the lesions wherewith particular diseases may be complicated. Yet that it is not unnecessary is but too evident when we observe how many there be who seem

altogether ignorant of the fact, that precisely similar phenomena may result from the most opposite causes, and that the accompanying lesions are not unfrequently of a totally different nature from that of the primary disease, and consequently require a most dissimilar treatment.

These observations are, we believe, but too clearly illustrated in the disease termed *delirium tremens*, or *delirium cum tremore*. The opinion entertained by many, that it is invariably an asthenic condition of the system, and consequently admits, and even requires, in all cases and stages, a liberal and almost indiscriminate employment of stimulants and narcotics, but too surely bears us out in this assertion.

As long as we view the phenomena constituting this disease as indicative only of one and a peculiar condition of the nervous system and general habit, as occurring only in individuals who have done an abiding injury to their system by the *habitual* abuse of intoxicating liquids, and as caused only by the want of an accustomed stimulus, or by some sudden shock, so long will our views be complicated and incorrect.

It is necessary to analyze that we may distinguish, and if we prove, as is our object in this paper, that *delirium tremens* results from very different causes, that it is frequently complicated with, or even caused by inflammatory affections, that in very many cases the disease is curable by antiphlogistic means only, and that in some of these instances narcotics and stimulants are not only unnecessary but at the same time injurious, then shall we have taken a step in effectually distinguishing that which ought never to have been confounded.

Certainly, the most usual form under which *delirium tremens* appears is, as occurring to individuals prone to the abuse of spirituous liquors, and in whom, from the absence of their accustomed stimulus, or from some grave or sudden shock, the nervous system is thrown into a morbid condition, whereby the vital manifestations of the system are disordered, giving rise to those phenomena to which, collectively, nosologists have assigned a peculiar designation. In such cases we usually observe, after death, no appearances to which we can legitimately refer the phenomena manifested during life; or, if any peculiari-

ties do exist, they are neither constant in their occurrence nor analogous in different cases; such cases may be termed pure or simple; they are mostly curable by narcotics and stimulants, and being sufficiently recognised and understood will not be again referred to.

Those, however, which it is our purpose here to examine differ from the preceding, not only in occurring occasionally in individuals of the most abstemious habits, but likewise (and this is the point on which we would particularly insist) in invariably presenting symptoms during life, and appearances after death, which unequivocally denote a well known, because appreciable, organic lesion—that lesion being either gastritis, enteritis, or both conjoined.

To complete this cursory view of the disease, it is requisite here to mention those cases in which there exists an evident degree of cerebral inflammation, and which usually require, for their successful treatment, antiphlogistic means, either general or local, conjoined or not, as the case may be, with the simultaneous employment of narcotics and stimulants.

CASE I.—A gentleman, a member of the University of Oxford, aged 28, of extremely temperate habits and of nervous disposition, received, during a temporary stay in London, some distressing intelligence. He repaired to the house of a friend who, seeing his depression, after much trouble, prevailed on him to take some wine. His spirits rallied considerably, he continued drinking for some hours, and left in a state of severe intoxication: he reached home about three o'clock in the morning, and was put to bed by his servant: he got no sleep, was exceedingly ill during the night and throughout the following day, being restless and delirious. He was seen the following morning, the second of his illness: his countenance was expressive of deep distress; there was considerable tremor of the extremities, the tongue could be with difficulty protruded, its centre was covered with a dirty and creamy mucus, its sides and tip being of a dark red. He is constantly getting out of bed and walking about the room, yet can be easily led back again; he talks loudly of an imagined wrong done him by his brother, and if interrogated upon any point at first hesitates, but answers after a

few minutes. He complains of intense thirst, heat of mouth and throat, with nausea; there is some fulness about the epigastrium, with great tenderness; the bowels have not been evacuated since yesterday morning. There is considerable headache, the eyes are suffused, the pupil contracted, and the ball of the eye rolls rapidly about in the orbit; pulse 110, as nearly as it can be counted.

Fifteen leeches to be applied to the epigastrium. An enema of Sulph. Magnes. et Infus. Sennæ. To take nothing but cold water.

In the evening he was much the same; tremors still considerable, and the same agitated manner; has had no sleep; the epigastric tenderness appears but slightly diminished. He experiences much relief from the cold water, which is taken in small quantities, but at very short intervals. The heat and clamminess of the mouth make him very irritable; pieces of ice, of about the size of a walnut, were therefore ordered to be put in fine muslin and held to the mouth until dissolved.

Third day.—Has slept two or three hours; the tremors are lessened, as are likewise the cerebral symptoms; he still complains of pain and heat in the stomach, if long without the ice; the epigastric tenderness is still considerable; pulse 106.

Applicetur hirudines octo epigastrio.
Injiciatur enema commune.

Fourth day.—In every respect better; has slept well; the tremors are much diminished, as are also the symptoms of gastric irritation; the bowels twice opened; the headache is nearly gone; he is more collected, and thirst is but little felt. He was now ordered slightly acidulated lemonade to be taken *ad libitum*, and a purgative enema.

On the fifth day he took some cold chicken broth, and from this time went on gradually improving without the employment of stimulant or narcotic. The bowels, however, for many days subsequently, remained very inactive, and the secretions being manifestly disordered, he was recommended to take, every third night:

Pil. Hydrarg. Ext. Taraxaci et Ext. Coloc. comp.; following it the ensuing morning with Magn. Sulph. Pot. Sulph. Pulv. Rhei. Inf. Sennæ et Inf. Gent. c.

CASE II.—G. B. aged 17. Oct. 27,

1837. This patient presented all the symptoms of delirium tremens, want of sleep, restlessness, trembling of all the limbs, suffused eye, creamy sort of mucus covering the centre of the tongue, the sides and tip of which were red; has been unruly all last night and during the whole morning, is very thirsty, the epigastrium is exceedingly tender, and the bowels are constipated; pulse 118, weak, with some wiriness; pupils contracted. On inquiring into the history of this case, we were informed that about a week previously he had been apprenticed, and on that occasion had had several friends with him; they drank freely, he became much intoxicated, and, as his mother believed, for the first time. Since that period the present symptoms have been gradually coming on; his nights have been without sleep; his actions have all become unsteady, so much so that he cannot now carry a tea-cup to his mouth without spilling its contents; and his temper is very irritable.

Applicetur hirudines duodecim epigastrio. Ol. Ricini f. 3vi. statim. To take iced lemonade or cold water.

27th.—Much the same; bowels not yet relieved.

Injiciatur enema Ex. Magn. Sulph. Infuso Sennæ et syr. Rhamni.

28th.—Bowels freely opened, somewhat better, not so unruly, has yet slept but very little, the epigastric tenderness only slightly diminished.

Applicetur hirudines octo parti maxime dolenti.

30th.—Obtained much relief from the leeches, the symptoms are all diminishing, he has slept freely, bowels open, epigastric tenderness nearly gone, and the tremors are only observed in the hands, when held out at a distance from the body. To take a pint of cold chicken or veal broth daily.

On Nov. 5 he was so much better as to render our further attendance unnecessary. Having since met him in the street, he informs us that he is perfectly well, and has suffered no relapse.

CASE III.—Jan. 2, 1838. H. N., carpenter, residing in Stanhope-street, Clare-market, a man usually of temperate habits. On Christmas-day he got intoxicated, and suffered so much on the two succeeding days from its effects, that he again had recourse to beer and stimulants, in the hope of removing the disagreeable feelings under which he

laboured. In this course he proceeded until Saturday evening. On Sunday he kept his bed, was much agitated in his manner, and very restless; slept but little on Sunday night; he got up on Monday (Jan. 1) and proceeded to his work-shop; he trembled so much as to render him incapable of work, and his employer sent him home.

The following day (Tuesday), we visited him, and found him sitting up in his bed; his extremities, tongue, and, indeed, the whole body, being at the same time in a constant state of tremor. He is vigilant and suspicious, watching every movement around him—his temper is very irritable, and he is constantly finding fault with and abusing his wife and children. The skin is warm, and covered with a clammy perspiration, but his cheeks are pale, and his eyes bright, though inexpressive. He frequently gets out of bed and walks about the room, but, with a little reasoning, at length goes back again. He has had no sleep since Sunday; the pulse is small, rapid, and irritable, 120; the tongue smooth and moist—tremulous and somewhat pointed. He complains of thirst and slight pain in the epigastric region, increased by pressure, and on taking any warm drinks. Finding from his wife that he was not addicted to the daily employment of intoxicating drinks, and that these symptoms supervened upon an unaccustomed debauch—bearing in mind, at the same time, the insidious latency of gastric affections in many cases, and the existence in this individual of symptoms denoting disorder of the stomach—we were induced to attribute the symptoms to gastritis, and treat it accordingly. The following was the prescription:—

Hirudines x. et postea cat. sinapis epigastrio. *Ol. Ricini* ʒvi. statim.
Enema c. Sulph. Magn. Infuso Sennæ et Syr. Rhamni.

R. Hydrargyri c. Cretâ gr. iv.; *Pulveris Rhei*, gr. v. *M. ft. pulv. 6tis horis exhibendus.*

Jan. 3d.—Slept a little after the removal of the mustard poultice; the tremors are but slightly, if at all diminished; bowels freely open; and the epigastric tenderness and thirst diminished. To take cold barley water, apple tea, or lemonade; repeat the castor oil to-morrow morning, and continue the powders.

4th.—In every respect better.

6th.—Tremors greatly diminished;

sleeps comfortably, appetite returning; but he experiences slight pain, a feeling of tension and heaviness in the epigastrium after eating; bowels confined.

R. Hydrarg. Chloridi gr. v. nocte: et haustus ex Infuso Sennæ Magn. Sulph. and Jalapa. Cras mane.

9th.—Tremors entirely gone; indeed he appears perfectly recovered from his original attack, but still complains of gastric uneasiness; bowels still constipated.

R. Bismuthi Oxydi gr. v. bis. die.

Pil. Hydrarg. ʒi.; *Ext. Coloc. Comp. ʒiij.*; *Ext. Hyocyami* gr. viii. *M. et divide in pil. xvi. Capiat duas omni nocte.*

20th.—The bowels are becoming more regular, his tongue is cleaner, but he still complains of severe pain after eating; has had no return of sleeplessness or trembling.

R. Morphiæ Muriat. gr. i.; *Pulv. Tragac. comp. ʒss. tere bene simul et per-misce.* *Fiat. pil. xii. Sumat unam ter die.*

Has been seen this day (25th); the morphia has acted most beneficially, and the gastric uneasiness has nearly vanished.

REMARKS.—The three cases above mentioned will be amply sufficient for the purpose which we have in view, that of affording a specimen of delirium cum tremore, originating in gastritis; or, in other words, of delirium tremens, as symptomatic of, and secondary to, gastritis. In this mode, then, we view gastric inflammation as the proximate cause; an opinion by no means new, inasmuch as the founder of the so-called "physiological school" insisted strongly, many years since, upon this connexion. Here, however, as in many other instances, has Broussais been led astray by his favourite theory—by his love of generalization; he has looked on this relation as of more frequent occurrence than we observe in nature. What he considers the rule, we must look on as the exception. It may be difficult to determine what is the precise numerical relation of such cases with others of a different character; but the following may be taken as a general approximation. Delirium tremens usually presents itself as a disease of debility, and is benefited by stimulants and narcotics. Next to these, in point of frequency of occurrence, are cases complicated with

cerebral inflammation; then follow those cases of the disease of an asthenic nature, but accompanied or complicated with gastric inflammation; and, lowest on the scale, that is to say, of least frequent occurrence, are cases such as narrated, and presenting the connexion on which Broussais insists. A somewhat similar view to that which we have just expressed has been taken by Dr. W. Stokes, of Dublin, and it gives us great pleasure in stating that our attention was first directed to this interesting point by a perusal of that gentleman's excellent lectures on the theory and practice of medicine: "A patient," says he, "who is in the habit of taking wine or spirituous liquors every day in considerable quantities, meets with an accident, or gets an attack of fever; he is confined to bed, put on an antiphlogistic diet, and in place of wine and whiskey punch, gets whey and barley water: an attack of delirium tremens comes on, and symptoms of high cerebral excitement appear. Another person, not in the habit of frequent intoxication, takes to what is called a fit of drinking, and is attacked with delirium tremens. In the first case the delirium arises from a want of the customary stimulus; in the second, from excess. In each the cause of the disease is different, and, consequently, with this view of the subject, it would be a manifest departure from sound practice to treat both cases in the same way."

How are we to discriminate between these different cases. What are the sources of our diagnosis? First, *the history*. If we learn that the patient is of temperate habits, that he is unused to the daily employment of wine, spirits, porter, or the intoxicating beverages,—if he be not an habitual opium eater,—and if the symptoms have supervened on a solitary and unaccustomed debauch, or on what is called a fit of drinking,—then there is a strong presumption that the stomach, from these circumstances, has been over excited, and that its increased physiological action has passed into a true pathological state—that of inflammation, which has been productive of the symptoms constituting the disease. The second source of diagnosis are the *symptoms*. If, in addition to the phenomena necessary to constitute our idea of delirium tremens, we have superadded many of those which indicate an excited state of the stomach,

as nausea, vomiting, heat of throat and stomach, great thirst, desire for cold drinks, fullness of the epigastrium, tenderness on pressure, costiveness, with red and pointed tongue, then our supposition becomes more and more confirmed. Here, however, it is necessary to guard against an error of judgment. Many or all of the symptoms indicative of gastric inflammation may be obscure, or altogether wanting, and yet the disease itself be present. The latency of these affections is well known, and their obscurity is, moreover, much increased; or, in other words, they are, as it were, kept in abeyance by the more prominent symptoms of disturbance in the nervous system. It behoves us, therefore, in all cases when the history has raised suspicions, to be minutely careful in our investigation of symptoms. The third means on which we should rely for a correct diagnosis are the effects of remedies. If both the history and symptoms are insufficient for our decision, then should we cautiously employ different remedies; according as stimulants increase or diminish the symptoms, or as leeches, cold water, &c., produce the same effects, so should our judgment be decided. These are all the means applicable to our particular patient: the morbid anatomy comes in only for the "confirmation or connexion of our previous opinions," after all the efforts of our art have proved unavailing*.

A very few words will suffice for the treatment; reference to the detail of each case will be sufficient to indicate the course which we consider necessary. A point in practice may, however, be agitated; we refer to the employment of morphia in these cases. Morphia appears to us injurious when given in intense inflammation of the stomach. The symptoms will be best alleviated by the usual antiphlogistic means—leeching, cold water, ice, &c., but after the violence of the inflammatory symptoms has abated, when it has arrived at the subacute degree, then we know of no remedy possessing equal power with that now under consideration.

It is our intention, at an early period, to forward some cases and remarks illustrative of delirium tremens, as occurring in confirmed drunkards, but united with gastritis as a complication.

* See further Principles of Diagnosis, by Marshall Hall, M.D. F.R.S. L. and E. London 1834, 2nd ed.

CASE OF FUNGOID DISEASE OF
THE KIDNEY,

WITH THE POST-MORTEM EXAMINATION.

To the Editor of the Medical Gazette.

SIR,

I SHALL feel greatly obliged by the insertion of the following case and dissection in your valuable journal.

I am, sir,

Your most obedient servant,

JOHN GORHAM, M.R.C.S.L.

225, High Street,
Monday, 29th Jan. 1835.

On the 23d June, 1836, Alfred Trendall, aged seven months, was brought to Guy's Hospital as an out-patient, on account of a large tumor in the abdomen. The mother had always suckled him. The two central inferior incisors were cut when he was six months' old.

A month since he had a bowel complaint, which lasted about ten days; the motions were at this time watery and greenish; he vomited also a yellowish matter three or four times a day; and when his digestive organs were thus deranged his mother observed that he looked yellow. He has had no other disorder incident to infancy.

A week since, the mother observed, while washing him, a swelling in the abdomen, for which an emetic and a warm bath were prescribed by a medical man.

On examining the abdomen, I find a large tumor occupying all the regions, but situated rather to the right side, extending from the ribs above to the iliac fossæ below. A depression exists between the margin of the ribs and the tumor, which is more evident when pressure is made with the fingers; this depression renders the origin of the tumor from the liver doubtful.

The abdomen feels hard, there is no fluctuation, the veins are turgid, and the general aspect of this cavity is ascitic. He passes less urine than usual. Since the swelling was first observed it has increased rapidly. Slight cough; skin hotter than natural.

Hyd. Cretâ, gr. iij. o. n. Liq. Am. Acet.
et Sp. Æth. Nit. t. d.

June 10th.—Abdomen 22 inches in circumference; no sickness; motions in large quantity, and of a dark green

colour; countenance pale, dirty colour; slight cough; resp. 80; pulse 188. Therm. Fah. 99.—Pergat.

July 7th.—Abdomen 22½ inches; vomiting; motions in large quantity; resp. 60; pulse 148; ther. Fah. 95; some emaciation; frequent moaning.

11th. Abdomen 22½ inches; frequent moaning.—Pergat.

As the mother supposed that little could be done for the infant, I saw him no more till August 11th, when I was informed that he had died the day before, and I was requested to examine the body.

Post-mortem, 24 hours after death.—Considerable general emaciation.

Head not examined.

Chest.—Thoracic cavity very much encroached on by the abdominal tumor; the summit of the diaphragm rising to an imaginary line, extending from the under edge of the third ribs, where they join with their cartilages.

Right lung.—Pale, anteriorly crepitant over whole extent; reddish posteriorly, from cadaveric infiltration, and it was emphysematous.

Left lung presented, on the posterior part of its apex, a small whitish elevation of the size of a silver penny, which, when cut into, allowed a cheesy matter to escape. On making an incision into the substance of this lung, I could not discover any more traces of tubercular disease: it was emphysematous.

Abdomen.—Greatest circumference 23 inches, the length of the entire body being 27 inches: when its cavity was laid open, about two ounces of a serous fluid escaped. On turning down the flaps, I found the peritoneum inflamed and adherent for the space of one square inch to the tumor. The following parts now presented themselves to view:—

The lower portion of the ileum first attracted attention. It was distended with flatus, and traversed the fore part of the tumor obliquely, from the left side below to the right above, and terminated immediately under the right lobe of the liver in the cæcum; from this point the ascending colon passed transversely, taking the place of the transverse colon, which latter was lost amongst the small intestines, and these were lying to the left of the tumor. The only part remaining exposed to view, before displacing the viscera, was a large tumor situated rather to the right of the abdomen, extending from the liver to the pelvis, displacing the

large and small intestines. It was of various colours externally, being in some places livid, bluish, and streaked with beautiful red vessels; in others it had a paler or yellowish hue. On the left side it was lobulated and nearly white. It seemed to originate in the right kidney, as no traces of this gland, as it exists normally, could be found. It weighed about $4\frac{1}{2}$ lbs. The left kidney was in its natural position.

For all that follows I am indebted to Thomas King, Esq., curator of the Museum of Guy's Hospital.

The tumor was as large as an adult woman's head. The ureter was seen slightly dilated above, and much contracted below, coursing over the inner part of the tumor; the capsula renalis was normal. The tunica propria of the enlarged kidney was very extensively separated from the gland, at different parts, by a sanguineous, serous, and inflammatory effusion. The external surface of the membrane, at the upper part, seemed to have produced a large cake, like a six months' placenta; the texture of which was firm, medullary, fibrous, and cystiform. There were some traces of venal substance pretty generally beneath the proper tunica, pale, compressed, and rather dense; but, for the most part, a section of the kidney presented a complete fungoid degeneration.

The pelvis was dilated, and filled with various more or less vascular excrescences of different sizes; in the interior of these, the formation of compound cysts was evident enough. The other parts were more sensibly medullary in appearance, and contained portions in different stages of dissolution.

There appeared to be a few simple renal cells.

ANOMALOUS CASE OF HYSTERIA. —MESMERISM.

To the Editor of the Medical Gazette.

SIR,

If you will have the kindness to give insertion to the following letter and case, you will much oblige

Your obedient servant,

S. SANDYS,
Surgeon.

4, Francis Terrace, Kentish Town,
Feb. 5, 1838.

To the Baron Du Potet de Sennevoy.

SIR,

I FEEL it but due to you to acknowledge publicly the source from whence I derived such a knowledge of the science of mesmerism as has enabled me to pursue the course of inquiry, and to obtain the results, detailed in the following case.

Surprised at the phenomena exhibited by those patients whom you treated at the University College Hospital, where, through the kindness of Dr. Elliotson, I was invited to attend, I could not hope that so shortly afterwards I should thus, in my own practice, see the beneficial results of mesmerism.

I have only to add, that my attention was not directed to the respiration (as was that of an eminent physician, who has already given us the results of his experiments), nor indeed had I any other object in view than that of simply imitating, as far as I could, the method of manipulation practised by yourself.

I am, sir, with much respect,

Yours &c.,

S. SANDYS,
Surgeon.

4, Francis Terrace, Kentish Town,
Feb. 5, 1838.

A. S., aged about 42, a female of quiet and sedentary habits, her occupation being that of a sempstress, has for several years been subject to dyspepsia, with a remarkable though variable state of the tongue; the whole or a part of the skin falling off about once in ten days, leaving the exposed surface raw and painfully sensitive; in 24 to 36 hours it appears as usual again. For this she has been under constant treatment, of which daily exercise in the open air formed a part. About the middle of November she was affected with a constant vomiting, partially relieved by creosote (the effervescing saline having failed), and completely removed by hydrocyanic acid. This was accompanied by pain and weakness in the back and loins. About a month afterwards she was affected with retention of urine, requiring the constant use of the catheter. Six or seven days after this she was observed one evening to rise from her bed (about nine p.m.) in a state bordering on somnambulism; she put her shawl over her shoulders, saying that she must go out and take a walk; this she was with difficulty restrained from doing, as her

strength was much augmented at the time, and she declared vehemently "she would go out, as Mr. Sandys had ordered her." After a few minutes she began to tremble violently, clinging for support to any one standing near her: her strength gradually decreased, and she suffered herself to be laid upon the bed, when she shortly awoke sensible. This attack did not fail to come on every evening, at first for a few days spontaneously, afterwards in some way dependent upon the use of the catheter, as it always supervened immediately after that operation, though the time was purposely varied at uncertain intervals, between half-past eight and half-past ten, beyond which time, if the operation be delayed, the attack appears to come on spontaneously.

During the attack she exhibits a most pointed aversion to all females, not shewing any friendly recognition of her sister, or any of her friends or acquaintances, positively denying their identity, and declaring that they are all in league to keep her in confinement.

This paroxysm daily increased in violence and induration*. Persuasion, fear, and the cold "douche," having failed to quiet it, recourse was had to Battley's sedative solution, in half-drachm doses, given shortly before the expected time of the attack; the strait waistcoat was applied, and she was tied in bed. The attack came on as usual, but gradually gave way under the influence of the opiate; she slept about three hours. Her state, during the next and three following days, was, however, such as to forbid a perseverance in its use; she vomited her food constantly; her head was in racking pain; she felt worse than usual, and, to use her own expression, "perfectly wretched."

The duration of the attack at this time, if left uncontrolled, was usually two and a half to three hours, when she gradually became quieter, though she still continued muttering, and at length became sensible, without, however, having once closed her eyes.

* So violent had she now become, and so malicious were her expressed intentions during the attack, both as regarded the destruction of herself and sister, the burning of the house, the frequent attempts she made to jump out of the window, and to dash her head against the wall, that more active means of restraint were thought necessary.

I now felt that as mesmerism had the sanction of so eminent a physician as Dr. Elliotson in these cases, I was quite justified in trying it.

On the evening, therefore, of December 27th, the usual passes were made, and continued for about fifty minutes, beginning soon after the commencement of the attack. From that day to this, a period of more than a month, the manipulations have been followed by one uniform result—a state of coma (if I may so call it), or profound mesmerie sleep, coming on gradually after a certain length of time (requiring fifteen or twenty minutes before any effect is observed, and about thirty-five to produce the full effect), the respiration becoming slower, and so gentle as to be quite inaudible. At length a deep-drawn sigh, followed in about five minutes by a second, after which we find the extremities generally so rigid, that moving one leg moves the whole body. There is also always a great diminution, if not a total absence, of sensibility in the skin, as evinced by the following tests: pinching, pricking, scratching, and tickling, particularly the soles of the feet, which at other times she cannot endure.

Her sense of hearing seems equally dull, shewing no sign of perception when her own name is shouted in her ear; not the slightest movement even of the eyelids being visible when a watchman's rattle is suddenly sprung at her bedside. Yet it has appeared to me several times that, when in this state, certainly when in progress towards it, she is sensible of any exertion of the voluntary muscles, even of resistance without motion, and this after all the ordinary sensibility of the skin appears extinct.

She shews no consciousness when violently shaken, and the use of the scarificator (having been cupped on the loins when in this state) produced a short expression of uneasiness rather than of pain; and she immediately relapsed into her comatose condition, not having in the morning the slightest recollection of what had passed.

The catheter, if introduced while she remains in this state, has the effect of partially rousing her, requiring a few more passes of the hand to quiet her.

She was left for two successive nights

without the mesmerism; the attack each night came on as usual, lasted about three hours, and left her low, exhausted, faint, and sleepless; each morning she expressed herself as not being nearly so well. Next day the mesmerism was resumed, and she was quieted as usual.

About ten days afterwards the attack was one evening allowed to remain for forty-five minutes, at which time (her violence being undiminished) the usual passes were made, and were soon followed by their ordinary effect.

From this mesmeric sleep she commonly awakes in about three hours, quite sensible. I should here say, that the above phenomena have as frequently been produced by my pupil, Mr. W. Whitehouse, as by myself; indeed it was he who first, by persevering for about fifty minutes, produced the marked effects above described, though I had several times previously observed a slight and transient drowsiness or quietude result from a few passes.

The introduction of the catheter in the morning has once or twice been followed by a short and slight attack, similar to that of the evening; this has likewise sometimes preceded the operation.

There has throughout been a remarkable temporary loss of power in the lower extremities. In the day-time she can walk firmly; whereas at night, during the attack, when she sets her feet on the floor, the knees sink under her, and she falls instantaneously, without staggering, and without attempting to save herself.

The same results have uniformly followed the mesmerism if practised in the day time, independently of the attack or of catheterism—the same succession of symptoms—the same insensibility to external impressions. It, however, requires less time to effect this, twenty to twenty-five minutes being sufficient. From this day-sleep she awakes in about three hours, much refreshed.

In conclusion I have only to add, that should any member of our profession wish to convince himself, and to be an eye-witness of the results obtained, I shall be happy to shew him the case at any time.

PHARMACY LAWS.

ROYAL COLLEGE OF SURGEONS IN IRELAND.

To the Editor of the Medical Gazette.

SIR,

THE subject of the change proposed to be made in the character and functions of the College of Surgeons in Ireland, and of its members, has been now for some time before the public; and being a matter of much importance to the public itself, as well as to the profession, it has excited a degree of general interest which contrasts strongly with the indifference usually displayed towards questions of medical policy, but certainly not greater than it merits. The question is one of deep interest to the members of the College of Surgeons, and it is the duty of every member who feels that either the welfare of the public or the character of his College is at stake, to adopt the course which appears to him best calculated to protect both. Hitherto I have looked on; because it did not appear to me that the time had arrived when interference was likely to prove seasonable or effectual. As yet, in this, as in most other instances in which the interests and views of numbers are involved, the tide has flowed in one direction: the minds of the public, and of the profession at large, have been carried away by the representations which have been industriously put forth by one side, and the claims of reason and dispassionate inquiry have been lost sight of in the ardour of pursuit; but since the first impulse has now spent its force, and in some degree subsided, the minds of all parties must be in fitter mood for calm and impartial investigation. The intentions and plans of the advocates of the change have been in part made public, and the time has at last come when their value and title to support may be at once strictly and temperately weighed, and the public mind disabused of those erroneous impressions (if any) which may have been made upon it.

You are aware, sir, that there exist, in Ireland, three medical corporations, invested severally with distinct, appropriate, and independent rights, functions, and powers, all conferred and guaranteed either by royal charter or by act of parliament. Hitherto the Col-

lege of Surgeons has been, and still is, a corporation devoted exclusively to surgery, holding, as by its charter it is bound to do, the practice of pharmacy to be a distinct branch, foreign to it, and belonging by law to another body. Now, however, it is sought to alter its original intention and character, and to convert it into a pharmaco-chirurgical corporation for licensing general practitioners, who shall practise both as surgeons and apothecaries, and deal in and vend medicine.

The laws which it is proposed, in the name of the College of Surgeons, to establish and impose upon the College and the apothecary branch of the profession, so far as they have yet transpired, have been published in late numbers of certain newspapers, in articles headed "Royal College of Surgeons" — "Pharmacy Laws." Put forth, as this has been, under the name of the College, the public are, of course, led to suppose that it has been published by its order, and with its sanction. This, however, is not the case, and it would behave the College to see to it; unless it would be sponsor for every one who has the boldness to make an unauthorized and surreptitious use of its name.

I observe that you have been favoured with a copy of the articles; it will, therefore, suffice to refer those who may wish to know its contents, to your number of the 20th inst. The other provisions which it is intended that the act of parliament shall contain, have not been made public: that, however, there are others, of a most important character, I can positively state, and which will require the most serious consideration. But against the code here propounded for the regulation of pharmacy, I protest in the strongest terms, for reasons which I will presently state. Meantime I must advert to a matter which requires notice, before I proceed further.

It has been stated in the announcement of the proposed laws that it is "not for the purpose of deriving income from the sale of them" that practitioners are to dispense medicines. The interpretation put upon this by those ignorant of the real intentions of the parties is, that the members of the College of Surgeons intend not to charge for the medicines which they shall dispense to their patients; but this is as great a mistake on the part of the public, as I

fear the assertion was deceitful on the part of the writer; for, 1. The report of the committee contained no such statement. 2. The intention of the proposers avowedly is that the members and licentiates of the College shall have power to charge for the medicine they dispense, as well as power to dispense it. 3. It is notorious that the prospect of making the money which is now made by the apothecaries, and even in the same manner, is one of the considerations by which it has been attempted to induce members and licentiates to consent to the measure. Now let me ask is it to be believed that practitioners shall have power to charge for their medicine, and yet give it for nothing; or that charging for it, they will not charge as much as consistently with their interest they can do! If the public think so, it will soon be undeceived. It is not in nature that a man wanting money, and having a legal opportunity by which to make it, shall refrain from taking advantage of that opportunity when it may offer. But though the intention of not making money on the sale of their medicine were seriously entertained, it must prove, under the proposed system, impracticable; and either the one or the other must be abandoned. Under this system the practitioner alone could practise; and if he be not remunerated by fee for his service, he must indemnify and remunerate himself through his medicine. Now there is a large portion of the community which is altogether unable to fee a practitioner even to the most moderate amount, and pay also for medicine, and which can remunerate him only by what he may make upon it. All this portion, which really comprehends the greater part of the population of this country, the committee propose to prevent henceforth from applying to the regular apothecary, and to compel to have recourse to their practitioner for relief. How, I ask, is the practitioner to deal with patients of this class? They cannot fee him; and if he make not money by his medicine, how is he to be remunerated? Will any one believe that he will, or expect that he shall, keep a pharmacy for the supply and service of patients from whom he is not to receive a return, or that he will be either generous or fool enough not to take advantage of the powers with which he is to be invested, and remunerate himself by

taxing the medicine which he shall dispense? Doubtless he will; or if he do not, he will refuse to attend such patients, who would then be, by the wisdom of the committee, deprived of all resource.

Having shown that it is intended that the practitioner of the new system shall charge for his medicine, and indeed that he must do so, as well to protect the public as himself, I proceed to state my objections to the entire proposition. First—The College of Surgeons have no right, authority, or sanction, to take upon itself to regulate or propose laws for the regulation of the practice of pharmacy. Its authority is expressly defined by its charter, and does not extend beyond the regulation of the profession of surgery. The regulation of the practice of pharmacy and of the profession of the apothecary, belongs, and is by law conferred upon and entrusted to, the Apothecaries' Hall, and it is not less an encroachment upon the rights of the Hall, by the College to assume authority to interfere in these matters, than it would be of those of the College, if the Hall took upon itself to promulgate laws for the regulation of the profession of surgery. Nor ought it be countenanced for a moment. Let the members of the body, whether individually or collectively, lay before parliament the grievances or disabilities, if any, under which they labour, and seek from the legislature a remedy; but the College have no right to take upon themselves, in their corporate capacity, to make or propose laws for the regulation of a branch of the profession with which the legislature have not given them authority to interfere. If such a proceeding be once sanctioned, where or when is it to stop? Not until all the institutions of the country have changed places, or have been all concentrated in the College of Surgeons. Secondly—The members and licentiates of the College are bound by oath "to the utmost of their power to endeavour to promote the dignity of the College," and "that they will not at any time *hereafter* practise the business or profession of an apothecary, or sell drugs or medicines within the city of Dublin, or at any place within ten miles thereof, so help them God."

It is not necessary to dwell upon the obligations of this oath. No one, not even the advocate of the change, attempts to deny that the dignity of the

College, and the station of its members, would be compromised by their dispensing and selling medicine, and by its conversion from a college of pure surgeons into one of apothecary-surgeons. And if oaths are to bind at all, I know not how any member or licentiate who has taken that just cited can dispense or sell medicine within the prescribed limit, and not account himself a perjured man. The oath is express, positive, and unconditional. It is taken voluntarily and advisedly, and no opening for evasion is left. If gentlemen find the obligations which they have taken upon themselves inconvenient or irksome, they should, when they propose to throw off one part of the compact, put off the other also. Let them cease to be members of a body the functions and privileges of which do not suit their views and wishes, but to which they pledged themselves when they entered it, and let them go to another better adapted to the purpose they have in view. When to the preceding considerations we add the fact, that the College imposed the oath in question upon themselves; that their first charter did not contain it; that it was introduced at their own desire, and that but a few years ago into their new one; what justification have they now for seeking to get rid of it?

Thirdly—The proposed change has not received due consideration or sufficient sanction from the College or the profession. Here, doubtless, I shall be told that "it was passed at an unusually large meeting without a division," and "that the committee had received communications from all parts of the country, expressing satisfaction," &c. But let us examine this matter more particularly, for I like not such general statements. The College contains between one hundred and thirty and one hundred and forty members; and on no occasion has one-third of the number ever voted upon the question. Is it thus, sir, that such a question is to be decided—a question involving the nature, the functions, and the station of a public body? My opponents will say, Why did they not attend? I answer, There are sufficient reasons why they did not; but it should not have been left to option. There should have been "a call of the House." The members should have been specially summoned, and, if necessary, under fines. If this had been

done there would have been a different result, or I am mistaken. Next, sir, for the communications from the country:—The committee, according to their own statement, received nearly one hundred communications from members and licentiates in the country: some adverse; the majority favourable. Now there are about thirty members, and upwards of two hundred licentiates, resident in the country, so that making due allowance for the word “nearly,” and for the adverse communications, the proportion of those favourable to the change, does not exceed a third of the entire. But the strongest proof of my objection is, that the licentiates of Dublin have actually protested against the measure. The public have been told nothing of this, but it is nevertheless true. They have been convened within the College walls. They are upwards of one hundred in number. They have conferred with the committee; and yet, while they complain of their condition, and desire a remedy, they have protested against the proposed measure of dispensing and selling medicine, as unjust to the apothecary, derogatory to the profession, and one which nothing but extreme necessity could induce them to adopt. What is to be thought, sir, of this proceeding? Less than a third of the body take upon themselves to drive on a measure subversive of the nature and functions of the institution, and this in despite of the protest of the very persons whose interests are most deeply concerned. Sir, I have many times known the consideration of a far less important question postponed until a sufficient number of the body should be present to give the decision sufficient sanction. And why was it not so in this case? Will not, must not, the public conclude that those who have acted so precipitately have some object in view which the body at large would not approve? Why has the protest of the licentiates been withheld even from the College? Why have the committee not even alluded to it in their report, though they boasted of the many letters of approbation which they had received? Let them answer. Let them answer also this question: What moral epithet does such a proceeding deserve? or of what kind must be the cause which requires it?

My fourth objection to the proposed laws is, that they are, for the most part,

either highly objectionable, or impracticable.

Fifthly—They are not necessary for the purpose which they profess to seek.

Sixthly—They appear to me to be only a mask for another, as yet not avowed, design. The three last objections will require the space of another letter, and therefore, for the present, I subscribe myself,

Your obedient servant,
A MEMBER OF THE COLLEGE
OF SURGEONS IN IRELAND.

MEDICAL GAZETTE.

Saturday, February 10, 1838.

“*Licet omnibus, licet etiam mihi, dignitatem Artis Medicæ tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso.*”
CICERO.

CONVICTION FOR MALAPRAXIS IN MIDWIFERY.

IN the whole range of forensic medicine no subject can be more interesting, or more difficult, than *malap Praxis*. To lay down the limits of blameable inertness on the one hand, and still more culpable rashness on the other, and to show how large is the intervening space where difference of opinion may have full play, would require no ordinary tact; and we are not surprised, therefore, that some writers have altogether omitted this subject. In the obstetrical branch of our art, at any rate, we should imagine that all prudent persons would join with Dr. Blundell in reprobating what he calls “a meddlesome midwifery”; and, consequently, that no practitioner should be reprov'd for inertness, in abstaining from manual or instrumental interference, unless the case clearly required it, beyond all reasonable doubt. It may not be without interest to our readers to learn how they manage such things abroad; we therefore subjoin rather a curious case in illustration, which we

have taken from one of the German journals*.

On the 2d of January, 183—, Mr. K., an accoucheur, was called between seven and eight in the evening, as he stated in his first examination, to Mrs. X. She was 32 years of age, had borne several children, and was now in the eighth month of pregnancy. For twelve or fourteen days she had been suffering from hæmorrhage, which did not occur every day, but when it did, sometimes amounted to two or three pints. He said that at this, his first visit, the woman was weak and exhausted, the pulse slow, but not too weak, and that there was no hæmorrhage, and no nervous symptoms. On examination, he found the os uteri high, and no part of the child presenting; there were neither labour pains nor pains in the back, but in the body only. The accoucheur was of opinion that the placenta had been partially separated, and was lying upon the os uteri, and that the patient was in danger, but he did not attempt anything, as she was weak, and in want of repose. He therefore contented himself with prescribing an emulsion containing extract of hyoscyamus and ether, and introduced a piece of sponge moistened with alum, strong acetic acid, oxymel, and water, into the vagina.

On the 3d of January, between five and six in the morning, the accoucheur, and Mrs. C. the midwife, were at the patient's bedside together. Fresh hæmorrhage had set in towards the morning, and nearly destroyed the patient; her lips were pale, her eyes dim; she had a weak pulse and cold sweat, but spoke connectedly, and complained of no pain. Mr. K. found that the vagina was puffed, and preternaturally soft, and that the os uteri was high and closed, but remarked no labour-pains. Under these circumstances, and particularly on account of the great weakness of the woman, he did not think it advisable to proceed to delivery, as he feared a renewal of the hæmorrhage and other bad consequences. About nine o'clock he left Mrs. X., and directed the midwife gradually to dilate the os uteri. An hour afterwards he found the patient breathing her last, and she died in his presence. He did not try to resuscitate her, as he thought it superfluous. The child he declared to be

dead, as the mother had not felt any movements for three days. He mentioned the extraction of the child from the mother to the husband, who did not consent to it; but the Cæsarean operation was not expressly named. Mr. K. was opposed to delivery by the natural passage, as he thought that, under the given circumstances, it was too dangerous for the child.

Such was the history of the case, founded on the accoucheur's deposition; but it was contradicted in many material points. The husband of the deceased affirmed that Mr. K. did not return until eleven o'clock, when his patient had already expired; that, after repeatedly touching the body, he declared that the child was dead; and that no mention was made of the Cæsarean operation, which he (the husband) would certainly have allowed.

Mrs. C., the midwife, confirmed this; but added, that the accoucheur, on going away, had said to her that he would return in the afternoon to perform the Cæsarean operation. She also deposed that, by frequent examinations, she had convinced herself of the position of the placenta, but that the accoucheur considered this only as a conjecture; which, however, he altogether denies. She also added, that when Mr. K. directed her to dilate the os uteri till his return, she replied, that the patient would then grow weaker and weaker, and she was of opinion that delivery was indicated. This, too, was denied by Mr. K.; but Mrs. C. persisted in it when confronted with him. Moreover, on the 3d of January she found the os uteri low down, and dilated to the size of a *groschen* (a silver penny), and told this to Mr. K., who, however, says he never found it so. She endeavoured to dilate the os uteri as she was directed, but the floodings followed one another too quickly to allow her to repeat her attempts. Mrs. X. died half an hour before Mr. K.'s return.

Mr. K.'s second examination con-

* Schmid's Jahrbücher. 1837. Band 16. No. X. Heft 1. from Siebold's Journal.

tained still more inconsistencies. He said that he did not attempt artificial delivery, because it was impossible to reach the os uteri; and this, although he had repeatedly declared that the os uteri was closed; and, on the other hand, had confessed, when the protocol of the proceedings was read, that it would easily have admitted a finger. He stated that at first he had suspected a presentation of the placenta, but afterwards thought that the hæmorrhage proceeded from the vessels of the vagina, for which reason he plugged it. Lastly, he declared that he clearly foresaw the death of the patient, and did not wish to torment her uselessly, nor to be exposed to censure in case of her dying while under his hands.

The Board declared, that from the deficiency of information, from the contradictions between the testimony of Mr. K. and Mrs. C., and from there having been no post-mortem examination, they were unable to decide whether the death of the patient was caused by hæmorrhage or apoplexy; and whether it was to be attributed to Mr. K.'s not having delivered the woman, for it might also have taken place during the operation. They declared, however, that the accoucheur's conduct was blameable for the following reasons:—

1st. He had neglected to use proper remedies. They blamed him for having added the strong acetic acid to the solution of alum, declaring its action, when externally applied, to be very different from that of vinegar, and rather tending to increase hæmorrhage.

2dly. He had used plugs for the vaginal hæmorrhage, although, according to his first view of the case, the symptoms required a different treatment.

3dly. Notwithstanding the reasons which discouraged him from attempting artificial delivery, he ought not to have quitted the patient, nor to have left the dilatation of the os uteri to the midwife;

but he ought to have used proper remedies, and to have waited for a favourable moment for delivery, so as to be ready to meet the danger of renewed hæmorrhage, and, if possible, save the life of the mother and the child.

4thly. That he had neglected to try to resuscitate the patient, and had held the child to be dead, without any proof.

5thly. That he had by no means properly recognized the presentation of the placenta.

In consequence, the Board adjudged—1st, that Mrs. X. would perhaps have died under any other method of treatment, and that it could not be shown that Mr. K.'s conduct had unconditionally caused her death; 2dly, that Mr. K. had been guilty of several manifest offences against the rules of medical treatment, so that he could no longer be permitted to practise midwifery, unless he should show, by successfully going through a strict examination, that he had been led astray in the case in question by an accidental confusion of thought.

To this judgment Mr. K. replied, that from the os uteri being high and closed, neither internal nor external examination could prove that the placenta presented, particularly as the insertion of the finger produced no hæmorrhage. That occasional hæmorrhage, indeed, had taken place for a week, but that it had been but slight, according to the account both of the patient and the midwife; and being accompanied by bearing down, and pain in the sacrum, seemed to show that premature delivery was at hand. He endeavoured to answer the several reasons for the judgment (with the exception of No. 2) in the following manner:—

That he had prescribed the best remedies; for the accusation as to the strong acetic acid was founded on an error of the pen (in one of the written pleadings, we suppose), and he referred

to his prescription of the 2d of January, to show that he had ordered vinegar. That on the following morning things had altered, and, after a violent flooding, he had convinced himself of the presentation of the placenta; but that the patient was very weak and near her end, and the dilatation of the os uteri would have required from an hour to an hour and a half, and would therefore have infallibly killed her.

That the duty of a physician, as defined in his oath, is to leave no patient without aid, but that he is not obliged by any law to remain with a dying person till the last sigh. That artificial delivery could not be practised in this case, and as midwives are not forbidden to dilate the os uteri, that it was just the same whether he or the midwife did it. Cordial medicines would have been hurtful by exciting hæmorrhage.

That *apparent* death is not to be presumed when a pregnant woman dies of hæmorrhage from separation of the placenta, but only when death has been suddenly produced by nervous attacks, spasms, apoplexy, or the like. Nor was it possible for the child to live with a separated placenta, and its death was confirmed by the symptoms of putrefaction. It was the fault of the police that he had not performed the Cæsarean operation after the death of the mother; for, when the husband hesitated, they would not afford him the necessary assistance.

That in a woman who has borne many children the os uteri is closed at the end of pregnancy, even although it were possible to press the finger in, which in this case could not be done, from the uterus lying so high. He added, that in a woman in the eighth month of pregnancy, in whom the os uteri was closed and lying high, and where hæmorrhage had come on in consequence of the separation of the placenta,

and the accoucheur was not called in till after the lapse of twelve hours, no practitioner could save the life of the patient. He concluded by attacking the midwife, who, he said, had caused the calamity, by not calling him in three days sooner, when he would have had time to dilate the os uteri.

On this, the board promulgated a second judgment to the following effect*: —Mr. K. is mistaken in saying that no better remedies could have been used to check the hæmorrhage, for experience has sanctioned the internal application of alum and the mineral acids. He was also to blame in refraining from artificial delivery on account of the fainting or weakness of the patient, as such a state is often relieved by the stimulus resulting from dilatation of the os uteri.

The practitioner should certainly never quit his patient when a remedy is to be used with the intention of warding off immediate dissolution. This is especially necessary in artificial delivery, as partial dilatations of the os uteri are injurious, and the patient is exposed to dangerous hæmorrhage, unless with due circumspection and presence of mind the delivery is concluded as quickly as possible after the artificial dilatation of the os uteri.

Mr. K. is quite opposed to experience when he asserts that under these circumstances death is never merely apparent, and that the children are always born dead. His ignorance, too, is shown in other inconsistencies; thus he rejected delivery by the natural passage, because it would have been too dangerous for the child, and yet he says it was dead, because it had not moved for three days.

Mr. K.'s want of knowledge is further shown by his theory as to the closure or openness of the os uteri at the conclusion of pregnancy; for he makes no distinction between the external and

* As Mr. K. had omitted No. 2 in his answer, the board omit it in their reply.

internal os uteri. No fault is to be alleged against the midwife. Following the instructions of her manual, she called in an accoucheur on the appearance of frequent hæmorrhage; and Mr. K. says himself, that he found the pulse slow, but not weak, and that neither hæmorrhage nor nervous attacks were present. Mr. K., however, showed the greatest want of knowledge in entrusting the midwife with the dilatation of the os uteri during his absence.

For these reasons, the board confirmed their former sentence.

This case hardly requires any comment. Mr. K. was certainly censurable for leaving his patient at the period when he did so, especially if we judge by the event; yet if this is thought sufficient to justify so severe a sentence, the accoucheurs of his district must have attained absolute perfection; if his fellow-practitioners occasionally err likewise, then is Mr. K. the most ill used of men. Surely a censure from a public board would have been punishment enough, without suspending him from the practice of midwifery.

COLLEGE OF PHYSICIANS.

THE evening meetings for the season commenced on Monday, on which occasion the rooms were crowded to excess. Among those present we observed his Royal Highness the Duke of Cambridge, the Archbishop of Canterbury, the Duke of Wellington, Sir Robert Peel, and many distinguished members of the church, the bar, and the senate.

The paper, which was listened to with great attention, and excited general interest, was—

On some of the Results of the successful Practice of Physic. By SIR HENRY HALFORD, Bart., President of the College of Physicians, &c. &c.

After acknowledging the favourable manner in which his former papers had been received, the learned President proceeded to enlarge upon the facility with which the physician obtained

a moral influence over his patient in the hour of suffering, and on the advantage which might be taken of this for the best and noblest purposes. The attachment of the sick, and the respect of their friends towards the physician, were strongly sketched by Sir Henry; who, however, did not dwell upon this part of the subject.—

I will rather, said he, turn to history for instances in verification of my position, yet I will not go back to antiquity; though I might find passages in Homer strongly expressive of the attachment and respect of the Grecian heroes to their medical attendants, and several instances in the life of Hippocrates demonstrative of the regard and admiration of his countrymen. Our own times furnish me with a striking example of the deference paid to a physician by the highest potentates. When Dr. Jenner first promulgated the protective influence of vaccination against the danger of small-pox, the king of Spain fitted out an expedition to carry the vaccine matter to every part of his transmarine dominions; and after having left the valuable material at the Canary Islands, the Caraccas, and all the provinces of South America, the director of the expedition resolved to carry the preservative to the remotest part of Asia; and having stopped at Acapulco and the Philippine Islands, he introduced vaccination into China. The Emperor of Russia, too, after he had established the practice of vaccination throughout all his European dominions, sent Dr. Bontaltz to traverse his Asiatic possessions for the same benevolent purpose. The mission, therefore, when it had reached the capital of Siberia, proceeded to Ocholtz; from whence he sent it to Kamschatska, and the islands situated between Asia and America. Fresh matter was transmitted also into China; so that the Spanish and Russian expeditions reached different points of the celestial empire nearly at the same time.

In the Island of Java certain portions of land have been set apart for the support of vaccination; and as a mark of respect and veneration for the author of this valuable discovery, these lands have been designated Jennerian lands.

This detail may be considered, perhaps, as affording evidence rather of the confidence of these sovereigns in the merit of the new invaluable resource, than of their personal respect and de-

ference to Dr. Jenner. But it is true that Bonaparte, in the plenitude of his power, accorded their freedom from bondage to no less than nine captives severally, at the request of Dr. Jenner—a homage to the benevolent author of so important a discovery; and that the Emperor of Austria and the King of Spain paid equal attention to Dr. Jenner's intercession in behalf of individuals who were detained in their dominions.

But the anecdote most flattering to the medical profession, which I would recal to your remembrance, is the occasion of the first establishment of the East India Company's power on the coast of Coromandel, which was procured by the favour of the Great Mogul to one of our profession (Gabriel Boughton, of the ship *Hopeful*), in gratitude for his efficient help in a case of great distress, to the monarch. It seems that in the year 1636 (a very early period of our direct intercourse with India, after the Portuguese had discovered the passage thither by the Cape of Good Hope) one of the princesses of the Great Mogul's family had been burnt dreadfully by accident, and that a messenger was sent to Surat, where foreign traders resorted, to desire the assistance of one of the English surgeons there, for they had acquired a great reputation amongst the natives for their skill in the cure of diseases. Gabriel Boughton proceeded forthwith to Delhi, and was successful in performing a cure, on which the Great Mogul's minister asked him what his master could do for him to manifest his gratitude for so important a service. Gabriel answered with a disinterestedness, a generosity, a patriotism beyond my praise, "Let my nation trade with yours." Be it so. A portion of the coast was marked out for the future resort of English ships, and all duties were compromised for a small sum of money. A better station, it is true, was selected at the mouth of the Hoogley River some twenty years afterwards, and Calcutta was built; but *here* was the first establishment of our power—here did the civilization of that vast continent begin. From hence the blessed light of the Gospel may have been first promulgated amongst 100 millions of native idolaters, since subjected to the control of British power, and made partakers of our enlightened comforts.

This happy result of the successful

interposition of one of our medical brethren suggests a question to my mind of the expediency of educating missionaries who are to be sent to the rude uninformed population of distant countries to propagate the Gospel, in the medical art, as the earliest object of their studies, in order that they may make themselves more acceptable than if they presented themselves professedly to teach a new religion. I propound this question with great diffidence, particularly in the presence of that part of my audience with whom it may rest to direct the preliminary education of this useful body of men; but I know that the candour of these venerable characters is equal to their high dignity, and that they will receive my suggestions in good part, and feel assured that I mean that these missionaries should carry the Gospel in their heads and in their hearts, and govern their conduct by its precepts; though I conceive it might further their purpose to withhold the sacred volume until after they had acquired the goodwill and confidence of their hearers, by the service they had done them in healing their sickness. We know what the Jesuits have accomplished in the pursuit of this concurrent object, wherever they have found admittance, and I am sanguine enough to believe that even that proud and exclusive people, the Chinese, would receive those who entered their country with these views without that suspicion and distrust which they never fail to manifest when they surmise that trade is the object of the stranger's visit, or some covert intention to interfere with their institutions; and that this might be made the occasion of giving the comfort of the Gospel to 300 millions more of the inhabitants of our globe, in process of time.

The Chinese received vaccination kindly; and when a small English tract, written expressly to recommend that process, had been translated into their language, though strenuously opposed to every innovation on all occasions, they not only submitted to receive this new practice, but actually raised a considerable subscription to assist in diffusing it throughout their vast empire; and since that time they have allowed missionaries from America to establish an ophthalmic institution, and also a general dispensary for the supply of European medicines and advice to their poor. I will read a part of a

letter from Canton, dated the 2d of April, 1837, from the Rev. Mr. Gutzlaff, a member of the American Medical Missionary Society there, in which he says, "I humbly hope that the Association will prove a great blessing to this country. Dr. Parker continues to operate most astonishing cures, and other physicians have already arrived at Singapore. By endeavouring to benefit both the body and the soul, some favourable impression, it is to be expected, will be made on the minds of this people."

Undoubtedly the Chinese practice of physic is so meagre, and inadequate to meet and control the ills which flesh is heir to, as to give an easy superiority to the commonest pretensions of European knowledge. You will agree with me in this opinion when I tell you what their physiology and pathology is, and what are their resources under disease, which I am enabled to do by the kindness of the Right Hon. Sir Alexander Johnstone, who put into my hands a MS., since printed in the *Asiatic Journal*, giving, from the best authority, a full account of the practice of physic of China.

It seems that they know nothing of the doctrine of the circulation of the blood. They believe that the human body is composed of five elements—water, fire, wood, metal, and earth; that as long as the equilibrium between these elements is maintained, people enjoy health; as soon as one predominates, sickness ensues; that all diseases arise from disturbing the equilibrium of these parts, and that the art of healing consists in restoring their mutual relation. They know nothing whatever of chemistry. There is not even a name for it in their language. Their medicines are almost all vegetable, and the Ginseng root their panacea. Detesting the sight of blood, the abstraction of it, by whatever means, is almost or altogether unknown amongst them; and their utter aversion to any surgical operation reduces them to the necessity of depending upon the inefficiency of internal remedies in surgical cases. The employment of the moxa and acupuncture can hardly be considered as exceptions to this general interdict.

So long as the erroneous opinion prevails with them that every thing ancient is excellent, and superior to recent in-

ventions, they must necessarily remain in their present state of ignorance, and the most useful sciences will be rejected. Not that we can expect the Chinese to grasp with eagerness at our improvements, yet the cure of diseases set down at once as fatal in their experience, must be likely to facilitate the introduction of our knowledge, and add most humanely to their comfort and civilization; and not to their temporal happiness only, but to their future felicity, by the introduction of the Holy Scriptures amongst them by this avenue.

With those who practise upon such a system of physic as I have detailed, if system it can be called, is it possible that the acquired knowledge of his profession, in an English medical practitioner, should come into competition without the greatest advantage in his favour? Is it possible that his knowledge of anatomy, by which he is enabled to detect the seat of disease; his acquaintance with chemistry, and all other resources of his art, by which he administers effectually to maladies within the reach of human skill, should not give his patients, and those who surround them, a greater confidence in his judgment than in that of a feeble native practitioner? Be it understood, however, that I do not claim your acknowledgment of his superiority for an Englishman of superficial knowledge only. In our profession a little knowledge may be a dangerous possession; and it is on this conviction that I humbly propose that those who are to be educated to become missionaries, after having had their minds thoroughly imbued with moral and religious principles in their first scholastic discipline, shall then attend to anatomy and chemistry, and other courses of medical lectures, and for a certain time frequent some one of the great hospitals, so as to qualify themselves to practise physic and surgery, as if they were to prosecute our profession as their means of living.

With minds so exercised, men are surely not ill prepared to receive, and weigh well, and deeply study, the sacred truths; and having the sanction of holy ordination, to go forth amongst the heathen to expound them. The Americans seem disposed, according to the accounts from Canton, to keep separate the civil and the clerical characters, and have no other missionary there

professedly educated for the practice of physic and surgery but Dr. Parker; yet it is acknowledged that he is most in credit for the good which he does. Now confidence is not transferable, and it does not follow that the impression of gratitude and attachment which the physician shall have made by his successful ministration to disease, will be given necessarily and of course to a stranger introduced to explain what is required for the salvation of the soul. I hold it, therefore, far preferable that the two characters should be united in those whose zeal for the benefit of mankind may carry them to remote parts of the world. Of this union we all know numerous and respectable instances at home; and it cannot fail to be remembered within these walls, that our founder, Linaere, the first President of the College, took orders in the latter period of his life, and died in the exercise of those sacred functions.

But (said Sir Henry, in conclusion) I am afraid I become tedious. Let me make haste, then, to thank you for your patient attention, and to add, in evidence of the sincerity with which I have represented the frequent occasions of the purest mental gratification which the physician finds in the exercise of his calling, independently of any other consideration, and contrary to the maxim in Horace—

“Nemo, quam sibi sortem
Sui ratio dederit, seu fors objecerit, illi
Contentus vivat, laudet diversa sequentes;”

if I were to begin my life again, I would adopt the profession of physic.

Amongst these gratifications, I should be ungrateful to you if I did not enumerate the repeated pleasure of meeting and co-operating with you for the improvement of medicine, and for the promotion of the respectability of our profession, in these evening assemblies at the College; and when it shall please the Great Creator to remove me to another world, which must necessarily be at no very distant day, some of you who now listen to me may bear in mind these my unfeigned sentiments and attachment to our common profession, and not be surprised if they should bear that—

“Occidit et dulces moriens reminiscitur Argos.”

LATE IMPROVEMENTS IN MEDICINE AND SURGERY.

THE diseases of the bones have, of late years, engaged the earnest attention of professional men; and a *concours*, proposed by the Academy of Sciences, on the subject of spinal deformities, could not fail to be an inducement to additional study. After waiting five years, the great prize was carried off by a brother journalist, M. J. Guérin, and the second one by M. Bouvier.

Orthopedy seems to have arrived at a point where it is no longer a mechanical and illu-vory art, but the whole of medicine applied to a branch which may be considered as new, since our predecessors knew nothing but its name.

Medical gymnastics, directed according to the data furnished by the last researches into pathological anatomy, now form one of the most essential parts of the treatment of deformities. Gymnastics, when employed as they are at the present day, constitute an important branch of therapeutics, and are really a science practically applied, but one with which the majority of practitioners have but a very slight acquaintance.

An equally brilliant improvement, which is exclusively of modern date, has been made in the treatment of club foot, and angular ankylosis of the knee. Some years ago, it would hardly have been thought possible that a few weeks should suffice to cure completely, and almost without pain, deformities which were recently thought by great masters of our art to be incurable, particularly in adults; but facts are stubborn things, and we must submit to evidence. Several dozens of children operated upon by M. Duval, in our presence, were perfectly cured of their club-feet in a fortnight or twenty days; and many other patients, aged from twenty to fifty, received the same benefit under the hands of the same practitioner, in a space varying from one to two months after the tendo Achillis had been cut. Our readers are already acquainted with the first results obtained by the operation of cutting the tendons of the popliteal region for the cure of angular ankylosis of the knee; and we shall soon be able to publish several other cases of the same kind, which have been operated on in the institution for club feet, at Paris. An important circumstance to be noted in all these operations is, that if the tendon is not completely divided, they fail entirely in their object. In fact, if a very small strip of tendon has escaped division, the limb cannot be made perfectly straight. This has occurred at the Hôtel Dieu, and

even in private practice, in the case of one of the principal editors of a great political journal, and the deformity returned from this circumstance alone. This want of success proceeds merely from the method of operating employed; for the operation never fails when the tendon is cut according to Stromeyer's method, as improved by M. Duval. It is obvious, indeed, that by cutting in the contrary direction to the one which he follows, that is, if we cut from the skin inwards, the tendon is pushed back, and we are liable to leave some of the tendinous fibres untouched, or else to injure parts which ought not to be meddled with*.

The treatment of fractures of the limbs has undergone important modifications. M. Seutin, chief surgeon to the Belgian army, has successfully altered and generalised the fixed apparatus; he employs a solution of starch as a plastic material for the straps, and uses pasteboard splints instead of wooden ones; at the same time he has adopted the principle of making the patients walk as soon as the bandage is dry.

The results obtained by M. Seutin with the starch apparatus, have received the approbation of the Society of Medicine, and the Academy of Sciences, at Paris, particularly in fractures of the leg. We shall return more in detail to this method of treatment when we give an account of the work which M. Seutin has just published. Let us mention, meanwhile, that several patients with fractures of the leg, who are now in the Charité, and are being treated in this manner, are suffering from considerable eschars.

The pathology of the urinary passages is understood better and better. Strictures of the urethra are no longer treated by any dilatation, except that which is merely momentary; for experience having shown that permanent bougies irritate the canal, delay the effects of dilatation, and predispose the patient to relapses, the practice has been adopted of dilating the urethra for a short time only every day. Bougies are employed of a gradually increasing size, and they are left in the urethra for a few minutes, or at most half an hour, each time. Patients treated in this way experience no bad symptoms, can attend to business, and get well more quickly and certainly than by the old method. The bees'-wax bougies found in

our shops are now generally preferred to those of elastic gum. This method, though sanctioned by the experience obtained in private practice, is not yet employed in our public hospitals.

But whenever a stricture of the urethra seems insurmountable by ordinary means, practitioners do not now hesitate to make use of the bistoury. Experience seems to have shown that in such cases the small incision made at the very point where the stricture is, dissolves the diseased tissues, or rather discharges them by the suppuration which follows, thus ultimately allowing the dilatation of the canal of the urethra.

Lithotripsy continues to extend its domain, and lithotomy has really become an exception. The *percuteur à marteau* gives pitiless raps both to the *pince à trois branches* and to the adversaries of this beautiful operation. Meanwhile, M. Souberbielle is still considered the most successful of lithotomists.

The radical cure of hernia is actively pursued by several professional men. We have seen patients who had been cured by M. Lafond; and M. Bonnet, of Lyons, has made other cases known. Much remains to be done in this important department of surgery.

Hydrocele has of late been the subject of therapeutic research, particularly in England. Mr. Lewis, of London, cures this disease in a very simple manner. He inserts several acupuncture needles, or common sewing ones, into the tumor, and leaves them there a few moments, twisting them between his fingers, in order to make the punctures round; the water becomes extravasated, drop by drop, into the cellular tissue of the scrotum, and is afterwards re-absorbed; the sac gradually returns to the testis, and the cure takes place without adhesive inflammation. The cure is radical, and is effected in a few days, without pain, and without confining the patient to his bed, or to his room. Nevertheless, relapses have sometimes occurred, in which case the operation has been performed again*.

The same treatment has lately been applied with success to idiopathic dropsy of the abdominal cavity.

Microscopic pathology seems of late to have got into favour with experimental physicians in different countries. M. Delle Chiaje, of Naples, has discovered the *polytoma* in the blood of man: it is a small worm, shaped like a leech, which is developed in the circulating fluid of some pa-

* The Gazette des Hôpitaux, of January 4th, contains a letter from Dr. Bonvier, in which he says that he prefers the method of operating which is censured in the text—namely, that of dividing the tendon from the skin inwards; and he asserts that he has never failed, either in the Hôtel-Dieu or in his own institution, or in private practice.—Translator.

* Mr. Lewis's claim to the invention of this method seems doubtful: our readers will find several controversial letters on the subject in the 19th vol. of the MED. GAZ.—Translator.

tients. M. Delle Chiaje has observed a great number in the blood which various phthisical patients have vomited in his presence, as well as in the blood which he has drawn from the veins of persons labouring under other diseases. The *poly-stoma* is found also, without the existence of disease, in the blood of several non-ver-tebrated animals. M. Delle Chiaje makes use of his discovery to explain the develop-ment of the disease called *helminthiasis*, or verminous affection, and the formation of hydatids in general; as, according to him, the blood secretes the germs of these animals. Another equally curious fact has been brought to light by microscopic ob-servation—we mean the animalization of the globules of milk, which M. Turpin seems to have confirmed.

Thus we see that the year which has passed away is more remarkable for im-provements than for great discoveries. At its beginning, certain persons proclaimed their appearance upon the medical hori-zon with a sort of *fracas*; they called them-selves reformers of the whole science, and loudly announced the fact in their jour-nals. Others, perhaps, unterrified by their fall, will succeed them, ready to wander in the same path, while they forget the first part of the saying of Archimedes—*give me a spot on which to stand!* Yet after, as before them, science will continue its habitual progress, slow, but still advanc-ing.—*From the Gazette des Hôpitaux of Jan. 2, 1838.*

EXPERIMENTS ON ANIMALS WITH THE BLOOD OF CHOLERA PATIENTS.

By DR. NAMIAS, OF VENICE.

UPON opening the body of a man, aged fifty, who had died in the cold stage of cholera after twenty-four hours' illness, at the hospital of St. Daniel, the blood in the cavities of the heart was found to be black and congealed together, with one or two polypous concretions. A portion of the congealed blood, of the size of a straw-berly, was inserted, without causing much suffering, under the skin of the thigh of an old and fat rabbit. The fur was shaved off, and an incision having been made through the skin, it was separated by the handle of the scalpel from the cellular tissue beneath; and into the cavity thus formed the blood was introduced, and the wound was then carefully closed with sutures. This mode of inoculation was followed in all the experiments.

Exp. 1.—Five days after the operation the rabbit appeared ill, its evacuations were

less solid than usual, and a whitish glu-tinous matter was observed upon the ground. The animal was found dead on the eighth day. The blood of the heart was black and grumous, with some fibrinous concretions; the bladder was full of urine; the injected blood had pervaded the surrounding tissues; the lips of the wound had their normal consistence; the internal surface of the whole of the skin was covered with blue spots of ecchymo-sis; but the rest of the organs exhibited no alteration.

Exp. 2.—An equal quantity of the blood of this rabbit was injected in a similar manner into the thigh of a grey female rabbit. It was found dead in twenty-four hours afterwards, with the same appear-ance of the whitish matter upon the ground. The body, when examined, ex-hibited similar results as in the former experiment. The author here judiciously remarks, that the rapid death of the second rabbit arose not only from its weaker age and sex, but also from the blood of an animal of the same species being more readily absorbed than human blood.

Exp. 3 and 4.—The blood of the last rabbit was inserted into two others that died in six days with the same marks of disease. These rabbits were much larger than the former.

The next experiments were performed with the blood of a man who died in the cold stage after twelve hours' illness. The blood was black and grumous, but there were no concretions.

Exp. 5.—This was a fat old grey rabbit, of the male sex. It died five days after the operation. There was no whitish matter on the floor; the bladder was empty; there were many brown spots on the inner surface of the skin. The state of the blood and of the wound, that was not of a kind to affect the health of the animal, was the same as in the other cases.

Exp. 6.—Performed with the blood of the last on a male rabbit, two pounds and a quarter in weight. It died in forty hours. Bladder full; blood black and fluid; the usual spots on the skin. In this instance also, the poison transmitted through a similar animal acted more rap-ly than when taken directly from man.

Exp. 7.—Another rabbit, inoculated with the blood of the last, was found dead in six days.

Exp. 8 and 9.—In these two experiments, for the sake of greater accuracy, the Doc-tor weighed both the rabbits and the blood. Into one, weighing three pounds and a half, he injected ten grains of blood from the last experiment; and into the other, weighing three pounds, he inserted

eight grains of the same blood. Both died in six days.

The next experiments were undertaken with the view of ascertaining whether human blood, taken from persons not dead of cholera, would produce the like results. Three rabbits were inoculated: one, of two pounds and a quarter, with seven grains of black fluid stinking blood, from the heart of a patient dead of gangrene of the bowels; a month afterwards the animal was well and vigorous. Another, of three pounds and a quarter, with the blood of an aneurismatic patient, of which twelve grains were used; and the third, of two pounds two ounces, with fourteen grains of the same blood, which by its quantity only would probably be fatal: on the twenty-sixth day afterwards the larger rabbit was sprightly and vigorous, the smaller dull and out of condition, with a large wound on the seat of the inoculation.

Dr. Namias considering the re-action of cholera as a beneficial effort of the vital powers to eliminate from the body matters that are incongruous with its tissues, to restore the deranged state of the sanguiferous system, and to renew the exhausted nervous energy, was inclined to believe that the blood of persons dying in this stage would be less injurious than that from persons in the pulseless state; and some experiments that he undertook with Professor Rima seemed to confirm this notion. Twelve grains of the blood of a female, dead after three days' illness with imperfect re-action, were inserted in the usual way into two rabbits, and a third was made to swallow a scruple of the same blood. All three survived, but a great number of spots, produced by the drying of a white glutinous matter, were found in the hutch of the two former. The other appeared to vomit the blood it had swallowed, and for some days after it vomited a similar white substance. Four scruples were injected under the skin at the back of the neck in a little dog. This blood had been taken from a woman dead of cholera in seventy-two hours after incomplete re-action. For the first two days the dog passed, along with natural faeces, many small white worms; on the third day he vomited milk he had lapped; and he died early on the fourth day. Incipient suppuration was observed under the separated skin; the bladder was quite empty; the heart contained very fluid blood in small quantity: and there was no evident cause of death. — *Bullettino delle Scienze Mediche*; and *British and Foreign Medical Review*.

ON THE

FIBROUS MEMBRANE BENEATH THE PLEURA PULMONALIS.

BY M. BAZIN.

AFTER quoting the opinion of Colombo, the pupil and successor of Vesalius, that the pleura is composed of two laminae, between which the blood-vessels and nerves are distributed, the author of this memoir states that the researches of succeeding anatomists have led to a different conclusion, in consequence of their having been confined to the pleura costalis. His own extended investigations into the structure of the respiratory organs in the series of vertebrated animals, have led him to the conclusion that the lungs, like other organs, possess a proper capsule. He remarks, that capsular envelopes are found to exist in three states: 1, a fibrous network with large interstices, frequently taken for cellular tissue; 2, a complete fibrous or sclerous membrane, whose density and thickness may be variable; 3, an osseous plate. The capsule of the lungs in most animals presents the first condition, that which exists in man; but in the elephant a distinct fibrous membrane is met with, consisting of bundles of parallel fibres interlacing at certain points with others, like the muscular coat of the bladder. The lung of a panther, which died of phthisis, presented M. Bazin with a hypertrophied condition of this membrane, which in the healthy state of this animal is not thicker than in man. — *Annales d'Anatomie et de Physiologie*; and *Brit. and For. Med. Rev.*

ROYAL INSTITUTION.

Friday, Feb. 2, 1838.

THE subject of this evening's lecture was—

The Skeleton of Insects displayed through a Gas Microscope. By Mr. GOADBY.

The first skeleton brought into view was very distinct, in which the lecturer pointed out the three great divisions, consisting of—1st, The head, which has attached to it the mouth, the antennae, and the eyes. The mouth consists of an upper and lower lip, a pair of upper and a pair of lower jaws. The antennae are two articulated bodies, one on each side of the head. The eyes are of two kinds, compound and simple. 2d, The thorax, consisting of the prothorax, mesothorax, and metathorax. 3d, The abdomen is made up of several

segments, the number varying in different animals. The insect displayed was the *Blatta Americana*, which has three simple, besides two compound eyes. The antennæ, or feelers, whether they are possessed of touch or not—a point which has given rise to much discussion—yield at least great assistance in holding the food. The wings of this insect are of a very beautiful texture; and the bones in them, which are mere hollow tubes, and serve the purpose of conveying nourishment, as well as the vessels by which blood is carried to and from them, were very distinctly seen.

The lecturer then proceeded to make some remarks on aquatic insects, and some peculiarities in them suited to the element in which they live. Among these he noticed the immense size of the coxæ, or hips, in which the muscles are fixed, and which adds considerably to their strength. He also alluded to the row of small apertures round the outside of the back of the skeleton, by means of which they inhale air when they come to the surface of the water. Thence he passed to the *Mantis Religiosa*, so called from its being worshipped by the Brahmins of India. The Chinese keep them, and make them fight like game-cocks, for which they are well adapted, being very cruel and carnivorous insects. Their wing-cases are articulated with particular beauty, and still more so the wings themselves. Mr. Goadby was then going on with some remarks on the dragon-fly; but the gas, which had not burnt well during the latter part of the lecture, now went out; so that, after apologizing for the insufficient supply of gas which he had prepared, and for the imperfection of the microscope, which he stated that he was in the course of improving, he was obliged to bring his lecture to rather a hasty conclusion.

PHYSICAL SOCIETY, GUY'S HOSPITAL,

January 27, 1838.

DR. ADDISON IN THE CHAIR.

MR. ASPLAND read part of a thesis on the physiological and pathological relations of the heart.

The chief points upon which the author dwelt, were—

1st. Physical evidence of hypertrophy. He doubted the existence of a bellows sound in this lesion, when unconnected with induration or contraction of an orifice. 2. Disease of valves. He had not unfrequently had opportunities of

examining *pure* cases, in conjunction with renal disease, which were afterwards demonstrated by post-mortem examination, but there had never been in these instances a bellows sound.

Amongst the causes of pericarditis, measles was adduced, and maintained by Mr. Aspland to obtain more frequently in very early life than any other.

The physical signs of morbid states of the pericardium were then entered upon, and cases illustrating partial "frottement" read.

After enumerating the symptoms referred by authors to adherent pericardium, two were selected, as most frequently present—epigastric undulation, and increased impulse towards the base of the heart. To these the author added contraction* of the intercostal spaces, synchronous with that of the epigastrium, that part of the chest opposed to the apex of the heart being at the same time impelled outwards more powerfully, and a greater fixedness of the point of impulsion of the apex, than during health; the difference being very marked on changing posture.

He then read four cases of adherent pericardium, where a distinct rubbing sound was audible: he had met with it in similar cases, believed it not uncommon when hypertrophy also existed, and was inclined to localise it in the pleura, as immediately caused by friction between the pericardial and pulmonary *pleura*. The three first cases were verified by post-mortem inspection; in the last the patient was convalescent. He was seized whilst in the hospital with acute pericarditis, complicated with pleuritis. A loud superficial to-and-fro sound accompanied the systole and diastole of the ventricles; this suddenly ceased, and was immediately succeeded by the phenomena mentioned above as indicative of the pericardial adhesions. During the course of convalescence a pleuritic rubbing sound, synchronous with inspiration and expiration, became developed; at the same time a similar sound became audible to the left of the præcordia, but synchronous with the heart's contractions. This he considered without doubt as seated in the pleura.

Amongst the complications of adherent pericardium, the author stated disease of the mitral valve to be very frequent: where adherence of the pericardium had been present for any length of time, and where the circulation had been active, he confidently anticipated mitral valve disease. He suggested that owing to the adhesion, the axis on which the blood was propelled from the ventricle might be altered, and

* Only perceptible where hypertrophy existed.

instead of finding directly the aortic orifice, impinged against the mitral curtains, and thus produced the effect mechanically.

Diseases of the mitral valve occupied a short time. The author thought he was not refining too much in classifying them into three, which allowed of ten of diagnosis.

1. Simple thickening of the curtains, which he had diagnosed in some cases by the existence of a click immediately after the first sound.

2. Excessive contraction of the orifice, not allowing of regurgitation. Amongst other peculiarities this condition was rarely attended with bruit, often not complicated with hypertrophy*; pulse more inclined to vary in power than frequency.

3. Contracted valve, but allowing of regurgitation, attended with bruit, and excessive irregularity of pulse.

Pure mitral valve disease he considered as not often attended with dropsy, or diseased liver; the lungs, whatever be the state of the tricuspid, bearing the brunt of the disease.

The discussion that arose upon this paper was long and animated, and bore chiefly upon two of the topics introduced by the author. The one was a question as to the value of "frottement" as a pathognomic sign of pericarditis; it being contended by several speakers that this sound existed occasionally without pericarditis, and was sometimes absent although pericarditis was present. It was further contended that no friction could occur when the pericardium was closely adherent, and that friction outside could not indicate inflammation inside the membrane.

Mr. THURNAM suggested that authors, such as Watson, Stokes, &c. had said that frottement only prevailed during that early stage of pericarditis when the effused lymph was first poured forth, and disappeared after adhesion.

The other question was in relation to mitral valve disease; some maintaining the occasional existence of bellows-murmur from regurgitation in this diseased condition, others denying it altogether.

Dr. ADDISON took occasion to state the following to be a general rule, which the result of a long experience upon the subject had led him to adopt. He had examined hearts of all sizes, affected with diseased mitral valves, and had never known bellows sound to exist during life, where he did not find some other lesion after death, besides that of the valve, competent to produce it; and on the other hand, where no lesion but that of the valve appeared after death, he had never heard bellows-murmur during life.

* A fact noticed by Dr. Addison, and also Dr. Latham, in his Croonian Lectures.

In the course of the evening, Mr. BRERETON took the opportunity of referring to some observations of Dr. Macleod's, which had been published lately in the GAZETTE, to the effect that pericarditis was connected with rheumatism of fasciæ, and tendinous structures, and not with synovial rheumatism. Mr. Brereton's attention had been attracted to the subject by the MEDICAL GAZETTE, and he had since seen several cases of pericarditis connected with rheumatism of joints alone, which were altogether in contradiction to Dr. Macleod's experience. Mr. B. related one of these cases, in confirmation of his statement.

The thanks of the Society having been presented to Mr. Aspland, for his Thesis, the meeting adjourned to Saturday, Feb. 10, at 8 P.M.; when Mr. Dendy will read some observations on the effusions that occur in the membranes of the brain and spinal marrow; Mr. Edward Cock in the chair.

ACCIDENTS ADMITTED AT THE LONDON HOSPITAL.

Surgeon for the Week, MR. LUKE.
Assistant-Surgeon, MR. CURLING.

Jan.	Sex.	Age.	Case.
16.	M.	29	Contused knee.
	M.	37	Contused back.
	M.	28	Injured side.
	F.	6	Severe burn (since dead).
	F.	43	Contused knee.
17.	F.	6	The toes crushed (a).
	M.	2	Burn.
	M.	53	Fraactured ribs.
18.	M.	16	Compound fracture of the tibia.
	M.	56	Fraactured patella (transverse).
	M.	25	Severe contusion of the arm.
19.	M.	44	Fraactured ribs.
	M.	20	Contused leg.
	F.	2	Severe burn (since dead).
	F.	14	Contused side.
	F.	29	Sprained ankle.
20.	F.	17	Contused hip.
	F.	23	Concussion.
	F.	57	Contusion in the head, and concussion.
	M.	46	Contused ankle.
	M.	37	Contusions on head and shoulder.
	M.	45	Severe cut over the nose.
21.	M.	53	Concussion.
	M.	65	Fraactured ribs.
	M.	31	Incised wound on the forehead.
	M.	16	Contused hip.

F. 7	Severe burn (since dead).
22. M. 47	Fracture of both bones of the forearm, and contused knee.
M. 23	Wound of the ulnar artery.
In-patients.....	29
Out-patients	34
Total	63

(a.) The injury to the toes in this case was so severe that they have all mortified, and separation has taken place at the first articulation.

INQUEST ON MARY STONARD.

To the Editor of the Medical Gazette.

SIR,

I PERCEIVE from the *Times* of this morning that your last number makes the case of Mary Stonard, who died suddenly at Clapham, on Thursday the 11th instant, the subject of severe remark, and that it is designated as "another death from starvation."

I take the liberty of informing you that a more groundless misrepresentation never was palmed upon the public, and that every material circumstance of the original report has received the clearest and most decisive contradiction, upon the testimony of the husband, the daughter, and the regular medical attendant of the deceased, supported by a mass of corroborative evidence.

To go into detail would occupy too much of your space. I will therefore only mention, that the woman died of a violent fit of spasmodic asthma, to which she had long been subject, and that she and her family were in circumstances of comparative comfort, their earnings being nearly 16s. a week, and their cottage and large garden held of the parish rent free. They never were without food or firing; they had meat, by their own shewing, nearly, if not quite, every day; and on the day before her death the poor woman had fried bacon, potatoes, and greens, for dinner; tea, sugar, and dripping toast in the afternoon; and boiled bacon and parsnips again at supper. Before she left home, on the morning of her death, she had sugared tea and dripping toast for breakfast, and, except her asthma, looked as well as at any time this ten years.

The accessory circumstances of "potatoe parings," and the attendant said to have accompanied her to Clapham, are utterly false. Much of the present statement came out upon the inquest (from which, by the

by, the most important medical witnesses were excluded,) but was suppressed by the reporter or his superiors.

The present is not the first occasion on which, to my knowledge, the inventions or misrepresentations of the newspaper press have betrayed you into bitter but undeserved reflections upon respectable individuals implicated in occurrences connected, or supposed to be connected, with the New Poor Law.

I am, sir,
Your obedient servant,
BENJAMIN FIELD.

Clapham Common,
31st January, 1855.

[The part omitted contains a strong censure of certain newspapers, which we do not feel called upon to insert. In respect to what refers to ourselves, we can assure Mr. Field that we have been influenced merely by feelings of humanity. We have no other sources of information on the subject alluded to except the newspapers, and when we have been led into error, as appears to have happened in the present case, we shall always be glad to be set right.—ED. GAZ.]

PARACENTESIS THORACIS.

To the Editor of the Medical Gazette.

SIR,

WHEN I delivered the lecture reported in the last number but one of your journal, I forgot to include, among the cases of Paracentesis Thoracis which I had myself witnessed, one in which that operation was performed by my colleague, Mr. Mayo, upon a boy, in whom empyema had ensued after fracture of the ribs. Complete recovery followed. The case is alluded to in Mr. Mayo's *Outlines of Pathology*.

The addition of this example renders my own list still more encouraging, in respect to the success of well-timed operations. In three out of six instances perfect recovery took place. In a fourth, the patient's existence was apparently much prolonged by the tapping; and I do not know that, in either of the two remaining cases, the fatal termination was in any degree accelerated by it.

I have the honour to remain, sir,
Your obedient servant,
THOS. WATSON.

Henrietta-street, Cavendish Square,
February 7, 1855.

January, 1838.

UNIVERSITY OF LONDON.

A MINISTERIAL print informs us, that on Wednesday night the following resolution was adopted almost unanimously by a committee of the whole senate of the University of London, at which 25 members were present:—"That examination in the Hebrew text of the Old Testament, and in the Greek text of the New, and in Scripture history, shall be instituted by this University, to be followed by certificates of proficiency, and that all candidates for degrees in arts may, if they think proper, undergo such examination."—*Times*.

MEDICAL PROMOTIONS IN THE NAVY.

JOHN RICHARDSON, M.D., F.R.S., &c., the celebrated Arctic traveller and distinguished naturalist, has been appointed Physician to Haslar Hospital, from the Surgeoncy of the Chatham division of Royal Marines; Mr. Rae, the junior Surgeon of Plymouth Hospital, succeeds Dr. Richardson at Chatham; Mr. Johnson, of H. M. S. Britannia, to Plymouth Hospital; and Mr. Martin, late of H. M. S. Hastings, to the Britannia.

BOOKS FOR REVIEW.

The Stomach in its Morbid States. By Langston Parker, Esq.

A Short Treatise on the External Characters, Nature, and Treatment of the different forms of Porrigo, or Scalled Head and Ringworm. By Walter Dick, M.D.

Prize Thesis.—Observations on the Influence of Climate on Health and Mortality. By Arthur Saunders Thomson, M.D.

The Cyclopædia of Anatomy and Physiology. Edited by Robert B. Todd, M.D. &c. Part XIII.

COLLEGE OF SURGEONS.

LIST OF GENTLEMEN WHO HAVE RECEIVED DIPLOMAS.

December, 1837.

Charles Thomson, Exeter.—T. Evans, Little Hampton.—G. Douglas.—J. O'Donnell, Kelbush, Co. Clare.—H. O'Hagan, Dundalk.—J. Borrer, Brighton.—J. Popham, Cork.—W. S. Clark, York.—A. Hobson, Tabberpatuck, Wicklow.—H. Bloxam, Newport, Isle of Wight.—G. Willson, London.—J. Ward, Kil-intyke, York-shire.—T. A. Wethered, Antigua.—W. C. Beeston, London.—J. Havers, Thelton, Norfolk.—R. H. Anwyll, Prince Edward's Island.—J. A. Partridge, Colchester.—C. P. Mann, Colchester.—C. F. Hodson, Islington.—T. R. Cotterell, Campden.—N. B. Turner, Keyme, Sussex.—W. Garden Sangster, Banff.—C. M. Miller, London.—W. Story, Mile End Road.—A. Wright, Birmingham.—H. Keyworth, London.—B. Clarke, London.

W. Weston, Siddlescombe, Sussex.—G. H. Bett.—W. Seagrave, New Kent-road.—H. Bickersteth, Cape of Good Hope.—E. Chinery, Melford, Suffolk.—W. D. Husband, York.—J. R. Lynch, King-street, Smithfield.—J. W. Holm.—Richd. Bryan, Stowmarket.—C. Collins, Maryport, Cheshire.—L. Holden, London.—W. Haynes, Quennington, Gloucester.—D. H. McNicoll, Liverpool.—H. H. Bowling, Hammersmith.—D. J. Thomas, Jamaica.—R. Johnson, Enneskillen.—T. Harper, Plymouth.—W. F. Barlow, Writtle, Essex.—G. R. Gilbert, London.—J. Robinson, Upper Eaton-street.—W. Veal, Abingdon-street.—J. H. Bell, Bristol.—H. Packard, Middlesex, Suffolk.—Jos. Toynbee, London.—Richard L. Jenkins, Newport, Monmouthshire.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, Feb. 6, 1838.

Abcess	2	Inflammation	22
Age and Debility	62	Bowels & Stomach	6
Apoplexy	1	Brain	2
Asthma	42	Lungs and Pleura	8
Childbirth	4	Insanity	2
Consumption	47	Liver, diseased	3
Convulsions	31	Measles	14
Dentition or Teething	4	Paralysis	7
Dropsy	17	Rheumatism	1
Drops; in the Brain	5	Small-pox	11
Erysipelas	2	Sore Throat and	
Fever	20	Quinsey	2
Fever, Scarlet	8	Thrush	1
Fever, Typhus	2	Tumor	1
Hæmorrhage	1	Unknown Causes	89
Heart, diseased	5		
Hooeping Cough	10	Casualties	6

Increase of Burials, as compared with } 164
the preceding week }

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.

Feb.	THERMOMETER.		BAROMETER.	
Thursday	from 23	to 33	29.96	30.07
Friday	26.5	35	30.19	30.25
Saturday	20	35	29.27	Stat.
Sunday	19	31	30.26	30.23
Monday	19	33	30.19	30.12
Tuesday	21	35	30.01	29.75
Wednesday 7	27	42	29.53	29.25

Winds N.E. and S.E.

Except the 2d and 4th, and morning of the 5th and following day, generally cloudy Rain on the 1st, a little snow on the 3d and 5th, a very heavy fall of hail on the evening of the 6th, and rain on the 7th.

Rain fallen, .275 of an inch.

CHARLES HENRY ADAMS.

NOTICE.

J. S. The paper has not reached us; we shall be happy to receive it.

WILSON & SON, Printers, 57, Skinner-st., London.

THE LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, FEBRUARY 17, 1838.

LECTURES

ON

FORENSIC MEDICINE;

Delivered at the Aldersgate School of Medicine,

By SOUTHWOOD SMITH, M.D.

Physician to the London Fever Hospital.

LECTURE VIII.

Distension and Corrugation of the Dura Mater

—Inner surface spotted with clusters of Red Points—Deposition of Matter between its Laminae—Deposition of Bone—Tumors—Diseases of the Sinuses—Diseases of the Glandulae Pacchioni—Adhesions between the Dura Mater and the Arachnoid—Effusion of Serum between the Dura Mater and Arachnoid—Effusion of Blood—Aneurismal Sacs—Osification of the Arteries—Diseases of the Arachnoid and Pia Mater—Increased Vascularity sometimes accompanied with a deposition of False Membrane—Thickening and loss of Transparency—Effusion of Serum tinged with Blood—Ecchymoses—Deposition of Lymph, Pus, and Fibrin—Looseness or Separation of the Membranes—Preternatural Firmness of Adhesion—Tumors—Serous Cysts—Flattening of the Convulsions—Hypertrophy—Inanition.

Distension and corrugation of the dura mater.—On removing the calvaria, you may find the dura mater preternaturally distended, and presenting the appearance of a bladder fully inflated, and elastic to the touch. This you may regard as one appearance evidentiary that death has arisen from a natural cause. That cause may be hypertrophy of the brain, from too abundant nutrition causing too rapid growth; or it may arise from the effusion of fluid between the membranes or into

the ventricles. On the other hand, you may find the dura mater in the opposite state, corrugated and falling down quite loose upon the convulsions, conveying the idea that its contents had not filled its cavity. This condition may result from the want of the due nutrition of the brain, in consequence of diseases which have impaired the digestive organs and produced extreme emaciation, or in consequence of the gradual failure of the powers of nutrition in advanced age; or it may arise simply from the accidental puncture of the dura mater on the removal of the calvaria, and the consequent escape of fluid which had been contained beneath it.

You will generally find, when inflammation attacks the dura mater as an idiopathic disease, that the vascularity which denotes the existence of this affection is seated chiefly in the membrane which lines the internal surface of the dura mater, commonly regarded as a reflection of the arachnoid. How intensely red this membrane may become under acute inflammation, you see represented in this drawing. From a careful attention to the history of the case, you may distinctly trace the commencement of the inflammation to the dura mater; but it is seldom that the affection is confined to this membrane. It almost always extends in the progress of the disease at least to the arachnoid. The portion of the dura mater implicated in the state of inflammation, as has been already shown, is sometimes very distinctly circumscribed; but at other times it extends over all that part which covers one hemisphere, the longitudinal sinus affording the line of separation.

Inner surface spotted with clusters of red points.—Occasionally you may find the inner surface of the dura mater spotted over with clusters of red points like the effusion of blood in purpura. You will generally be safe in regarding this appear-

ance as the result of natural disease. The disease in which it has its origin may, indeed, be seated in some distant organ, the disease of such organ producing in its progress some irritation in the brain. Instead of red points, you will sometimes observe minute spots, or small lines, of a dark colour, which communicate to the membrane, for a considerable extent, a hue actually black. It is the result of extravasation of blood. At the moment the extravasation takes place it may have produced an attack of apoplexy or of palsy, which not terminating in death, and the other portions of the blood having been absorbed, the only remaining trace of the event may be the appearance which you see represented in this drawing.

Deposition of matter between its laminae.—

As the result of common inflammation, the dura mater is liable to a remarkable degree of thickening, and to the deposition of new matter between its laminae. The deposition may go on slowly increasing, without at first producing alarming symptoms, until, on reaching a certain point, violent convulsions supervene, in a paroxysm of which the patient may expire; at other times the deposition may take place with great rapidity, and occasion death quite suddenly. A gentleman, aged 60, had long been afflicted with severe pain in the head, and giddiness, the attacks of which generally went off with vomiting. At length the giddiness was attended with such a complete loss of muscular power, that on attempting to move he fell to the ground if not prevented. In these attacks he never lost his recollection, and he recovered completely in a few minutes. At length he was seized with hemiplegia, from which he partially recovered; but this was followed by a sudden attack of convulsions, in a violent paroxysm of which he died. Along the upper part of the right hemisphere of the brain, there lay a remarkable tumor, five and a half inches long, two and a half broad, and about half an inch in thickness. It was formed by a separation in the laminae of the dura mater, and a deposition of new matter between them. This new matter in some places was white and firm, in others it consisted of more recent coagulable lymph, firm, yellow, and semi-transparent; at its anterior part there was a cavity containing yellowish serous fluid. The dura mater all around the tumor was considerably thickened, and so were the coats of the longitudinal sinus. The surface of the brain where the tumor lay was depressed, so as to retain an impression of its figure; but the substance of the brain itself was healthy, both in this part and in every other.

A boy, six years of age, was seized suddenly with pain over a circumscribed spot on the left side of the head, followed by drowsiness; the attack proved rapidly fatal. On the part corresponding to the seat of the pain there was found a tumor, the size of a large hazel-nut, formed by the separation of the laminae of the dura mater, and the deposition between them of a bloody serous fluid. There were several similar tumors, but of smaller size, along the course of the longitudinal sinus on the left side.

Deposition of bone.—On the inner surface of the dura mater you may find deposited plates of bone, which may vary in extent from the most minute spiculae to the enormous masses of bony matter which you see in this specimen. Such depositions seldom fail to produce severe cerebral diseases—epilepsy, convulsions, coma, or apoplexy, in a violent paroxysm of some one of which, suddenly induced, death may take place with extreme rapidity.

Tumors.—From the same surface of the dura mater you may find growing tumors of different kinds, which, slowly increasing, at length produce such a pressure upon the brain as is no longer compatible with life, which may be extinguished quite suddenly in a paroxysm of convulsions, or in coma, terminating almost as rapidly in death.

Diseases of the sinuses.—Connected with these various diseases you will often find the sinuses of the dura mater changed in structure; their parietes thickened, solidified, almost cartilaginous; their cavity either entirely obliterated, or greatly diminished, and containing, instead of blood, purulent matter, or coagulable lymph, which may even become organized, and give to the vessels the appearance as if they were injected with wax. The calibre of the vessels being thus filled with solid matter, the transmission of blood through them may be impossible; and you may occasionally find hundreds of small vessels which have given way in consequence of this obstruction to the return of the blood. Such a condition of the blood-vessels must, of course, greatly add to the intensity of the cerebral disease.

Diseases of the glandulae Pacchioni.—Even the glandulae Pacchioni may become so exuberant in their growth as decidedly to obstruct the passage of the blood. In this manner they may lay the foundation of apoplexy, and other violent and rapidly fatal cerebral affections. Cases are on record in which they resemble fungous growths, and in which the patient has died in coma, or a paroxysm of dreadful convulsions. Sometimes, in connexion

with fungoid disease of the brain, these bodies assume the appearance of grumous blood.

Adhesions between the dura mater and the arachnoid.—Between the dura mater and the arachnoid you will often find preternatural adhesions, sometimes so slight that they are easily detached, but at other times so firm that they cannot be removed without bringing away parts of the brain. These adhesions are always the result of inflammation, and this inflammation may be caused by natural disease; but it may also be the result of external violence. You may find the adhesion connected either with a circumscribed spot of inflammation, or with superficial ulceration, or with abscess, tumors, and other lesions of long standing. When you find the adhesion connected with a circumscribed spot of inflammation, look carefully for some indication of external violence, of which it is very frequently the consequence.

Effusion of serum between the dura mater and arachnoid.—Between the dura mater and the arachnoid it is not common to find any accumulation of serum, although these tissues taken together form a very extended surface, which consists of serous membrane. In some cases of chronic hydrocephalus, however, this is the original situation of the accumulated fluid, and more especially in those in which the malady is accompanied by spina bifida; while in other cases of chronic hydrocephalus the fluid obtains this situation in the progress of the disease by escaping from the ventricles in which it was secreted.

Effusion of blood.—But the space between the dura mater and the convolutions is a very common situation for the effusion of blood; sometimes in consequence of the spontaneous rupture of blood-vessels at the surface of the brain, the ruptured vessels being occasionally visible; sometimes in consequence of the erosion of blood-vessels during the progress of ulceration seated in the cineritious substance; sometimes in consequence of the rupture of blood-vessels deeply seated in the substance of the brain, the effused blood tearing and breaking down the cerebral fibres, and thus finding its way to the surface of the convolutions. Effusions of this kind may be the sole result of natural disease. It is a very common situation for the blood to occupy which is forced out from a ruptured vessel during an apoplectic seizure. But such an effusion of blood may be the result of external violence. A blow on the head may produce it by causing a rupture of the capillary blood-vessels, and a laceration of the cerebral fibres. The quantity of blood effused from the direct effect of the blow may at first be small, but it may go on progressively increasing

until at length some violent cerebral affection supervenes, and life is suddenly extinguished. Whenever, therefore, you find blood effused in this situation, inquire minutely into the history of the case, and examine closely every part of the skull, with a view to the detection of some trace of external violence, the evidence of which will often be discovered if carefully sought for.

Aneurismal sacs.—Effusion of blood in this situation may be the consequence of the bursting of an aneurismal sac which you may find seated in the trunks of the carotids, or in their larger branches; but sometimes the aneurism so deranges the circulation as to cause the rupture of other vessels, while the aneurism itself escapes.

Ossification of the arteries.—Rupture of the vessels, and the consequent effusion of blood, may also be the consequence of ossification of the arteries. Between the coats of the arteries a quantity of osseous matter is sometimes deposited, by which they lose in a great degree their contractile and distensible powers; with this loss of contractility and distensibility their tenacity and strength are proportionally diminished, and hence, whenever any causes occur to quicken the circulation, or to retard the return of blood by the veins, an unusual quantity of blood accumulates in the arteries, and these vessels, no longer capable of any considerable degree of distension, are ruptured. It is seldom that the rupture takes place in the large arterial trunks; it is almost always in the minute capillaries.

The deposition of bony matter in the arteries of the brain may take place at any period of life; but it is a frequent occurrence in advanced age. It is usually found in the carotid and basilar arteries; but occasionally a large portion of the arteries of the brain are involved in the malady. Sometimes, without any deposition of bony matter, numerous arteries are entirely obliterated—a condition which deserves your notice, as one among other facts evidentiary of a diseased state of the circulation, which may have prepared the way for an attack of some violent cerebral disease proving rapidly mortal.

Diseases of the arachnoid and pia mater.—The other investing membranes of the brain, the arachnoid and the pia mater, are far more frequently the seats of fatal disease than the dura mater. These membranes are so similar in structure, and so associated in function, that their morbid states are intimately blended, and it is rare to find disease in one while the other is sound. The most common disease to which they are subject is inflammation, indicated by increased vascularity; so that these membranes, instead of being colour-

less and transparent, present in patches, or over a large portion of their surface, a bright scarlet colour. The fine arteries distributed to them gradually enlarge until they become capable of permanently admitting the red particles of the blood. When the vascularity is confined chiefly to the veins, instead of presenting a bright scarlet colour, the membranes assume a dusky or purple hue. The enlarged veins are often exceedingly turgid with dark-coloured blood, constituting the state of venous congestion.

Increased vascularity sometimes accompanied with the deposition of false membrane.—Often there is no other morbid appearance whatever in the brain but this increased vascularity of the membranes. You may find the vascularity confined to a small spot. You may find it very general, or even universal. In either case it is capable of proving fatal with great rapidity. A person may be seized suddenly with a paroxysm of violent convulsions. Headache may precede this attack, but it may not. Vomiting also may precede it, but this symptom likewise may be absent. The convulsions may pass into coma, and the coma may terminate rapidly in death. From a state of apparent health a person may be dead in a few hours with no other symptom than these, and on examining the brain, to ascertain the cause of death, you may find every portion of the cerebral substance perfectly sound; you may be able to detect no appearance but increased vascularity in the arachnoid and the pia mater. Often, however, this increased vascularity is accompanied by a deposition of false membrane between these tissues. This false membrane is sometimes spread uniformly over the surface of the convolutions, while at other times it dips considerably between them.

A child, two years of age, was seized suddenly in the morning with severe and long-continued convulsions, which left her in a dull and torpid state, in which she did not recognize the persons about her. In a few hours the convulsions returned, and continued to recur in paroxysms of great severity, in one of which she died, in the evening of the third day. The pia mater throughout was in the highest state of vascularity, more especially between the convolutions. When the brain was cut vertically, the spaces between the convolutions were marked by a bright line of vivid redness, produced by the inflamed membrane. There was likewise a deposition of adventitious matter between the arachnoid and the pia mater.

A girl, nine years of age, awoke suddenly in the middle of the night screaming from violent headache, and exclaiming that some person had given her a blow on

the head. Soon afterwards she was seized with violent and long-continued convulsions, followed rapidly by profound coma, out of which she never recovered, but died in this comatose state on the following day. The arachnoid and pia mater were unusually vascular, and a layer of adventitious deposit was spread out between the two membranes.

After some previous indisposition, a child, six years of age, was seized with severe headache, considerable hesitation in the speech, and occasional delirium, followed quickly by convulsions, in a violent paroxysm of which she continued without intermission for the space of two hours, and then died. Every part of the brain presented a perfectly healthy appearance, excepting a small spot on the lower surface of the anterior lobe of the right hemisphere, where it lies over the orbit. There was at this place a distinct deposition of adventitious matter, scarcely larger in extent than a shilling. No indication of disease could be discovered in any other organ.

But a highly vascular state of the pia mater may prove rapidly and unexpectedly fatal without convulsions or coma, apparently from a sudden depression of the vital powers after intense excitement. There may at first be no indication of disease, except an unusual quickness of manner; to this may succeed constant talkativeness, and a total want of sleep; a state of depression, alternating with an almost maniacal excitement: the patient either lapses into a state of coma, or sinks exhausted. A lady who had suffered much distress of mind, which had impaired her appetite and deprived her of sleep, fell into a state of incessant talking, alternating with screaming and singing. For two days she continued in a state of alternate excitement and coma, when she died. The only morbid appearance that could be discovered in any organ was a highly vascular state of the pia mater, with numerous red points in the substance of the brain.

All the circumstances and cases to which I have now adverted, are of the greatest interest in a judicial point of view. The sudden and unexpected seizure of a person, previously in apparent health, with such violent illness, the peculiar symptoms which have been described as accompanying such illness, and the rapid death, may often give rise to the suspicion of violence produced by human agency; it may be impossible to remove this suspicion, or to ascertain the truth, without an examination of the brain. On examining the brain, and finding no more appearances of disease than some of those which I have described, you may at first be in

doubt whether they are adequate to account for the fatal event; it is therefore of great importance that you should previously study these morbid appearances, as indications of natural and rapidly mortal disease, that you may be conducted to a correct conclusion by a clear and exact knowledge of the subject.

Thickening and loss of transparency.—You may find the arachnoid and pia mater thickened, opaque, or of a milky hue, either in spots or in large patches, following the course of the blood-vessels. Beneath these membranes there is frequently poured out a quantity of serum, which, collected in the delicate meshes of the pia mater, assumes a gelatinous appearance. This appearance is connected with a great variety of causes, which it is necessary that you should be capable of accurately discriminating. When the quantity is very small, it is doubtful whether it should be regarded as the indication of any morbid action during life, or should not be considered rather as the effect of that congestion which forms a part of the process of death. But in proportion as the quantity is large, it is indicative of disease. It may be the result of inflammation, having its primary seat in the membranes themselves; it may be caused by any condition of the system which obstructs the free circulation of the blood through the brain, and particularly by diseases of the lungs and heart; but it may also be produced by external violence—suspension, submersion, and suffocation; and you will often be called upon to determine to which of these two classes of causes it is to be referred.

Effusion of serum tinged with blood.—Occasionally you will find the serum effused in the meshes of the pia mater tinged with blood, an appearance which will deserve your careful attention. It may be the result of the rupture of the minute vessels of the pia mater caused by external violence, as concussions or blows; but it may also be produced by transudation after death, in those states of the system in which the blood retains its fluidity, and in which there is a tendency to transudation through the coats of the blood-vessels on the trunk and extremities; or by transudation consequent upon the commencement of the process of putrefaction. The importance, in a judicial point of view, of a cautious consideration of all these circumstances, must be obvious to you.

Echymoses.—You will often find blood effused in minute quantities between the arachnoid, and within the meshes of the pia mater, forming small patches of echymosis, varying in size from a sixpenny piece to that of a crown. This is some-

times the result of natural disease, especially when death has been preceded by violent convulsions, as in hooping-cough, epilepsy, hydrophobia, &c.; but at other times it is the consequence of external violence, concussions, blows, falls, &c. It may be produced by a narcotic poison, as opium, and it is not an unfrequent consequence of death from suffocation. Whenever, then, in a case which has given rise to suspicion, you meet with these circumscribed effusions of blood in the membranes, endeavour to ascertain whether death were preceded by convulsions, the consequence of some natural disease; and if there be no evidence of this, search diligently for the proofs of poison, suffocation, or some form of mechanical violence.

But sometimes the quantity of blood effused in this situation is extremely great, extensive coagula of which you may find lying either upon the surface of the convolutions, or occupying and very much increasing the interspaces between them. The ruptured vessels from which the blood has issued may be the minute capillaries on the surface, which have given way in consequence of some obstruction to the course of the blood in the large trunks; or the ruptured vessels may be seated deep in the substance of the brain, and the blood which has escaped from them may have forced its way to the surface. The violence of the symptoms produced by blood effused in this situation being commonly proportionate to the quantity which has escaped, when the quantity is very small, it may occasion no appreciable inconvenience; but when large, it may produce all the ordinary effects of pressure, from dulness, drowsiness, and coma, to complete apoplexy. It has been observed, that a given quantity of blood deposited between the convolutions produces severer symptoms than an equal quantity more diffused upon the surface of the brain.

Deposition of lymph, pus, or fibrin.—The meshes of the pia mater are also sometimes filled with lymph or pus; at other times a layer of fibrin is deposited between the pia mater and the cineritious substance, undoubted results of inflammation, the exact cause of which it will be the very purpose of your investigation to discriminate and verify.

Looseness or separation of the membranes.—On attempting to remove the membranes from the brain, you may find that they separate with unusual facility, by the slightest force applied to them, a condition which is generally connected either with congestion of the membranes, or with the effusion of a small quantity of serum in the pia mater. You may also find the membranes absolutely and com-

pletely separated from the convolutions. This is seldom the result of natural disease; it is generally the consequence of violent concussion, and is often accompanied with laceration of the convolutions.

Preternatural firmness of adhesion.—Sometimes, on the contrary, the membranes adhere to the convolutions with such firmness that they can be separated only in flakes and shreds; another undoubted result of inflammation. When the inflammation has been intense, the force which is necessary to remove the membranes seems to be on the point of tearing the cineritious substance; and when the inflammation has been both intense and long-continued, it is absolutely impossible to separate the membranes without bringing away portions of the cineritious substance. This morbid condition is sometimes circumscribed to a small and defined space; at other times it extends over a large surface of the brain. When circumscribed to a defined spot, it is often connected with external violence, but the more extensive it is, the stronger is the indication that it is connected with a general and not a local cause of inflammation.

Tumors.—When tumors grow from the arachnoid and pia mater, they are usually of a fungoid character, and are associated with fungoid diseases in other parts of the body, and, therefore, however rapidly they may have become developed in the brain, and however violent and sudden the cerebral symptoms produced by them, you cannot hesitate to refer their origin to some natural cause.

Serous cysts.—And equally to a natural cause must be referred those serous cysts which are occasionally formed in the membranes, and which are sometimes coeval with life itself, and always exceedingly slow in their growth. It is probably owing to this slowness in their growth that they produce so little inconvenience; not only the brain itself, but the bony parietes of the skull, being often moulded to their form.

Flattening of the convolutions.—A morbid change in the appearance of the convolutions, interesting from the number of natural causes which may give rise to it, is the obliteration of their defined and rounded figure, and the general flattening of their substance. Perhaps the most common cause of this change is the collection of serum within the ventricles, which occasionally accumulates in such quantity as altogether to destroy the divisions of the convolutions. It may also be occasioned by the effusion of blood into the ventricles, or into the substance of the brain; nay, even by a great degree of congestion of the blood-vessels; by abscess

at the surface or in the substance of the brain; by softening of the cerebral substance; by tumors, which, though generally limited to a confined space, yet often compress the convolutions of one hemisphere, or a portion of a hemisphere; by several diseases of the dura mater which have been already described; and by a morbid growth and thickening of some particular portion of the skull.

Hypertrophy of the brain.—But the most interesting cause of this flattening of the convolutions, as the result of natural disease, is the disproportion between the volume of the brain and the capacity of the skull, occasioned by too abundant nourishment, and consequently too rapid growth, of the brain. To this affection, which has been termed hypertrophy of the brain, the special attention of practitioners has been recently directed. On removing the calvaria, the dura mater (as has been already stated) looks preternaturally distended, and fits closely to the brain within. On taking away the membranes, the whole surface of the brain presents a flattened appearance, so that the distinction of convolutions is either entirely obliterated or is marked only by superficial lines. At first view it is impossible not to conceive that the ventricles are distended with fluid; yet, on examining these cavities, they are found smaller than natural, and containing less fluid than in the sound condition of the brain. There is, at the same time, a remarkable deficiency of fluid in the whole brain; so that when it is sliced in all directions, little or no serum, or blood, escapes. The blood-vessels are preternaturally empty, and the investing membranes are unusually dry. The cerebral substance is denser and firmer than natural, and conveys the idea that it is in great abundance and massive. On the removal of the calvaria, the brain instantly springs upwards, like an elastic body on the removal of a compressing force, as if the organ were too large for the case that contained it.

The early stage of this affection, which is generally slow in its progress, is completely latent; like several other diseases of the brain, it may go on increasing, up to a certain point, without giving any indication of its existence; but at last, passing beyond the point which is compatible with the due exercise of the functions of the brain, violent symptoms suddenly supervene, and the patient appears to die after a few hours' illness.

Hypertrophy of the brain is most common during the period of childhood, when all the nutritive functions continuing very active, the mental faculties are also in

constant operation. I have observed it particularly in children who, from the general configuration of the head and their precocious intellectual and moral attainments, afforded unusual promise, and excited expectations of future power and excellence, too soon buried with their object in the grave. The fair and pinky skin, the blue eye, the light hair, the quick and active mind, so often connected with this temperament, as well as the tendency to glandular disease, should always excite alarm, and lead to the adoption of every possible means that can strengthen the physical health, and retard the development of the mental faculties.

But this disease may take place in the adult, and may appear to be very suddenly induced, giving rise to an attack of illness perhaps produced by no manifest cause, and characterized by severe cerebral symptoms, the most prominent of which may be a lethargic drowsiness, attended with a preternatural slowness of the pulse.

It may also be the result of an inflammatory action set up in the brain by external violence, as a blow or a fall. A child, three weeks before its death, fell from a table and struck its head. At the time no serious symptoms were observed; but at length a large tumor, containing a fluid, made its appearance, from which, on being opened, a quantity of pus was evacuated. That day the child had a severe convulsion, which being followed by a continual succession of convulsive fits, in the intervals of which the child was sensible, death took place on the fourth day from the first appearance of the tumor. The most remarkable appearance in the brain was a flattening of the whole surface of the convolutions, with increased vascularity of the cerebral substance.

Inanition.—But you may find the very opposite condition, the convolutions shrunk, wasted, without their usual plump and rounded form; the cineritious substance diminished in thickness, separating easily from the medullary matter; and the whole mass of the latter appearing deficient in quantity. This state of the brain depends on a converse condition of the system—on deficient nourishment from an inadequate supply of nourishing and stimulating blood. It is the natural result of that failure in the nutritive process which is consequent on old age; but it may exist at any period of life, and may be induced by whatever causes impair the nutritive functions; by excessive blood-letting, which may remove suddenly from the system a larger proportion of the solid constituents of the blood than the digestive organs can resupply; by food bad in quality, and deficient in quantity, so that

the material requisite for forming healthy blood is not afforded; by long-continued confinement in impure air, and by the want of the general invigoration communicated to the system by active exercise.

LECTURES

ON THE

ENTOZOA,

OR

INTERNAL PARASITES OF THE HUMAN BODY,

Being part of a Course on Morbid Anatomy, delivered at the London Hospital,

By T. B. CURLING, Esq.

LECTURE V.

HAVING terminated the consideration of the various entozoa infesting man, and the lesions which they give rise to, and having briefly noticed the treatment most appropriate for their removal, it may appear necessary that I should allude to their origin and the mode of their development. From the nature of their local habitation, and the remarkable circumstances under which these animals are produced, this inquiry might be expected to be invested with a peculiar interest, and to throw light on one of the most intricate subjects that have ever occupied the human mind. It offers, indeed, a wide and difficult field of observation, and one, though not unexplored, from which it must be confessed that nothing satisfactory or conclusive has yet been deduced. It is evident that the entozoa must either come from without, or originate in the body of the animal in which they are found. That they have an external origin; that they are nothing more than aquatic or earth-worms, introduced either in their perfect state or in the form of ova, with their organization so modified as to be adapted to their extraordinary situation, though an hypothesis held by Linnæus and supported by Brera, is clearly shewn, both by Rudolphi and Bremser, to be incompatible with our knowledge of the economy of these animals, and of the laws of animated nature. The peculiarity of their organization; the limitation of particular kinds to certain species of animals, and peculiar situations in the body; their incapability of sustaining life when removed from their natural sphere of existence; and their occasional presence in the fœtus at birth, are powerful arguments in support of the opinion that they are deve-

loped in the interior of the animal in which they are observed. Dr. Barry has related, in the Transactions of the College of Physicians of Dublin, a case which is viewed by some as leaving no doubt respecting the external origin of the *ascaris vermicularis*. After a whole family, with their servants, had been infested with these worms for some time, it was discovered that the water of a well by which the inmates of the house were supplied contained an immense number of minute worms resembling the small ascarides, except that they were of a darker colour. Now I am by no means disposed to regard this observation as conclusive, especially as the identity of the worms found in the well was not satisfactorily established by examination of their organization. Besides, it cannot but be viewed as most extraordinary, that at a period when the animal kingdom has been the subject of such diligent investigation, the most common and most abundant of all the entozoa should, if existing out of the body, have only been discovered in this particular instance.

Some naturalists, and amongst others, Pallas, are inclined to believe that the existence of these animals is perpetuated by the ova being transmitted from generation to generation, or communicated from one individual to another. Pallas experimented upon the subject. He inserted the ova of the tænia of a dog into the abdominal cavity of another dog, and on opening the animal a month afterwards, he found some small tape-worms, about an inch in length, in the abdomen. Certain entozoa peculiar to fishes have been detected alive in the stomach and intestines of aquatic birds by which the fishes were devoured. That these worms cannot, however, be readily introduced into the system at all times in this way, is shewn by an experiment performed by M. Schreiber, of the Museum at Vienna. He fed a polecat during six months almost exclusively on different kinds of intestinal worms and their ova, mixed up with milk. On destroying the animal at the end of this period, not a single living worm of any description was found within it. If, then, the ova are communicable, as supposed by Pallas, they must be constantly exposed to causes tending to their destruction—a contingency which would seem to be fully provided for by nature, in supplying this class of animals with the most ample means of reproduction. In surveying, indeed, the economy of the entozoa, which present such great diversity of organization, and the type of almost every form of generation, there is no circumstance so remarkable as the care which seems to have

been taken to insure the continuance of the species, as well in the simple separation of the germ from the parent cyst in the acephalocyst, as in the oviparous generation of the *Sterelmintha*, and the viviparous progeny of the *Cœlelmintha*. Cruveilhier thinks that the more frequent appearance of hydatids in the liver and lungs, organs more abundantly supplied with blood than other parts, tends to show that the germs of these animals are transmitted in the circulating fluid. The ova, however, of some of the Entozoa, as for instance of the *Ascarus lumbricoides*, are so large and heavy, that they cannot be supposed to pass through the capillary blood-vessels, or to be transmitted through the medium of the atmosphere. There are, undoubtedly, several striking objections to Pallas' view of the origin of these animals. It is difficult to comprehend how hydatids and the other entozoa, insulated and included in a cyst, can be propagated in this way. We should expect, too, that some of the species, such as the *spiroptera* and *strongylus*, would be met with less rarely if they are transmitted in an uninterrupted line, and that there would be no interval in the hereditary succession. Notwithstanding these, and even other objections, the view which seems to me to accord best with the imperfect state of our knowledge respecting their origin, is, that the countless germs and ova of these animals are extensively diffused; that they are transmitted from generation to generation, and in various modes from individual to individual; that they enter the system with the food, and perhaps in other ways; and that, constantly subject to numerous destructive agencies, they are only developed under peculiar and highly favourable circumstances.

In conclusion, I may remark that some pathologists and naturalists have cut the matter short by rejecting Harvey's principle *omne vivum ex ovo*, and affirming that the entozoa are the result of spontaneous or equivocal generation, and have endeavoured to support this opinion by the evidence deducible from natural history as to the peculiar character of the animals inhabiting various parts of the earth, and by those facts revealed by the enterprising geologists of later years, which go to prove that different kinds of animals have been created at distinct epochs. To engage here, however, in a discussion on this plausible hypothesis, an hypothesis indebted to the phenomena displayed by the Entozoa for the most satisfactory evidence hitherto brought to its support, would be leading me far away from the course which it has been my aim to follow in the delivery of these lectures.

Spurious Worms.

I stated, when commencing the subject of the entozoa, that living beings are sometimes found in the interior of the body, which have evidently been introduced there from without, and I instanced the botts found in the stomach of the horse. These animals are termed by pathologists *spurious worms*. They are numerous and various, infest many animals as well as man, and gain ingress in different ways. They are generally introduced in the form of ova, or larvæ, and, finding a convenient situation and congenial temperature, undergo their natural changes. They have been known to enter by the nose, mouth, ear, anus, urethra, and vagina, and are sometimes lodged beneath the skin. They are found in the digestive organs, especially the stomach, more frequently than in any other part of the body, where they no doubt obtain admission with the food. They occur sometimes in the frontal and maxillary sinuses, causing headache and other distressing symptoms. A learned entomologist, the Rev. Mr. Hope, has lately communicated to the *MEDICAL GAZETTE* a set of curious tables, including 107 cases of spurious worms which have been found infesting the human body. He enumerates 45 different larval species, and 23 cases in which the species was unknown; 37 of the cases were female, and 32 male; 30 occurred in England, 15 in Sweden, 10 in Ireland, and 3 in Scotland. In seven instances they appear to have produced death. The most common, and perhaps the most curious, of the spurious worms, are the *Æstri*, which belong to the class of dipterous insects. They pass the state of larva and chrysalis, or that of larva only, in the body of the animal on which they prey. Different species are found infesting various animals. Some, as the *horse-botts*, inhabit the alimentary canal and rectum; others, as the *botts* of the ox, rein-deer, and goat, take up their abode under the skin; whilst others, as the *æstri* of the sheep and stag, occupy the nasal sinuses. The *æstrus humanus*, which is described by Linnæus and Humboldt, occurs only in the warmest regions of America. The larvæ reside beneath the skin for about six months, and deep seated pain and inflammation are induced if they are interfered with before they make their appearance at the surface, as small brown flies. Dr. Elliotson mentions the case of a child, who, having eaten part of a high pheasant, discharged from the bowels some months afterwards about a dozen live larvæ, maggots of the common fly. There is no end to the marvellous cases of spurious worms which the records of our profession furnish. Thus we have accounts of bees, caterpillars, spiders, millipedes,

flies of all kinds, earth-worms, leeches, snails, slugs, lizards, toads, frogs, and even serpents, having been thrown up from the stomach, or discharged from the intestines. Many of these cases we must admit, *cum grano salis*, and it is highly probable that in some instances deception has been practised. One of the most remarkable and best authenticated examples of this kind on record is that of a woman whose case is related in the Transactions of the College of Physicians of Dublin. From certain superstitious notions, she had been in the habit of drinking daily a mixture of water with the clay taken from the graves of two priests, and was likewise addicted to eating chalk. During three years and a quarter this woman discharged per anum above two thousand of the larvæ of the beetle, and most of them alive. Both the larvæ, pupæ, and the perfect insects, were simultaneously observed. Many made their escape the instant they were discharged, and ran off into holes in the floor; and two large-winged insects were so lively on their expulsion, that they immediately flew away and escaped!

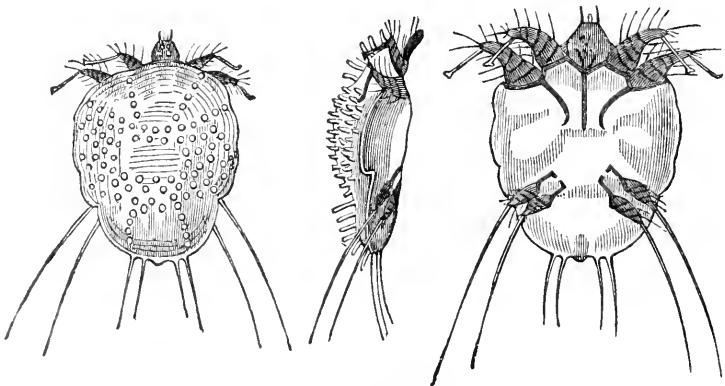
The Ectozoa, or Parasitic Insects.

In addition to the various kinds of worms, there are numerous parasitic insects infesting the human body, which differ from the Entozoa in many points of their organization, but principally in the circumstance that, living in the open air, they possess organs of respiration which, you must have observed, are wanting in all the species of the internal parasitic worms which I have described. They are termed, in contra-distinction to the Entozoa, *Ectozoa*; but they are so seldom connected with morbid changes, that they can scarcely be said to come within the limits of these lectures, and I shall therefore only cursorily allude to them. With some of these parasitic insects you are no doubt sufficiently familiar, and have perhaps occasionally been troubled. The *Pulex irritans*, or flea, and the *Cimex lectularius*, or bug, are too well known to need any description; but if not, they may be observed with great advantage in the wards of the hospital, where, I regret to say, they abound. The *Pulex penetrans*, or *chigoe*, is a sort of large flea, which infests the inhabitants of the West India Islands, and is likewise very troublesome in some of the colonies of South America. These insects get under the skin, and there lodge their eggs, which are very numerous, and are hatched in this situation, often giving rise to ill-conditioned ulcers. They sometimes occasion so much irritation, and such troublesome sores, that amputation of the affected limb has been rendered necessary. *Pediculi*, or lice, the offspring of

filth, and the most offensive of these insects, may at all times be seen luxuriating on the persons of many of the hospital patients. Of these there are three species:—1. The *Pediculus humani corporis*, or body louse; 2 the *Pediculus capitis*; and 3, the *Pediculus pubis*. A due attention to cleanliness is, in general, a sufficient security against the annoyance of these disgusting vermin. Campbell, the poet, in his Letters from the South, mentions an ingenious mode adopted by the Arabs, to get relieved of their numerous companions. Stripping themselves naked, they spread their garments upon ants' hillocks; and the ants, after devouring all the vermin which they find on the clothes, retire from them well satisfied into their nests. Some individuals are more disposed than others to be infested with them. Parent-Duchatelet informs us, in his elaborate work on Prostitution in Paris, that the heads of very few of the unfortunate women on the town, even of the better class, are free from pediculi; but I am not aware whether they are equally abundant amongst the prostitutes in this country. It is stated that the members of one of the first aristocratic houses

in this country have some difficulty in keeping themselves free from the *Pediculus corporis*. Pediculi are said to be more common in cold climates than in warm. They are very numerous in Russia; and it is by no means uncommon to see one of the inhabitants engaged in the friendly act of ridding his neighbour of some of his troublesome associates, a process in which they acquire considerable dexterity. They exist in prodigious quantities in Greenland, where, according to the account of an English traveller, they constitute an article of diet. Pediculi are extremely prolific, and, as you are doubtless aware, may be communicated from one person to another. The *Pediculi pubis* are often caught in sexual connexion; and I may remark, that inunction with mercurial ointment is a tolerably certain mode of destroying them. In inveterate cases, an infusion of the tobacco leaf, or a mixture of turpentine, the strength being properly regulated, may be employed. Some caution is necessary in using the former remedy.

The last of the external parasites of which I shall speak is the *Acarus scabiei*, or itch insect. Although this insect is mentioned by some of the early writers, and has been



Acarus scabiei, or Itch insect.

long familiarly known to the inhabitants of southern climes, its existence has been repeatedly denied by modern authors, and is not even generally admitted at the present day. It was distinctly described by Mousset, an English author, who, in his *Theatrum Insectorum*, printed in 1634, quotes Avenzoar, an Arabian physician who flourished in the twelfth century, and other writers of the fifteenth and sixteenth centuries, as having noticed it. Mousset states that the *Acarus* does not reside in the vesicles, but near them,—a fact fully

confirmed by later observations. The *Acarus* was clearly ascertained and figured by Bonomo, an Italian physician, in 1683; and his account of it was communicated to the Royal Society of London, and afterwards published by Dr. Mead. De Geer gave an excellent representation of it. In 1805, Dr. Adams transmitted a good description of the insect from Madeira, in a letter to Sir Joseph Banks, which was inserted in the *Philosophical Transactions*; and when Dr. Adams published his work on Morbid Poisons, in 1807, this letter

was introduced, with some farther particulars, and a plate containing two accurate views of the *Acarus*. In France the search after this insect was less successful, and for a long period it escaped observation, until the year 1812, when M. Gales, after many experiments at the large hospital of St. Louis, at Paris, succeeded in extracting and designing an insect, which was generally regarded in France as the true *Acarus*. Unfortunately the artist employed by M. Gales figured a mite which had been placed near the *Acarus*, in order to be compared with it; so that when, in 1829, M. Raspail clearly shewed that this insect was nothing more than the common mite found in cheese, further disputes arose, and many of the physicians of Paris, and amongst others Lugol, positively denied the existence of the *Acarus*. Baron Alibert, however, still continued to believe that it existed, although frequently challenged to "produce the beast," by Lugol, who, moreover, offered a prize of 300 francs to any one who would shew it him. Thus matters rested in Paris until the month of August, 1834, when M. Renucci, a student from Corsica, happening to be in attendance at St. Louis, offered to remove all doubts; and stated that it was so commonly found in cases of itch in his own country, that the peasants were accustomed to extract them from each other with pins and needles. He publicly demonstrated the *Acarus*, and obtained the reward. According to Renucci, the itch insect is only to be met with in a small canal, indicated by a slight elevation of the epidermis, and a dull, white, opaque speck, adjoining the vesicle. The *Acarus* is so small as only to be just perceptible to the naked eye, appearing like a minute speck. It is about half a millimetre in diameter. When seen in a microscope, it presents the form of a tortoise, and a white opaque colour. The body is round, and striated with papillæ on its dorsal aspect. The head projects in front, and is provided with a sucker. There are eight articulated feet bristled with hairs: four in front, two on each side of the head, and four behind.

That there is some difficulty in detecting it, and a little tact required, cannot be doubted, since so many have searched for it in vain. The late Mr. Plumbe states that he has extracted many; but I cannot say that the few attempts which I have made were attended with success, though I have no doubt whatever of the existence of this insect, having seen it distinctly in Paris. That the *Acarus*, when placed upon the skin, can communicate the itch, has been fully proved; but whether they necessarily and invariably exist in the vicinity of the vesicles, and are the cause of their formation,

has not yet been satisfactorily determined. Cutaneous diseases, somewhat similar to the itch, to which some of the inferior animals, as the horse and sheep, are subject, have been shewn to depend on *Acaris*.

ON THE FUNCTIONS
OF THE
ENCEPHALON AND CEREBELLUM.

Cretâ an carbone notandum?

To the Editor of the Medical Gazette.

SIR,

In the last number of the *Lancet* there is a notice of a paper of mine communicated some time since to the Phrenological Society of London, in reply to which you will oblige me by inserting the accompanying. The notice alluded to has been communicated, I understand, by the secretary of the Society in his official capacity, but is not so much an analysis of the paper as a critique upon it, being, in its defects of omission and commission, much more like a reply in defence than an official abstract by the secretary of a learned Body. I wish very much, therefore, to be permitted to supply the deficiencies of that abstract, and, through the pages of the *MEDICAL GAZETTE*, to give to such readers of the medical weekly journals as care about such matters, the means of judging as to the objects aimed at in the paper, and the materials employed for the purpose of attaining those objects. All I shall attempt is an outline of the main facts and arguments which alone can be expected to interest the personally unconcerned readers of the *MEDICAL GAZETTE*; and, though not in possession of a copy of the paper, I have no difficulty in supplying from recollection, with the aid of memoranda and of the article in the *Lancet*, an outline of both, which, in its phrenological bearings, may be depended on as substantially correct. With the secretary's official reply to my paper in the *Lancet*, and my analysis of my paper in the *MEDICAL GAZETTE*, those that think it worth the trouble may easily satisfy themselves as to the pertinency of my facts and inferences, as well as of the value of the comments on them by the learned Secretary.

The paper was entitled "Facts and Inferences relating to the Development and Use of the Cerebellum." The

main object of the paper was to state some facts which had occurred to me in the course of inquiries respecting the nutrition, density, volume, &c. of the viscera under different circumstances of disease, age, sex, &c. &c. In the course of my observations upon the encephalon, I conceived I had found its development to be subject to a law not in harmony with the cerebral physiology of Gall, and that the cerebellum in particular appeared to increase and decrease in volume and weight, with the rest of the encephalon, and not contemporaneously with, or proportionally to, the development and activity of the functions attributed to it by Gall. From which I concluded that some error existed in the views of phrenologists respecting the cerebellum in particular, and that, taking the proofs of the Gallian views of the uses of that body as samples, and they are considered favourable samples of the evidence on which Gall has based his cerebral physiology, there is much reason to regard the whole fabric of phrenology as still hypothetical in science and conjectural in practice, and ample reason to distrust all phrenological diagnoses not aided or corrected by personal experience. Such was the scope of the original part of the paper, of the argument of which, as a whole, the following is an outline sketch.

After some introductory observations upon the reception experienced by Gall on the first publication of his system, and the necessity there was for reform in the methods of investigation employed in moral and mental science, and the tendency of Gall's labours to draw philosophy out of libraries into the field of nature, and from fruitless meditations to pregnant observations and experiments; and after avowing my conviction that Gall was right so far as he attributed to innate instinctive propensities and susceptibilities brought out in early life by sensible impulses acting as occasional causes merely, all our universal Judgments and Emotions, (such as the belief of the existence of external Nature, of Good and Evil, of Personal Identity, of personal Free Agency, of Supreme Power, &c. &c.; and the feelings of conjugal and parental Love, of Love of power, of praise, &c. &c.); and after making the further admission, that the old notion, that to certain classes of powers or affections of the mind were especially appropriated

certain parts or regions of the encephalon, as the seats of those faculties or affections respectively, was not destitute of plausibility, or even of probability,—I then proceeded to state that although I had often conversed on Phrenology with adepts, and had thought on it with some attention, and read several of the books of authority, I had been unable to satisfy myself that it had as yet attained to the rank of a true science in point of evidence, or to that of a useful art in point of practical certainty; and for several reasons, of which those that most influenced my own judgment, were—1. The impracticability of a correct estimate of the mass of the *living* encephalon, or of any part of it, in individual instances, without some as yet undiscovered measure of its density, which is variable. 2. The impracticability of an estimate more than conjectural, of the functional vigour of the encephalon, or of any part of it, in individual instances, without some as yet undiscovered measure of sensorial organic or vital power, *other than Experience*. 3. The want of satisfactory evidence that the frequent correspondencies between the form and the volume of the brain (as measured on the skull,) and the activity and energy of the mind, are really related to each other as *cause and effect*, and are not (like the parallel correspondencies between the temper, disposition, and other mental conditions, and the features of the face,) *mere coincidences*; or in other words, the want of proof that mental energy is not related to cerebral development by a *community of causation merely*, both being effects of the same higher and mysterious forces that form *in utero*, and sustain through life, in its personal identity, the moral-physical Being; but that the former (mind) is subject to and dependent on, and consequently, of necessity, proportional to the latter (brain) as a function on its organ, or an effect on its cause. Having, then, referred to other sources of doubt and difficulty, more especially to, 1. The want of subdivisions in the encephalon, corresponding even partially, to the Gallian organs; 2. The want of any means of estimating any organs seated at the base or in the interior of the brain, and the want of proof that there are not many so seated,—I proceeded to state that, in the course

of inquiries undertaken without any reference to the cranial physiognomy of Gall, I had met with new facts which appeared to me to harmonize with the considerations above referred to, as opposed to the Gallian Phrenology. Those facts were a tolerably numerous series of measurements by weight and volume of the encephalon as a whole, and of the cerebellum apart, from a comparison of which measurements it appeared that 1. the cerebellum is less in the female and greater in the male in the same ratio with the whole encephalon, and 2. less, likewise, at early and at advanced ages, as often as the encephalon is immature or decayed,—that, 3. the cerebellum, to which Gall attributes the sexual instinct as its principal if not sole function, bears after a few weeks from birth and up to extreme old age (that is, before the existence, during the activity, and after the failure of the generative power) one and the same proportion to the whole encephalon, viz. a ratio fluctuating between 0·125 and ·100, or between 8 and 10 to 1; and that 4. in females, in whom the physical appetite is comparatively feeble, and often almost if not altogether null, the same ratio prevails as in the male. These facts were detailed in two tables, containing the results of 150 and odd measurements of encephala and cerebella, belonging to as many subjects, of both sexes (nearly 80 of each sex), and of

various ages, from early infancy up to decrepitude. The first table gave the average weights of the encephalon and cerebellum at each age, for each sex separately; the second table contained the absolute weights of the cerebella, classified according to weight, sex, and age. From the first table it appeared that, with one exception, viz. that of an infant three weeks old, the cerebellum bore, as already stated, to the whole encephalon, a ratio varying from one-eighth to one-tenth, at all ages and in both sexes; while in the infant the ratio was one-sixteenth only. From the second table it appeared that the female cerebellum (like the female encephalon) differs by between —·12 and ·13 from that of the male, the former averaging nearly $4\frac{3}{4}$ ounces avoirdupois; the other, or male, averaging about $\frac{5}{8}$ of an ounce more, or something more than $5\frac{1}{4}$ ounces.

As the second table has been misunderstood both by some of the leading speakers on the occasion of the reading of the paper, and by the executive officer of the Society who drew up the imperfect abstract published, according to the usual practice of the Society, in the *Lancet*, I subjoin the facts in a form more simple, and less likely to be misapprehended by gentlemen not familiar with the tabular method of proof and illustration:—

MALE.			FEMALE.			
Age.	No. of Cerebella.	Average.	No. of Cerebella.	Average.	Difference.	Avoirdupois Weights.
		oz. dr.		oz. dr.		
Under 15	4	4 12	5	4 7	5	
15 to 30	10	5 4	10	4 10	10	
30 to 50	23	5 10	20	4 12	14	
50 to 70	32	5 12	20	4 10 $\frac{1}{2}$	17 $\frac{1}{2}$	
Above 70	7	4 9	8	4 4 $\frac{1}{2}$	4 $\frac{1}{2}$	

The preceding measurements were made with a balance by no means delicate, and therefore more advantageous to the male cerebellum as the larger, than to the female, but with a balance turning readily with a less weight than

ten grains. From those measurements it appears that the female cerebellum varies no more than half an ounce from infancy to age; that it is heaviest from 30 to 50; that it averages between 15 and 30, and between 50 and 70, about

the same weight, but weighs rather more in the advanced interval of age. Also, that in the male it varies, and much more than in the female, viz. nearly $1\frac{1}{2}$ ounces in the course of life, being heaviest between 50 and 70, in which interval of age it would appear to exceed the average for the quincennial, 15—30, by about half an ounce; that, under puberty, it outweighs the average of the years above 70; and that its average weight in the male (as in the female) increases steadily from youth to age, and only declines after 70. Now these facts, which are not improbably too few for scientifically accurate results, are notwithstanding, so far as they go, of some physiological interest, and of considerable weight, in my judgment, in their bearing on the Gallian views. From those facts, the medical statist will perceive that the supposed organs of the sexual instinct would appear to become, in both sexes, more developed as life culminates and declines, and the generative desires and powers ripen and decay—viz. from 15 to 70; and that they are most developed when sexual life is comparatively torpid, or quite extinct—viz. 50 to 70; and the weight of the inference is rather increased by the fact that half the males between 50 and 70 were 60 years of age and upwards, whereas of the females of the corresponding ages, 40 only were above, and the remainder (viz. 60, or three-fifths) under 60 years; also, that of the males between 30 and 50 years, more than half were 40 years and upwards, while, of the corresponding female cases, 75 exactly, or three-fourths, were over 40 years of age.

Since the preceding facts were tabulated for the Phrenological Society, I have collected about two dozen others of the same kind (including two or three negro cases), which, so far as I have examined, lead to pretty similar results; and I am still engaged in the collection of facts relating to the volume, weight, and density of the viscera in various circumstances, of which I mean to publish the results at no distant period. But in the meantime I conceive myself entitled *provisionally* to conclude that the cerebellum has other uses than those assigned to it by Gall; functions connecting it

rather with the well-being of the individual than the continuation of the species, and that the sexual instinct is, if attached to it in any especial manner, in all probability one of its subordinate uses, and by no means, as he seems to have conceived, its principal, if not sole, function.

Before concluding, I may mention that I have omitted all consideration of differences of volume, from finding the volume and weight so nearly parallel, that the former might, for every purpose of the paper, be without inconvenience omitted; and the same remark will probably apply to measures of density, respecting which, however, I have not yet facts sufficient for tabular exposition.

To conclude:—The Fellows of the Phrenological Society, speaking through their organ, the Secretary of the Society, seem (to judge by the official abstract and criticism contained in the *Lancet*) well satisfied with the results deducible from the facts I have collected. For my own part, having no system to defend, it was perfectly indifferent to me what conclusions I should arrive at; and I, of course, am contented with the results, however interpreted and estimated. My appearance before the Society was in some measure an accident, and wholly an after-thought. If that accident have in any degree contributed to fortify their science with new facts and arguments, I shall be amongst the last to regret it; and if, on the contrary, my observations shall in any way be found to weaken the evidences of phrenology, I shall, I conceive, have rendered important service to the Gallists, by drawing their attention to an assailable point in their defences. Thus, whatever be the result, I am not without hope, that, with that compassion which it is natural that the Secretary of the Phrenological Society should feel for my ignorance of the science, and of the writings of Gall, my learned friend will mingle a *tant soit peu* of grateful feeling for my happy temerity in venturing before the Society with a statement of my facts and inferences respecting the development and uses of the cerebellum.

JOHN CLENDINNING, M.D.
Sec. R.M.C.S. L. &c. &c.

Wimpole-street, Feb. 1838.

FRACTURE OF THE FEMUR,
WITH SEPARATION OF THE EPIPHYSES.

To the Editor of the Medical Gazette.

SIR,

Nov. 27th, 1837. — Stillwell, aged 12 years, a youth of delicate appearance and fair complexion. In consequence of the wheel of an unloaded timber carriage passing over the right thigh, the bone was separated at each epiphysis, and fractured obliquely through its middle. The limb presented an unusual extent of deformity, the knee lying on its fibular side. That the bone was broken was evident on the most superficial view; but in what manner, and where fractured, was not so easily determined. On making gentle extension, the limb appeared straight, and the fracture adjusted; but on placing it over a pillow, the knee fell again to the outer side, as if it moved on a pivot. I then passed my finger slowly and carefully from the condyles upwards along the bone. About an inch above the patella I detected a transverse separation, attended with a soft inaudible crepitus. Continuing the examination, I found the fracture in the middle of the os femoris to be oblique. Still continuing the examination, I found another separation of the bone in a transverse direction, immediately under the trochanters, which conveyed the same sensation as the one above the condyles. Now the epiphyses are generally ossified at seven or eight years of age; but being for the most part moistened by the synovial fluid, their external surface is still somewhat cartilaginous, and they are not yet united to their bones. It is at eighteen or twenty years of age that the epiphyses are entirely ossified.

The limb was placed on a pillow upon a double-inclined plane, and kept steady by means of three splints (as described in the twelfth volume of the *MEDICAL GAZETTE*, page 607), loosely connected with broad tape, which was tightened at the end of the seventh day. On the fourteenth day I found the union of the epiphyses so firm that, on removing the lateral support, the limb maintained its position. On the twenty first day I was satisfied that union of the fracture had taken place, but not such

as would have justified me in allowing the thigh to be unsupported. At the end of the fifth week I permitted the boy to use the limb gradually; at the same time cautioned the parents of the great risk of the parts becoming again separated from another accident.

As it is in the dissecting-room only where we can prove our assertions, it is with some diffidence I have published the above case, knowing how easily statements may be made unaided by proof; and yet, with all the sincere desire to relate facts, I have only described that which was evident to my senses. I am not aware that the separation of the epiphyses from an act of violence has ever been described in any English work on surgery. I feel fully convinced I have in children overlooked such a fracture or separation; its nature at once shows how easy it is of replacement, and how much sooner a firm union takes place than when the bone is fractured. As regards the process of ossific union, I firmly believe in the following statement of M. Dupuytren: "That when the two ends of the broken long bone are brought together and maintained in accurate apposition, they become, in the first instance, united by a swelling and subsequent ossification of the soft parts immediately surrounding the ends of the broken bone. This forms a kind of case for the fracture, by which the broken ends are held together for a time; then the broken ends so held together unite, and become consolidated." Thus he distinguishes between the first or provisional union produced by the swelling and subsequent ossification of the surrounding soft parts, and the ultimate or definite union by the agglutination of the two broken ends. The former he calls the "cal provisional," the latter the "cal défini." "The first period from the occurrence of the fracture to the end of about the eighth day, the periosteum, the cellular substance, the muscles, and other soft parts immediately surrounding the broken bone, become swelled and form a considerable tumor, which can be felt externally. The thickest part is opposed to the situation of the fracture, and it is gradually lost on the natural surface of the bone in both directions from the fracture. In the commencement the tumor is somewhat red, that is, there is some determination of blood to it; towards the end of the eighth or tenth day this redness

disappears, and the swelling, when examined internally, has a white appearance. In the early part of the time there is, in fact, a mixture of coagulated and fluid blood; blood is effused from the vessels which are in the situation of the fracture, and it subsequently coagulates. At this period there is either a kind of viscid fluid found between the ends of the bone, or there is something like a reddish granular substance between them, a sort of spongy substance, nothing osseous. From the tenth to the twentieth or twenty-fifth day, the swelling becomes less in size; it is considerably firmer in its structure. It begins to assume a fibrous or cartilaginous, or something of an osseous structure, and during the same time the medullary cavity goes through the same process; it likewise begins to assume an osseous or cartilaginous structure. During this period, the second period, the parts admit of motion, that is, the broken ends can be moved on each other, but no *crepitus* (*i.e.* from the 10th to the 20th or 25th day.) The third period, extending from the 20th or 25th to the 40th, 50th, or 60th day, and in that third period the external swelling becomes ossified. The internal swelling of the medullary matter becomes ossified in like manner; and, in fact, the external and internal ossification becomes firm enough to allow the patient to employ the broken bone, yet the actual extremities of the fracture are not united; they still remain connected with the red spongy substance." Although the fracture is at this time sufficiently consolidated to enable the patient to use the limb, yet it is weak enough, from an act of violence, to become separated again. "The fourth period extends from the fortieth, fiftieth, or sixtieth day, to the fifth or sixth month; and in that time the external swelling becomes completely and firmly ossified, and when you then examine it externally it seems as if the periosteum were continued over the swelling to the sound parts of the bone. It has assumed a strong ossific character; the medullary membrane is ossified also, and the ends of the bone which have not been closely united are found to be connected by bone; so that you only see the situation in which they have been previously separated, as a faint line, when the fracture is sawn through. In the fifth

period, which extends from the fifth or sixth month to the twelfth month, the external provisional callus is entirely absorbed, and the union of the bone becomes so firm that the bone will break more easily in any other part than at the seat of fracture. The provisional callus having served its purpose of keeping the broken ends of the bone together for a given period, is entirely removed; and that portion of callus produced by the medullary membrane is equally completely removed." This, then, is the course taken by nature in repairing the fractures of long bones, when the two ends are kept in proper apposition. This surely must be an answer to the *no-splint* treatment, though, generally speaking, it is better to let an error die away, which it invariably will do when tried by experience. Yet, on a review of the "days gone by," we cannot but be struck with surprise with the numerous theories and modes of practice recommended, adopted, and condemned; then lying dormant for a century or more, and again revived with all the ardour and impetuosity of an original idea;—witness the treatment of curvature of the spine. My case in a measure affords an example why some fractures are united in a much shorter period of time than has been taught by surgeons of the present day; it will also assist in explaining why the "no-splint" treatment has enlisted some supporters, their cases having occurred in children. Those who have advanced the doctrine of treating fractures without splints only reason from a negative premise, from which nothing can be inferred. They have opposed the universal with the particular. Much of the confusion, fallacy, and sophistry of reasoning, arises from these points not being sufficiently attended to, and distinctly and rigidly investigated. In reflecting on the mechanical part of the treatment of fractures it must be borne in mind that bones possess the property of solidity and a non-yielding, for the purpose of giving origin and insertion to the muscular system, and forming levers, which are moved by the muscles in various directions. Now the muscular system is in a state of constant action, and dependent on the nerves of volition and reflexion. The movement of the fractured portions must, if displaced, lacerate, irritate, and disturb the healthy process of ossific union; also undue

compression on a tumefied part will produce inflammation and muscular irritation. My patient suffered from an oblique fracture—a kind of fracture the most frequent. In the first volume of the *Edinburgh Medical Essays*, published in 1747, Dr. Alexander Monro has a paper wherein he has given, from the pen of Dr. Porterfield, some abstract mathematical calculations why long bones are generally fractured in an oblique direction: they are worth perusal. Dr. Porterfield's demonstration is as follows:—"The absolute force whereby bones resist being broken transversely is the power of cohesion uniformly diffused over the whole cohering surface; and this power of cohesion is compounded of all the powers exerted in every point of that surface. But these powers resist in parallel directions (being all perpendicular to the transverse section of the bone) with velocities proportional to their distance from their centres of revolution; and therefore the strength of the bone, or total resistance of all these powers, shall be as their sum multiplied into the velocity of their common centre of gravity—that is, as the area of the transverse section of the bone multiplied into the distance of its centre of gravity from the fulcrum or centre of revolution."

Your obedient servant,

JOHN GRANTHAM.

Crayford, Kent, Jan. 27th, 1838.

P.S. Feb. 12.—On Saturday I received the *Brit. and Foreign Medical Review*, for January 1838, wherein is an extract of a paper by M. Roux de Brignolle, from the 55th No. of the *Medical Press* of July 12, 1837, on the Separation of the Epiphyses. He records cases as having occurred in subjects up to the eighteenth year; therefore I am not right in my remark, as to my case being the only one recorded in an English work which I sent you. In M. Roux's paper, he vindicates his claim to originality against M. Guérin.

CASES OF HYDATIDS.

To the Editor of the Medical Gazette.

SIR,

If the following cases of hydatids possess sufficient interest to merit a place

533.—XXI.

in your respectable journal, I shall feel obliged by your inserting them.

I am, sir,

Your obedient servant,

H. BARRETT.

Feb. 7, 1838.

Thomas Jordan, a labourer, aged 32, after suffering with dyspeptic symptoms for two or three years, was seized about a year and a half ago with acute lancinating pain in the left side of the chest, inability to lie on that side, fever, and other symptoms of pleuritis. There were dulness on percussion, and ægophony. He was bled, and took calomel, opium, and tartarized antimony, and in six days was much better. He was then attacked with violent vomiting and purging, acute pain in the epigastrium, dry brown tongue, weak rapid pulse, and great prostration of strength. On examination, I observed an elastic tumor extending from the epigastrium to the umbilicus, and occupying the anterior half of both hypochondria. This was painful on pressure, and dull on percussion.

In the fluid matters which had been discharged by vomiting and by stool, there was a quantity of hydatids, all burst, and resembling the skins of green gooseberries, for which, indeed, I at first mistook them. Some of these, when entire, must have been as large as hens' eggs.

A large blister was applied to the epigastrium; effervescing saline medicines, and chalk mixture with opium, were given, and in a few days the more urgent symptoms abated. The purging, however, continued four or five weeks longer, but was ultimately checked by euphr. sulph. gr. ss., et opii, gr. ss., taken three times a day, the hydatids having ceased to come away three weeks before. The vomiting has continued more or less ever since.

The tumor was now somewhat contracted in its lateral dimensions, and softer, still emitting a dead sound on percussion. The most prominent and painful part of it was situated midway between the pit of the stomach and umbilicus, and a little to the left of the median line. Pulse 100; dyspnoea, cold perspirations, and acceleration of the pulse on slight exertion; pain in the tumor on pressure, and during deep inspiration; tongue quite clean; appetite tolerably good; but eating, and

even pressure on the tumor, generally caused vomiting.

Considering that a state of chronic inflammation existed in the cyst containing the hydatids, I now endeavoured to remove this by means of counter-irritants. Several blisters were successively applied, and small doses of calomel were given, so as to affect the gums, with considerable benefit. A plaster containing tartarized antimony was then applied, which caused sloughing and two deep ulcers, each as large as a half-crown. These continued discharging for several weeks, with relief to the pain and difficulty of breathing, and with a manifest diminution of the tumor. Ung. iodini and Lugol's solution were now tried; but as the man was getting worse they were quickly laid aside, and a seton was inserted over the most painful part of the tumor, and retained more than six months. During this time he gained flesh and strength. The vomiting has frequently occurred, but has always been relieved by hydrocyanic acid or creosote. He resumed his employment last Michaelmas, but some time after complained of pain and "a knob," in the middle of the left clavicle; and a few days after the bone broke in this place as he was turning in bed. Contrary to my expectations firm union took place at the usual time.

The tumor has not much diminished latterly, but is not so distinct in its outline as it was. The digestive functions are naturally performed, with the exception of slight sickness occasionally felt after eating.

It is probable that the cyst is connected with the liver, though there has been no particular disturbance of the functions of this organ. It must also be in contact with the stomach and some part of the intestines, and must have discharged some of its contents through openings formed by ulceration, into these viscera. I am inclined also to believe that the pleuritis arose from the irritation caused by the hydatids extending to the pleura. The fracture of the clavicle from so slight a force might lead us to suspect malignity, but the man's appearance does not warrant such a supposition.

Thomas Judge, a labourer, aged 44, previously healthy, but whose brother has scrofulous disease of the humerus

and tibia, was attacked, about five years ago, with inflammation of the lungs terminating in abscess. Since then he has been constantly suffering from dyspnoea, cough, and profuse expectoration, and has remained weak and pallid.

The expectoration appears, from his description, to have been muco-purulent, occasionally mixed with blood, and sometimes containing "finger-like substances, consisting of a skin filled with thick matter." He has, however, continued his work with very little intermission.

On the 30th of last October he was seized with rigors, constriction, and obtuse pain in the chest extending to the right shoulder, cough, and expectoration of mucus tinged with blood, inability to lie on the right side, &c. There was dulness on percussion, and absence of respiratory murmur at the lower part of the right side of the chest, but above this there was distinct crepitous rattle; bowels costive; urine high coloured. Pulse 90, and weak; face pale and slightly yellow.

As pneumonia existed, I bled him twice till he became faint, the blood each time being covered with a thick, tenacious buff, and deeply cupped. He took calomel, opium, and tartarized antimony, and had a large blister applied to the chest.

Nov. 5th.—The month became sore; the crepitous rattle had nearly subsided, and was replaced by the mucous and sonorous; the latter continually varying in situation and intensity. Still no respiration in the lower part of the right lung. Headache, cough, appetite, &c. not much better.

7th.—He began to expectorate hydatids in considerable quantities, some as large as walnuts, and others as small as a pin's head. The sputa consisted of tough mucus, lying at the bottom of a watery fluid in which the hydatids were swimming. Headache and dyspnoea rather better; cough and appetite still very bad, with a sensation of fulness at the stomach after taking food; pulse 84.

Omit the Cal. and Opium. T. Hyoscyami ʒij.; Tinct. Camph. co. ʒiv.; Vin. Ipecac. ʒij.; Aq. Pur. ʒvij. M. Cap. Coch. iij. t. d.; P. Doveri, gr. v., o. n.

The hydatids continued to be discharged, and the symptoms to improve,

till the 12th, when they ceased; the headache, cough, pain in the chest, &c. became aggravated, and there were now profuse night-sweats. Pain in the right hypochondria on pressure.

Mist. rep.; Morph. mur. gr. ss. o. n.

On the 16th the hydatids reappeared in the sputa, and the night-sweats abated; other symptoms better. Pulse 80; soft.

R. Tine. Hyosey. 5ij.; Tinet. Camph. co. 5ij.; Inf. Cascariæ ʒviiss. M. Cap. Coeh. iij. t. d.; Pil. Mur. Morph. o. n.

22d.—No hydatids discharged, consequently the cough, dyspnoea, headache, night-sweats, and appetite, much worse; but on the 24th these symptoms were alleviated by their reappearance. Pulse 84.

R. Inf. Gent. co. e. Pot. Carb. &c.; Morph. Mur. gr. ʒ. o. n. Emp. Picis. co. e. Ant. Tart. pectori.

The hydatids continued to be discharged, and the health to improve, till December 1st, when they ceased, and the usual effects followed. The night sweats were so profuse, that a vapour hung round the bed.

5th. Expecterated hydatids, and was much better.

R. Quinin. Sulph. gr. ss.; Acid. Sulph. dil. ℥xxv.; T. Hyoseyam. ℥xx.; Aq. Pur. ʒiiss. M. t. d. sumend.

The opiate at bed-time was omitted, as it seemed to increase the perspirations.

I did not visit him till the 11th, when I found that on the 6th the cough and dyspnoea had come on violently, with spitting of blood and hydatids, and that he had not perspired since. Appetite much better; no pain; pulse 80.

Quinin. &c. rep.

There was now dulness in percussion at the lower part of the right side of the chest, in which situation no respiratory sound could be heard, except when the chest was fully expanded, when there was a harsh, short, sonorous rattle at the bottom of the chest. The cough came on at times with great violence, and before these paroxysms occurred, loud, mucous, and sonorous rattles could be heard without applying the ear to the chest. The man continued to improve till the 15th, when the hydatids ceased, and the cough, head-ache, night sweats, &c., returned.

17th.—He expectorated in 24 hours as much as a pint of mucous watery fluid, and a number of small hydatids, and one as large as a nutmeg, which was not burst as the larger ones had hitherto been. Cough still very troublesome, but the other symptoms much better; no sweats; appetite good; pulse 72.

Quinin. gr. j. &c. rep.

26th.—Much better; expectorates nearly a pint daily, containing hydatids; sweats a little every night, but feels stronger; pulse 70.

Resumed his work on the 28th. Continued to spit hydatids, and to improve in strength and flesh. The perspirations and cough were still troublesome at night, and his breath was short.

The last time I saw him was on the 20th of January: he was still discharging hydatids, but had very much improved in his general health.

In this case, as in all others of the kind, the liver appears to be the birth-place of the hydatids. The opening which was first formed into the lungs was too small to allow of their free expulsion; but as it became larger, the checks to their discharge were not so frequent, or so long-continued. The large hydatids which were expelled at the commencement were all burst, but latterly several of considerable size have been discharged in an entire state; these are white, semi-transparent, and mostly round; some, however, are long, and have small round appendages, which communicate with the parent hydatid by a minute opening.

In regard to the treatment, after the inflammation had been subdued, I saw no indication but to support the strength. The opiate, which he took for some time, by checking the cough, favoured the accumulation of mucus, and prevented the expulsion of the hydatids, which was invariably followed by night-sweats, headache, &c., these symptoms subsiding as soon as a free expectoration had been re-established. To produce the latter effect I at one time gave him a scruple of ipecacuanha, but with so little benefit that I was not inclined to repeat it*.

* If the opening between the cyst and the lungs should remain sufficiently large to allow the free exit of the contents of the former, it is possible that a gradual contraction of its cavity may take place, and that the man may live some years with some degree of comfort. But it is more probable that he will ultimately die with the symptoms of hectic fever.

ON INHALATION IN TUBERCULAR
PHTHISIS PULMONALIS.*To the Editor of the Medical Gazette.*

SIR,

THE favour of your insertion of the following further observations, on the subject of inhalation in Tubercular Phthisis Pulmonalis, will oblige, sir,

Your obedient servant,
CHARLES SCUDAMORE.

Wimpole Street,
Feb. 10, 1838.

Three years have elapsed since my last communication in the *MEDICAL GAZETTE*, of the successful treatment of some cases of phthisis pulmonalis, in which the inhalation of iodine and conium formed a material remedy. I hope, therefore, that a concise summary account of my further experience in this lamentable disease, and in chronic bronchitis, may prove an acceptable offering to the profession. That it has been my fate to meet with a much larger portion of failure than success, in phthisis, cannot fairly be made the reproach of my plan of treatment; for it has happened to me, for the most part, that I have not been consulted till the advanced stage of the disease, when the lungs having become extensively disorganised by the spread of tubercles almost universal, by cavities the consequence of the softening process, and often with the complication of ulcers in the intestinal canal, my task could only be that of studying to alleviate symptoms, and to soothe, by tender care, the last weeks or days of life.

It not unfrequently happens with consumption to be overlooked in the early stage of the disease, when probably attentive appropriate measures might be attended with happy results. The danger is serious, however delayed the consummation of the event, when tubercles have begun to infest the lungs. But from the fear and anxiety of friends inducing a flattering rather than a true view of the case, with a reluctance to listen to any voice that gives a warning of danger; and from the tenderness probably more than the better judgment of the medical adviser, which makes him join in the delusion, it follows, unhappily, that the early indications of dangerous disease are not detected or sufficiently regarded, and the most valuable period for giving assistance is lost;

perhaps irrevocably. I have known very many instances exactly exemplifying this statement. The consoling opinion has been pronounced, "that at present the lungs appear to be safe," or that, notwithstanding the evident delicacy of constitution, and necessity for care, yet "that there is no reason to consider the lungs at all affected;" while at this very time tubercles do exist, and consequently danger is established.

It will be said, that in the commencement of tubercular phthisis, there is much obscurity in the symptoms, and that we cannot discover by auscultation whether or not tubercles exist, as they may be so minute and disseminated, or so very partially clustered, as not materially to interfere with the vesicular respiration. I freely admit the insufficiency of the stethoscope to detect the early state of the tubercular disease, on many occasions at least; but there are several circumstances largely instructive.

If the patient have lost strength and flesh, without apparent cause; have recently become short-breathed on slight exertion, especially on making the least ascent; have more or less of short dry cough, a quickness of pulse, night-restlessness, and perhaps some dulness on percussion, here and there, in the upper parts of the chest, we have surely great reason to fear that there are tubercles formed. If other members of the family have died from consumption, the suspicion is painfully increased.

This is the most important period for the adoption of a systematic plan of treatment, and the one at which I should have the most confidence in the efficacy of the inhalation of iodine and conium. But a suitable treatment embraces many other points of management.

I believe I may state with truth, that when the symptoms become so conspicuous as to receive medical notice, the most general principle of practice has been to seek, as the *summum bonum*, a retardation of the pulse, and that by means of sedative medicines, the use of digitalis especially, with a slender, light, and cooling diet. Sometimes this is restrained to farinaceous food, milk, vegetables, and fruits. I apprehend that only in the very acute cases of consumption, which occasionally happen to young persons, is such a mode of treatment at all proper. When it is not for-

bidden, as in such cases, by the high degree of hectic fever so prevailing as to be almost continued, it appears to me of great importance to sustain the powers of the constitution by a very generous and supporting diet, with some porter; and with or without the addition of diluted wine, instead of aqueous beverage. I also make choice of tonic alternative medicines rather than those of a debilitating nature; soothe the nervous system at night by anodynes or sedatives, and employ such mode of external counter-irritation as may be best suited to the individual case, for this must be varied. Some patients are so much lowered and distressed by blisters, that they become inadmissible. I find the oil of croton liniment often to be very useful, and much less inconvenient in its effects than the tartar emetic ointment. I attach importance to the daily ablution of the chest with a mixture of purified acetic acid, Eau de Cologne and water, substituting for water an infusion of tannin, when perspirations are excessive. All the general means of Hygieia are of course to be pursued. Lastly, and what, in my mind, is a most important link in the chain, I make use of the inhalation of iodine with conium, according to the formula stated in the second edition of my work*, and in the MEDICAL GAZETTE, vol. viii. p. 157. And here I will take occasion to make some further remarks on the use of this remedy.

It is of the utmost importance that all the medicines used for inhalation should be quite pure, and that the tinctures should be saturated with their ingredients. If the iodine be in the dry state it requires an equal portion of the iodide of potassium, that it may remain in permanent solution in the mixture; for "three" grains of this salt, expressed in my formula, read "five;" and according to the strength of the solution, so the proportions used are to be smaller. It is better always to add the conium at the time of using the inhaling. At the temperature of 90° the volatile properties of the iodine are given off very sensibly; but the conium requires more heat, and that of 120° is not too much for the iodine. This degree, therefore, I most recommend; or if the patient have not a thermometer, let the instruction be, to

put the water into the inhaler (first warming it a little to prepare it,) quite as hot as the finger can bear without pain. The inhaler should be kept immersed in rather hotter water during the process. A good glass inhaler also is a material consideration. If it be small, and the tubes too contracted in the bore, the difficulty of inhaling would be great to the invalid, whose respiration is easily embarrassed; whereas, with a fit apparatus*, the process is perfectly easy, and not fatiguing.

It would be inconsistent with my present purpose to enter at length into the *ratio medendi*; for which and other particulars I beg leave to refer the reader to my publication.

The Case V., detailed in my second edition, very clearly demonstrates the useful power of the inhaling treatment. This gentleman was labouring under the well-marked symptoms of pulmonary tubercles, much increased beyond the usual degree in which he had been for some time affected, and so as to excite immediate alarm for his safety. He recovered in so satisfactory a manner under my care, that he was enabled to pass the two following years at his seat in Scotland, and to enjoy the pleasures of his gun and hunting. He wrote me the most cheerful accounts, and described himself as perfectly well. I received a similar statement from his friends. He was of the most active disposition, too convivial, and passionately fond of field sports. At the end of the second year he became quite careless of his safety, often exposing himself to the most unfavourable weather, and frequently committing great excess at the table in wine and mixed spirits. Thus he renewed in the most active manner the disease which had been so satisfactorily controlled. I learnt that in a short time hæmoptysis occurred, attended with urgent cough; this was followed by a severe attack of influenza; and when, at a late period, I saw him in London, his situation was hopeless. A large excavation had formed in the upper part of the right lung, and a smaller one in the left. He was much emaciated, but he combated with his disease for a considerable time, till at last the strength of his constitution yielded.

I allude to this case for the purpose

* Cases illustrating and confirming the remedial power of the Inhalation of Iodine and Conium in Tubercular Phtthisis, and various disordered states of the lungs and air-passages.

* The best inhalers which I have seen are to be procured at Garden's, chemist, 372, Oxford-street; and Perkins's, chemist, 147, Oxford-street.

of shewing, as it appears to me, the beneficial influence of the inhalation, in the first instance, on the tubercular disease. Had the patient led a life of proper care, it is reasonable to suppose that the recovery which he had obtained might have been lasting.

I wish now to refer to some of my other published cases; and first to those which will be found in the number for February, of the fifteenth volume of the *GAZETTE*.

J. A., Case I. He had remained comfortably well from November 1834, till the period of influenza*, in the beginning of 1837. He was violently attacked with the worst symptoms of the epidemic, and fell a victim in a short time.

CASE II.—This gentleman continues to enjoy very excellent health, has married, and established himself in life.

The subject of Case III. has been free from all returns of disease.

CASE IV.—This gentleman has acquired so settled a state of health, that within the last few months he has married.

CASE V.—This young woman informs me, that since the period of my last report, she has very seldom had occasion to resort to the inhalation; but that when she has done so, she has never been disappointed in its good effects.

Of much the larger part of the cases related in my second edition, I could make an equally favourable report. I shall advert only to some of the most important examples.

CASE I. (2d series).—This lady has continued in the enjoyment of very comfortable health, possessing good strength and spirits up to the present time, with the exception of having suffered from influenza on each occasion of the epidemic occurring. It is most satisfactory to consider, that her remarkable recovery was so perfect as to admit of such a trial of its stability.

CASE VIII.—I have received the most agreeable accounts of the continued health of this gentleman.

CASE IX.—This young woman has continued to enjoy comfortable health.

I will now offer an outline of a small part of my more recent experience in consumptive cases:—

A lady, aged 22, mother of three children, consulted me in April 1835. She had been falling off in health for a year past. Her youngest child was fifteen months old, and she had nursed it for twelve, but with difficulty. She related that she had within the last few months lost flesh and strength very rapidly; that on six or seven occasions she had coughed up small quantities of pure blood, but latterly it had only appeared occasionally, streaking the expectoration.

At my visit I found her suffering from harassing cough, with the inspiration easily hurried by slight exertion, and a distressing sense of restraint over the chest. She had daily hectic fever, and severe night perspirations; the pulse upwards of 100; the animal heat 100°; the appetite was lost, and the spirits much depressed. The sputum was considerable in quantity, purulent in appearance, offensive in odour, and slightly streaked with blood. The signs by auscultation were, much mucous rale on each side, but on the right especially, and there mixed with the sibilant. In this part also the voice was very resonant, and on percussion the sound was dull. I considered that there were tubercles, but that no softening had taken place.

It had been thought proper to keep her on very low diet; and it had been candidly stated to her friends, that as further trials with medical treatment could not in all probability render any benefit, it would be most expedient to try change of air, and trust to that alone.

Without delay I prescribed the inhalation of iodine and conium; gave tonic and alterative medicines; used counter-irritation by means of oil of croton diluted with spir. ammon. comp. over the most affected parts of the chest; and elsewhere it was washed daily with a lotion of infusion of tannin, purified acetic acid, and eau de Cologne, slightly tepid.

She experienced very sensible relief from the inhalation, and in a short time was enabled to lie down in bed comfortably on either side, from which she had been long prevented, and the embarrassment of her breathing on exercise was most satisfactorily relieved;

* How severely did this epidemic test the vital power of all delicate constitutions, and more especially all those affected with pulmonary disease, or predisposed to phthisis pulmonalis.

she could take a considerable walk without inconvenience.

This lady quite recovered her health at the end of four months, and has since continued well, a period of almost three years.

A gentleman, aged 25, tall and slight, of consumptive family, and had recently lost a brother from consumption, when under the influence of mercury, exposed himself to wet and cold, and in consequence was attacked by acute rheumatism. Having, after a long period, regained his health, he joined in the sports of the field; and one day, when much heated and in a perspiration, forded a river, and kept on his wet clothes for some hours. Catarrhal fever and cough quickly ensued, and in a short time his state, as described to me, was that of a person labouring under the first symptoms of consumption. He took an early opportunity of visiting Torquay, and resided there some months, but made no progress towards health, although he had diligently followed the advice of his medical friends. One acquaintance had recommended him to consult me; but another, and he was medical, dissuaded him, assuring him that inhaling iodine would do him serious mischief. He passed through London on his way to the continent, Rome being his destination; and consulted a physician, who, with other means, prescribed the inhaling of creosote, which he tried steadily. It did not disagree, but proved of no benefit. He was exposed to many inconveniences in travelling, and at one place was detained in quarantine on account of cholera. His disease increased so much before and after his arrival at Rome, that his state became truly alarming; and his debility and emaciation had reached a fearful height. It occurred to him that, having possession of my book, he might adopt the treatment recommended in it, under the guidance of an English physician whom he selected*. He went afterwards to Naples, and followed up the treatment

there. The terms of approbation in which he expressed himself, when he described the extraordinary benefit which he derived from the inhaling of the iodine with conium, were enthusiastic; and he declared himself to have amended from the first moment. In the account which he gave me of his case at that time, when at Rome, he stated that he had daily two paroxysms of hectic fever, and suffered most severely from cough, attended with an offensive expectoration, which was frequently coloured; shortness of breath; loss of sleep at night; copious perspirations, and other bad symptoms. The hectic fever was immediately controlled by the inhalation, and the respiration remarkably relieved.

I found him still an invalid, but, according to his report, surprisingly increased in flesh and strength; and he was in good spirits. His weight, he said, was within seven pounds of what it had been formerly in his best health. By auscultation and percussion I discovered evident signs of obstruction at the upper part of the right lung, and slighter in the same situation of the left. There was no indication of cavity, but the mucous membrane of the bronchial tubes was not in a healthy state, and there was still cough. The pulse was moderate, and the animal heat 97°.

I recommended that the inhalation should be resumed; internally sarsaparilla, with small doses of the iodide of potassium; and externally, the daily use of ablution with the lotion of which I have before spoken, together with friction; that he should live well, regulating his diet, and lead a life of care.

The following statement of a young lady, who was evidently suffering from the early symptoms of tubercular irritation, and whose sister died from consumption, is, I think, sufficiently interesting to be related. I will give it in her own natural language:—

“At a period when I was suffering from one of my usual attacks of painful constriction of the chest, accompanied by general depression of spirits, and sometimes with shivering, followed by heat, shortness of breathing, and a troublesome cough, I was recommended by Sir C. Scudamore to make trial of his iodine and hemlock inhalation; and I cannot too strongly express my gratitude for the effects which it produced in relieving all my symptoms. After seve-

* This was Dr. Thompson, whom I have just now had the pleasure of seeing. He informs me that when he first saw this gentleman, he was evidently labouring under tubercular phthisis, and having a cavity in the upper part of the right lung. After some interval he consented to the use of inhalation, although with some reluctance. Recourse was soon had to the mixture of iodine with conium; and Dr. Thompson confesses himself quite satisfied with the benefit which the patient received from the inhaling.

ral times inhaling, the sense of soreness seemed healed, the pain removed, and I felt more expansion and strength of chest than I had experienced for a long period. To express more strongly my feeling from the inhaling, I should say, the effect was like the opening of valves that had long been closed."

Were space convenient, I should relate the cases of a gentleman, aged 54; of a young lady, aged 20; and of a medical practitioner, aged 30; in which the most unequivocal symptoms of tubercular disease were strongly developed; in which there was every threatening of danger; and in all of them I was happily quite successful.

I prefer, however, to substitute the case of Dr. Davidson, of Setham, near Forfar, subjoining the communication with which he has favoured me of other cases, and requesting that his letter may be copied *verbatim* :—

Case of Dr. Davidson.—About the 1st of September (1836), I was seized with catarrh, which continued with slight aggravation till the 1st of October, when I was affected with severe pain in the left mammary region, and all at once the sputa, which had usually been from one to two ounces in quantity in the twenty-four hours, assumed a puriform appearance. By general and topical bleeding the pain of my side was relieved, but soon returned in a slight degree, and was then removed by a blister; yet I felt uneasiness behind the sternum. I was confined to bed, and my strength was much reduced. My pulse, which in health is 60, was then between 80 and 90.

He proceeded to state he was apprehensive of phthisis, and wished my opinion as to the treatment of his case. In his second communication, in January 1837, he reported that he had inhaled the mixture of iodine with conium, and taken the medicines which I had prescribed, with much benefit; but that a severe attack of the epidemic influenza had reproduced his symptoms, and compelled him to suspend the treatment for a short time. In his third letter, Jan. 22, 1838, he relates that, after an interval of three weeks from the time of being attacked with the influenza, he had returned to my method of treatment, of which he thus speaks :—"I resumed the inhalation, and with the same happy effect as formerly, for in the course of about three weeks the puru-

lent expectoration gradually subsided, along with the cough and pains of the chest, and by the 1st of April I was so much improved in health and strength, that I was able again to fulfil my active professional duties. It is proper to mention that, while using the inhalation, I took at bed-time a small dose of the solution of acetate of morphia, and once or twice in the day a few drops of acid. hydrocyan. in an effervescing draught. Over the site of pain in the left mammary region, I kept up counter-irritation from time to time, by emplast. cantharid., or by ung. antimon. tartarizat. I regulated the bowels by a powder composed of pulv. rhei. carbon. sodæ, p. zingib. et sulph. ferri.

My much lamented friend, the late Dr. Mackintosh, of Edinburgh, as well as others, assured me that the left lobes of my lungs were rather emphysematous, which renders me somewhat more liable to catch cold; but, with that one exception, thanks to Divine Providence and your kind assistance, I am now in as good health as I have been for many years past. With two exceptions, I have not even had a catarrh during the present winter. These attacks were slight and of short duration, although I have been daily and nightly exposed, in the discharge of my professional duties. Having given you these particulars of my own case, I will now offer you the relation of a few other cases, in which I have given the inhalation of iodine and conium a fair trial.

January 7, 1837, I was requested to visit Mr. G—, aged 66, parish schoolmaster, of Kirkden. My assistant stated that he complained much of tightness across the chest, pain in the left mammary region, severe cough, with mucopurulent expectoration; bowels costive; tongue dirty towards the root; fauces red and tender; pulse 100, and the body weak and much emaciated. My assistant attended the patient, and treated his case with much skill, till the 3d of March, when I visited him myself, and found that from the usual remedies which had been tried he had derived no benefit whatever. Upon auscultating the chest, I found the vesicular murmur faint on the left side, and marked by bronchial respiration, or mucous râle. The mucopurulent expectoration had greatly increased since he came under our care, and his body was reduced to a perfect skeleton. I

recommended the inhalation of iodine and conium, and gave him your work to read. He commenced on the 6th, and inhaled the medicated vapour three times a day till the 1st of May, when he had recovered so much that I made him reduce it to twice; and by the 15th of the said month he gave it up, and resumed his teaching by the 25th, and has continued in the full enjoyment of good health ever since. It is proper to state, that with the exception of one small blister, and occasionally a gentle dose of some opening medicine, he used no medicine whatever after he commenced the inhalation.

Mrs. F—, a woman of a very delicate constitution, *æt.* 40, had been complaining from the 9th December 1836. As Dr. McDonald, my assistant, attended her during the early part of her illness, I am not able to give you the early history of her case. When I saw her, about the beginning of February, she was much emaciated, complained of severe pain in her right side, with tightness across her chest; incessant cough, with copious expectoration, amounting to two English pints every twenty-four hours. Copious night sweats, with well-marked hectic fever. Pulse 120. The matter expectorated was of a highly purulent nature, and her relations and others who saw her were of opinion that she could not survive many weeks. The stethoscope applied under the right clavicle detected well-marked pectoriloquism, and the respiration absent to a very considerable extent. The parts were very dull on percussion. I prescribed the inhalation of the iodine mixture, &c. renewed the blister, and ordered a wine-glassful of the *mistura ferri composita* of the London Dispensatory, to be taken three times a day. The change which was effected in this patient in the course of a fortnight, was quite remarkable. Want of room prevents me from giving you particulars, but I am proud to say that, in the course of one month, her cure was greatly advanced, and, on the 10th of April, she paid me a domiciliary visit, having walked a distance of three miles. I saw her lately, in perfect good health.

June 14th, Mrs. S—, a farmer's lady, about 47 years of age, who had been long subject to winter colds and weakness of the chest, requested me to visit her. I found her in bed. She complained of tightness across the chest, with shooting pains betwixt the

shoulders. Her cough was quite distressing, and she expectorated, with much difficulty, about *x.* of mucopurulent matter every twenty-four hours. Pulse 96. Auscultation detected bronchophony in the right axilla; respiratory murmur absent over a great portion of the right side, and very weak over the whole. She had night sweats, and had lost much flesh. I prescribed the inhaling mixture, and iron medicine, all of which she used with much diligence. Being at a distance, I saw her but seldom. On the 20th she expressed herself better, and her pulse down to 86. On the 25th she was visibly improved, and on the 6th July she was able to drive this length along with her husband. I did not see her again until last week, when I had occasion to visit one of her daughters; and I am happy to state that she continues well.

Mrs. Y—, the wife of a very respectable farmer, *æt.* 34, who had been subject to severe attacks of inflammation of the lungs and pleura, was delivered of a child on the 15th July last. Had a fair recovery, but a cough, which had annoyed her during pregnancy, now became much worse, and her sputa assumed a purulent appearance. Pulse 70, tongue clean, and bowels nearly regular. Complained of severe tightness across the chest. Auscultation discovered that the vesicular murmur was weak all over the chest, and nearly extinct in the scapular and interscapular regions on the left side. On the 5th of August I prescribed the inhaling mixture, which she continued to use, with many of the other auxiliary remedies used in such cases, for nearly two months, but, I am sorry to say, without deriving the least benefit from any one of them, when I considered her case quite hopeless, and for some time past I have adopted a palliative course of treatment only. She is still in life, but ere many weeks elapse she will have closed her earthly account. The now evident issue of her case has both surprised and disappointed me, as I, at the outset, had every reason to expect a favourable termination.

July 30th, Mrs. W—, *æt.* 27, of a dark complexion and narrow chest, the lady of a minister of the Church of Scotland, in the Highlands, came to her father's in this neighbourhood, and was placed under my care. She had been mostly confined, since January

1837, with a hard confined cough, which prevented her from obtaining any thing like regular rest in the night; shooting pains in, and tightness across, the chest, and great difficulty of breathing; and for the last four months she had purulent expectoration, to the extent of 5 oz. or 6 oz. every twenty-four hours, with occasional night sweats, and slight accessions of hectic fever at night. Pulse 120. She was very thin, and had much anxiety in the expression of her countenance. Respiratory murmur very faint, and nearly extinct in the subclavian region on the left side. Percussion natural. I ordered the medicated vapour of iodine and conium to be used three times a day; the body to be sponged with vinegar, &c. with the use of the flesh brush; a blister to the chest, and the *mistura ferri comp.* to be taken three times a day. In the course of a week her breathing was much relieved, expectoration much freer, the circulation somewhat mitigated, and she slept soundly at night.

She continued under my care till the 30th of August, during which period she had been regular in using the iodine mixture, in form of vapour, and also the *mist. ferri*; and at that time, so much had she improved, that had it not been that her sputa, though much diminished in quantity, still continued purulent. I should have pronounced her cure complete. When she went home, she took a supply of medicines with her; but her husband wrote me, some time afterwards, that she had given up the inhalation, as the use of it distressed her much, and she got considerably worse. Again he wrote about a month ago, for a fresh supply of the iodine mixture, and also for a few bottles of the *ferri* mixture; and I learned from her sister, a few days since, that she is again much better.

I have only left myself room to say, that from the very favourable issue of a great majority of these cases, my opinion of the influence of the inhalation of iodine and conium is highly favourable; and you are at full liberty to make any use of this letter that you think proper. With many thanks for your disinterested kindness to me, and with every good wish, I am, with the highest respect,

Yours ever,

A. DAVIDSON.

Setham, Jan. 22, 1838.*

I indulge the hope that my present communication, and these unreserved details, will procure a large share of increased attention to the subject of inhalation: from many medical friends I have received the most agreeable assurances of their approbation of it. Unfortunately the larger number of cases of this melancholy disease, tubercular phthisis pulmonalis, admit only of palliative relief; but even in those the inhalation is not only admissible, but very useful in alleviating the symptoms.

A medical gentleman, in the last stage of the disease, came to me, from a considerable distance, in the hope of procuring some relief to his sufferings, which were unusually great and complicated. He was fully aware that he had no probable chance of recovery, but much desired to try the inhalation of iodine and conium. He received such sensible relief from it, that he repeatedly declared he was fully repaid for all the exertions he had made in taking the journey, in the unexpected mitigation of the cough, expectoration, breathing, and general feelings of the chest.

I should too much lengthen this article were I to advance all the arguments which are at my command, derived from experience, in favour of the combined method of treatment which it is my wish to advocate. I say *combined*, for I should regret to be considered as placing my confidence in inhalation alone.

Every one must admit the necessity of seeking for new remedies in phthisis pulmonalis. Is it not true that when once a case is pronounced to be consumptive, the sentence of death is more than half uttered? Urged alike by science and humanity, let us increase our exertions to overcome any particular disease, in proportion to the difficulties by which we are assailed.

As one very leading principle of practice, I apprehend that we should study to overcome the tubercular diathesis*,

* Lately I had an opportunity of seeing the character of hereditary tubercular disease strongly exemplified, in an infant that died in a state of the most extreme emaciation, at the age of four months. The lungs, liver, and spleen, were universally and very closely studded with tubercles: some minute, some large, some hard, others partly or wholly softened. Nor were the intestines free. The mother had died a month before, from the worst complication of pulmonary and intestinal disease I have ever seen;—the lungs broken up into cavities, ulcers in the intestines, and tubercles in their coats.

and to counteract the fresh formation of tubercles; for it very commonly happens that the relapse of the patient is owing to this cause. This comprehensive view can only be acted upon by using all the means, general and specific, which we think may be best calculated to effect a change in the circulating mass of blood. In addition to the medical treatment, all the jvantia are to be sought for in the general regimen, and all the ludentia are to be avoided; and without which care, no great disease can be cured.

In the employment of the inhalation, perseverance is necessary, and, in some instances, for many months. The object sought to be obtained is not merely palliative benefit—not merely a temporary impression on the morbid function—but the superseding of the diseased action by a healthy one, and the effecting some organic change.

Many other medicines may be employed usefully in the way of inhalation for tubercular irritation, chronic bronchitis, spasmodic asthma, and certain conditions of tracheal disease. These, with the exception of creosote and purified acetic acid, I have noticed in my publication. For the tubercular disease of the lungs, it is in the use of the iodine and conium alone that I place my hopes as a curative agent; I will not say confidence, for that never can be entirely felt in any known treatment of this most fatal of all diseases.

I conclude with expressing my hope that my professional brethren will make fair trial of the plan which I have recommended; and I shall be obliged by receiving communications from them, whether in favour of or against it, for truth is my object. I trust that none will condemn the practice without experience, for this would be prejudice and injustice.

SPINAL IRRITATION.

To the Editor of the Medical Gazette.

SIR,

IF you should think the following case and observations worthy of insertion in your valuable journal, at your own convenience, by so doing you will oblige,

Yours respectfully, &c.

GEO. C. WATSON, M.D.

Surgeon, &c.

Edge Hill, Liverpool,
Jan. 29, 1835.

Some time ago, I read in your pages a critique upon a treatise published by a Dr. Griffith, I think, containing some observations on morbid irritation of the spinal cord, along with a series of cases. The tenor of his work, as far as I recollect, was to point out that in many cases, where the expression of pain, and other symptoms of morbid nervous affection, were confined to the part, or organ, supplied by nervous filaments from the spinal centre, the true seat of the disease would frequently be found to be in the *origin* of those nervous trunks in the medulla spinalis; and that from having observed this difference in locality between the disease and its expression by pain, &c. at some distance from the real irritation, he inferred that accuracy of observation, and careful interrogation, alone could point out the true situation where local remedies should be applied. At the time, I read your extracts from Dr. G.'s work with much interest, and felt convinced of the value of his observations and suggestions, and resolved to follow up his plan of interrogating the nervous system the earliest opportunities that might present themselves. It was not long before such did occur, and in several cases I reaped the benefit of Dr. G.'s remarks; through attention to them being enabled to direct my treatment to the true seat of the disease, which issued in more or less complete success, where the old routine plan effected nothing at all; but the following case, which I have to lay briefly before you, was the most unequivocal instance which has come under my notice of the *practical* utility of Dr. G.'s hints, and which, therefore, I hope, will not be occupying your columns uselessly.

W. H., æt. 45, phlegmatic temperament, came to me complaining of acute pain running down the outer side of the left fibula, and thence shooting down to the sole of the foot. The ankle-joint, and indeed the whole limb, appeared quite free from any tumefaction or other evidence of inflammation; nor in the line of the pain, which he traced with his finger, was any thing to be perceived save only increase of it on pressure. The pain did not appear to be at all of a rheumatic character. He did not ascribe the affection to any immediate exciting cause. The pain was

sufficient then to cause a limp in his walk. He did not complain of any other part, and his general health, though never what could be called "rude," yet did not seem particularly deranged at this time. Some months previously he had been under my care with an obstinate catarrh, which yielded through perseverance in the usual remedies.

I contented myself with ordering some purgative medicine, and a lotion containing liq. plumbi diacetis. After some days he returned to me, complaining still of the same constant pain. I ordered him to apply six leeches to the leg, and prescribed a mixture containing diaphoretic doses of the potassio tart. of antimony. The leeches afforded but little relief, and for a short time. No other inflammatory symptom having set in, which could contraindicate the trial of the opposite plan, I ventured to prescribe a tonic mixture, consisting of di-sulph. quinine, dilute sulph. acid, and inf. rosæ, thus exhibiting about five grains of the quinine daily, whilst he discontinued all other remedies except a purgative. After giving what I thought a fair trial to the quinine, I found that the pain still persisted, and that he especially had to complain of it when the leg was in a depending posture, so that he was generally obliged to pass the day with it supported on a chair, and was frequently obliged to desist from his occupation, at which he still continued to employ himself, as well as he could. At this time I told him to try light warm bread poultices for a few days over the painful part of the leg, and requested a friend of mine, a surgeon of much intelligence and many years' experience, to examine the leg with me; on doing which carefully, we discovered a small *cicatrix* which appeared to be the chief focus of the pain. His general health seemed to suffer more now, in the shape of a *malaise*, his tongue getting a whitish fur; his bowels had been kept open all along; he informed us that the leg felt a little colder than the other. All we could further detect was a slight puffiness of the part around the *cicatrix*, and we were induced to recommend the practising of a small incision for about an inch and a half long, which we thought might relieve him, either by discharging any little pus which might be formed, or by liberating the *cicatrix*;

at all events we determined (with the consent of the patient) in case the neuralgic character of the pain should be shown *negatively* by the absence of any consequences of active inflammation, to establish counter-irritation in the wound by the stimulating dressings. Accordingly the incision was made, and the alternative of counter-irritation had recourse to, exhibiting, at the same time, ses. carb. of iron and soda, along with small quantities of ginger: the acute form of pain around the *cicatrix* rather abated after this, but the limb grew in some degree numb, and the singular sensation of modified pain and numbness began to run upwards towards the sciatic nerve, and followed that large trunk to its emergence out of the sciatic notch. This new feature in the case most annoyingly added difficulty of sitting to that of walking. It was not until we could acquire a larger insight into the case that we could discover the true nature of the disease, and from the tracing out of such, direct our remedies to the right quarter. The remarks of Dr. G., which I had only just then become acquainted with, served to draw our attention and direct our interrogations to the spinal column, for the probable origin of the pain; we, therefore, carefully examined the spine, by exerting uniform and steady pressure from the dorsal region downwards, with a wet towel: no indications of tenderness were perceived till reaching the last lumbar vertebra, when we observed the patient to wince, and complain of decided tenderness at its junction with the sacrum. We were now, for the first time, made acquainted with the real locality of the irritation, and felt some surprise that the patient had never spontaneously referred us to this spot, by expression of pain or uneasiness there. Of course it now became a straight-forward case, and the setting up of an active counter-irritation over this region soon ended in procuring almost simultaneously the total relief of both the spinal irritation and the distant pain.

In concluding this case, I would venture, Mr. Editor, to remark, that it seems probable to me that at first the irritation might have been confined to the minute nervous filaments which supply the integuments around the *cicatrix*, and that it extended itself very early to

the cauda equina, thence to be reflected with additional intensity towards the leg; or it is possible that it was an instance of distant pain in a lower extremity, particularly felt in the most recent deposit of soft parts, not alone the only prominent symptom of irritation of the lumbar portion of the spinal column, but the first expression of sympathy with that affection. This view of the case will recal the mind of the reader to the well-known analogous fact of cicatrices being the first to become affected, getting tender, and taking on an inflammatory disorganization, when the constitution is suffering much, from whatever cause. The cicatrix was from a slight wound of many years' standing, and it never troubled him before. He has not had any return of the attack, which was more than a year since. Whichever view of the case be the true solution of it, I feel that thanks are due to the accurate remarks of Dr. Griffith for whatever benefit the patient has obtained, or instruction we have derived.

P.S.—I recollect some observations resembling those of Dr. G., in Sir Benjamin Brodie's valuable clinical lectures on Hysterical Affections of the Knee-joint, as reported in your Gazette.

ON RUPTURE OF THE HEART, AND ON HÆMORRHAGE INTO THE PERICARDIUM,

WITHOUT RUPTURE OF THE HEART OR
GREAT VESSELS:

With Cases.

By JOHN THURNAM.

Our knowledge of rupture of the heart is not so complete but that the publication of fresh cases, and especially of such as bear upon the previous symptoms, is still desirable. For the particulars of the first of the following cases I am chiefly indebted to my friend, Mr. Thomas Davies, of Chester.

CASE I.—*Spontaneous Rupture of the Right Auricle and Ventricle of the Heart, with extraordinary and general softening of that organ.*

Mr. —, aged 60, a surgeon, formerly in extensive practice in this me-

ropolis, but who had resided for many years in the country, was of active and temperate habits, and had enjoyed remarkably good health until within about four years of his death. At this period he had severe, and often-repeated, attacks of gall-stones, the passage of which was accompanied by hæmorrhage from the stomach; but, after having suffered in this way for some time, his health was gradually re-established. For three years, or longer, after this, with the exception of an occasionally disordered state of the bowels, and of rheumatic pains in the shoulder and hip joints, his health appeared very good. He had never been the subject of acute rheumatism.

During the month of May, 1837, he frequently complained of wandering pains in the chest, with oppression, and a sense of hurry and uneasiness when walking more quickly than usual, but these symptoms were not so urgent as to attract much of his own or of his friends' attention, nor to prevent his leaving home, on business, on the 29th of May. Having proceeded a considerable distance by coach, he walked ten miles further, partly in the rain, and clothed in a Mackintosh cloak, by which he was much overheated. On his arrival, he complained of a pain in the left shoulder and in the right side, which he attributed to an attack of gall-stones. After taking some slight refreshment, he retired to bed much fatigued. The next morning he rose early, expressed himself better, and, after having breakfasted, he walked about two and a half miles, to the house of a friend; but on the road he again suffered from such severe pain in the right side and left shoulder, as to have much difficulty in proceeding. His friend observed that he appeared dull, dispirited, and disposed to sleep, and requested him to recline on a sofa, and in this way he slept for about an hour. Upon awaking he felt refreshed, and walked out until three in the afternoon. At this time he dined, eating and drinking moderately; but, upon taking a cup of tea in the evening, he vomited his food along with much bile. He now complained of increased pain in the upper half of the left humerus, which extended backwards to the scapula; this pain, he said, was different to any which he had felt before. He took a dose of laudanum, and

retired early to bed; but when visited by his friend in the course of the night, he was found sleepless and uneasy; the pulse was full and rather slow; the countenance somewhat anxious; the surface of the body warm. He got up several times during the night; at five A.M. he vomited a little bile; at six A.M., upon being visited, he said that he had been altogether sleepless, but that he apprehended no immediate danger, and that he hoped he was merely suffering from a transient attack of gall-stones. He consented to try the effect of a warm bath, and while his friend had left the room, to see it prepared, he expired without a groan.

Dissection.—A medico-legal investigation being thought desirable, the thorax was opened at the place of death, and the pericardium, having been punctured, was found to be filled with blood. The body, in the meantime, having been removed home, the examination was resumed three days after death.

The body was found to be muscular, and furnished with a considerable quantity of fat; not the least decomposition having taken place. Upon removing the fluid blood with which the pericardium was filled, a clot, about the size of a filbert, which required a slight degree of force for its removal, was found attached to the surface of the heart, in the furrow between the right auricle and ventricle. A rupture, which had been closed by the coagulum, was now made visible, and was found to communicate with the cavities both of the auricle and ventricle. The whole of the muscular substance of the heart was so extraordinarily softened, as to give way, in every direction, to the least pressure of the finger; its texture was of a dark brown colour, and resembled the pulp of which common brown paper is made. The softening and separation of the muscular fibres had proceeded to such an extent, that the rupture might be said to have been simply one of the pericardium, for by this merely were the softened fibres held together. The heart was neither enlarged nor thickened, nor otherwise diseased, and the state of the valves and of the aorta was normal. No other disease was observed in the cavity of the thorax. Upon examining the liver, it was found that there was considerable disease of the biliary ducts and gall-blad-

der; the latter being remarkably contracted, and containing a few small biliary calculi.

This case would appear to have considerable analogy to four instances of rupture of the heart which have been reported by M. Bland,* and which were accompanied by a "gelatiniform degeneration." These cases all occurred in males, and in persons of the ages of 58, 66, 84, and 86, respectively; from the latter of which circumstances, coupled with the peculiar alteration of the muscular substance, the author concludes that they are examples of a peculiar form of rupture of the heart, to which he gives the name of "senile rupture." Two cases of rupture of the heart, in males, of the ages of 60 and 71 years respectively, are also recorded by M. Taxil St. Vincent†, and these appear, at all events one of them, to have been of the same nature as those of M. Bland. Should further inquiry confirm the view of this form of rupture being confined to the aged, and persons advanced in life, there can be little doubt that the case now narrated must be considered as of the same description.

In an important memoir on rupture of the heart, by M. Pigeaux‡, in which he gives the results of an analysis of fifty-four cases, I find that in thirteen the lesion was accompanied by general softening of the organ.

There can, I think, be little question that, in the case which I have related, the wandering pains in the chest, the oppression, uneasiness, and sense of hurry upon exertion, which were experienced for about a month before the fatal termination, formed a class of symptoms which are to be attributed to a gradually advancing softening of the heart. And I should presume that it will be equally admitted, that the fatigue and exertion which the patient underwent on the day of his journey, were the determining causes of the rupture, the occurrence of which was announced by the peculiar pain in the left shoulder, the mental dejection, and slightly anxious aspect, by the sleeplessness, vomiting, and slow pulse. Why the pain was also experienced in the right

* Journ. Gen. de Med. t. 72, p. 231.

† Journ. Gen. de Med. t. 73.

‡ Journ. Hebdom. t. 8, 1832, p. 407.

side I know not how we are to explain, unless we may be allowed to suppose that it depended upon some sympathetic disturbance of the liver, to which it was exposed from a previously diseased condition. The case, at all events, is valuable, from its throwing light upon the symptoms of softening of the heart, which it must be observed differ materially from those assigned to this lesion by Laennec and Dr. Hope; and were of a more local character, or more distinctly referrible to the central organ of the circulation.

There can be little or no question that the softening of the muscular tissue of the heart, which had in this case advanced to so great an extent, was altogether unconnected with any inflammatory affection: that it was, in fact, a primary lesion dependent upon some peculiar and inexplicable condition of the organism generally; upon a cachexy, as this would have been termed by the nosologists of a preceding generation.

CASE II.—Rupture of the right Auricle and left Ventricle of the Heart from external violence, without penetrating wound—Fracture of the Ribs and Sternum.

A stout man, porter to a cyder merchant, aged about 50, fell off his cart, the wheel of which passed obliquely over the left side of his chest. He was picked up in the street, and brought directly to the Westminster Hospital, Oct. 20th, 1837. When I saw him there was no pulse at the wrist nor at the heart, and the pupils were insensible; in fact, there was no sign of vitality, unless a strange semblance of life in his countenance could be so considered. The only external marks of injury that I detected were two or three abrasions of the skin on the left side of the front of the chest. I endeavoured to restore the action of the heart, by performing artificial respiration, but without effect. I observed at the time to one of the pupils that the heart was probably ruptured.

Dissection.—Upon examining the body I found that the sixth and seventh left ribs were fractured about 2½ inches from the cartilages, and that the neck of the eighth rib was broken very obliquely. The sternum was broken transversely in its lower third, but there was no displacement. There was serum in both

sides of the chest; and under the left pleura there was a considerable ecchymosis, and also in the muscular fibres of the diaphragm; but the lungs were not injured. The pericardium was not lacerated; it contained several ounces of coagulated blood and serum. There was a rupture of the right auricle, more than two inch & in length, just above the attachment of the tricuspid valve, extending from the appendage of the auricle almost to the inferior vena cava. There was a slight superficial rupture of the interventricular septum, at its highest part. There was likewise a rupture of the apex of the heart, which, externally, was about an inch in length, and extended into the substance of the septum, but which admitted only of the passage of a bougie into the left ventricle.

The spleen was extensively lacerated and bruised, and had given rise to a hemorrhage into the peritoneal cavity. The stomach and intestines were distended with half-digested food, which exhaled a strong odour of beer. With the exception of a mottled condition of the kidneys, there was no deviation from healthy structure detected in any of the viscera.

As there was no injury of the pericardium, and as no displaced splinters of bone were detected, the ruptures of the heart in this instance, which occurred in two of its thinnest points, were in all probability the effect of the inordinate contractions of this organ, excited by the compression of the thorax under the wheel.

The important bearing of a case of this description in a medico-legal point of view is obvious, and it may be observed that this point has been ably illustrated by Mr. Watson, of Edinburgh, in a recent work* of his, in which he adduces some analogous cases. Cases in which the rupture has originated in the same manner that it did in this, are, I should suppose, far from uncommon. Thus M. Bouillaud† reports four cases (137, 138, 139, and 140), in which a rupture of the heart was caused by severe violence applied to the præcordial region. In the first of these, the chest had been crushed by the wheel of a carriage, as in the case which I have

* Medico-legal Treatise on Homicide, 1838, see Brit. and For. Med. Review, vol. 5.

† Traité Clinique des Maladies de Cœur, t. ii. p. 504.

related, but neither in this nor in any of the other cases does it appear that the ribs or sternum were fractured. There is, however, a case reported by Mr. G. Bennett, in the *MEDICAL GAZETTE*,* about five years since, which is almost precisely parallel to the one now related. The rupture was seated in the left auricle and pulmonary vein, and occurred in consequence of a crash against the wall by the wheel of some vehicle, I think a cart. In this case the sternum and some of the ribs were fractured, but nevertheless the author, taking the same view as I have done, looked upon the rupture of the heart as, in one sense, spontaneous, and only as being excited, not determined by the external violence.

Injuries of a similar character, however, sometimes occasion a solution of continuity of the heart in a different way. The sternum, or ribs, or both, being fractured, a splinter of one of these is displaced, and leads to a true laceration of some part of the heart. A good example of this description is cited by Bonillaud† from M. Choisy. These two kinds of injury form connecting links between the altogether spontaneous rupture, such as occurred in the first case given in this paper, and penetrating wounds of the chest, involving the heart. There are, in fact, two forms of *rupture of the heart*; *first*, the purely spontaneous, or those the causes of which are seated in the organism itself; and, *secondly*, those the causes of which are external to the body, but not attended by a direct mechanical injury of the organ. On the other hand, there are also two forms of *wounds of the heart*; *first*, lacerated or contused wounds, occasioned by detached and depressed portions of the osseous system; and, *secondly*, penetrating wounds which are simple extensions of similar forms of injury of the thoracic parietes. This classification is a slight modification of that of M. Bouillaud.

Cases are on record, and Dr. Baillie‡ has particularly adverted to them, in which blood, to a large amount, has been found in the pericardium, but in which, after the most diligent search, no rupture, either in the heart itself or any of its vessels, could be detected. Cases

of this description have puzzled their observers, and have given rise to various surmises with the view of accounting for them. I am inclined to believe that they may be most, if not all of them, explained upon one of two suppositions.

It has been well shewn by Laennec, that the pleura, when under the influence of inflammation, is often the seat of other than the usual products of this morbid action, serum and lymph; in fact, that these may contain a considerable proportion of the blood-globules themselves, so that in many cases the pleuritic effusion in general appearance closely resembles the blood itself. As is well known, to cases of this description the distinguished pathologist alluded to gave the appropriate name of *hæmorrhagic pleuritis*. Cruveilhier and others have since shewn that we may also have cases of *hæmorrhagic pericarditis*, and there can be little doubt that the same form of inflammation occurs, more or less frequently, in all the serous membranes. It has been my lot, on two distinct occasions (one of which occurred some years ago, when I was studying in another institution) to be present at dissections in which hæmorrhagic pleurisy was overlooked, and its undoubted characters mistaken, in the one case, for the rupture of a blood-vessel, which was most diligently but unsuccessfully sought for, and in the other, for the effects of a scorbutic diathesis, which had permitted of an exosmose from the smaller vessels. On the ground, then, of analogy alone, I have but little doubt that many of the cases alluded to by Dr. Baillie were really instances of hæmorrhagic pericarditis.

In other cases I think it not improbable that the blood found in the pericardium, without apparent rupture of the heart, or of the pericardial portion of the great vessels, has originated in the rupture of one or more of the large veins or other vessels at the root of the neck, the extravasated blood of which having followed the course of the superior vena cava and arch of the aorta, has made its way into the pericardium, by simply raising and rupturing this membrane where it is reflected over the roots of the large vessels. I have, at the least, satisfied myself that this was the case in the instance which I will now relate, which at first sight, and indeed, until after a very scrupulous

* For 1832, vol. ix. p. 582.

† Op. cit. t. ii. p. 499.

‡ Morbid Anatomy, 2nd edit. p. 34.

examination, baffled us exceedingly in determining the source of the blood.

CASE III.—Blood in the Pericardium, without rupture of the Heart or pericardial portions of the vessels.—Fracture of the Cranium, Ribs, and Clavicles.

William Shingleton, aged 42, was brought to the Westminster Hospital, Dec. 2, 1837, with very extensive injuries of the bones of the head, ribs, and clavicles; he died almost immediately after being seen by Mr. Bury Dacent, the house-surgeon. It appeared that during the dense fog which prevailed at the time, he had been knocked down by a runaway horse and chaise, as he was entering St. James's Park, through Story's gate.

Inspection of the body.—Upon the calvarium being removed, the anterior and posterior portions of the base of the skull were found to be moveable upon each other, as if by an irregular kind of hinge-joint, from a fracture having traversed completely the middle cerebral fossæ. The left temporal bone was more injured than the right, the fracture on this side having implicated the internal organ of hearing, and petrous portion of the bone. Both the clavicles were fractured at about their sternal thirds, and two or three of the left upper ribs were also broken.

Upon opening the pericardium it was found to contain a considerable quantity of black fluid blood. Both the right and left cavities of the heart contained semi-fluid black grumous blood, but there was no rupture in any part of this organ, nor in the large vessels contained within the pericardium. In the upper portion of the mediastinum, and reaching to the root of the neck, there was found a considerable layer of coagulated black blood surrounding the bifurcation of the trachea, the œsophagus, superior vena cava, and ascending aorta; and upon tracing this ecchymosis carefully downwards, it was seen to terminate on the surface of the cava within the pericardium at its highest part, and at the same point there was found to be an irregular laceration of the fibres forming this membrane, through which the blood had evidently gained access into its cavity. Although I did not succeed in demonstrating it, yet I have little doubt that the extravasation had originated in the rupture of some of the large veins in

the front of the trachea above the sternum, and that this rupture had been occasioned by the displaced portions of the fractured clavicles.

Westminster Hospital,
Jan. 29, 1838.

CASE OF CÆSAREAN OPERATION.

To the Editor of the Medical Gazette.

SIR,

SHOULD the inclosed case appear to you of sufficient interest to merit a place in the columns of your journal, I will feel obliged by your giving it insertion.

I am, sir,

Your obedient servant,
J. WARD.

Sunderland, Dec.* 1837.

On the 18th of October, at half-past nine A.M., I was requested by Mr. F. D. Thompson to visit E. Hull, an unmarried female, aged 25, very much deformed in the spine and pelvis, with very short crooked thighs and legs. She had been in labour all the previous day, and attended during the night by Mr. F. D. Thompson. On examination per vaginam, duplicature of the funis was protruding through the os uteri to the extent of six inches. The occiput could be felt, but, from the great deformity, it was with some difficulty it could be reached by the finger; the promontory of the sacrum projected so far forward that there was only an inch between it and the pelvis; and the spine curved forward over the small aperture, so as to shut in the pelvis above.

At my suggestion a consultation was requested; and accordingly Mr. Gregory, Mr. Dixon, and Mr. Saville, saw the patient, when it was decided on performing the Cæsarean section, as the only possible way of delivering her. There was no pulsation to be felt in the funis; and likewise, from other circumstances, it was concluded the child was dead, which was proved to be the case.

At half-past 12 at noon the patient was laid upon a table; an incision was made in the direction and over the linea alba, commencing at the umbilicus, and

* The manuscript was accidentally mislaid.—
ED. GAZ.

continued to the extent of six inches; the parietes were carefully dissected until an opening was made through the peritoneum; two fingers were introduced as a guide, and the remainder of the opening down to the pubes accomplished. A second opening being made through the uterus, the placenta was brought to view, through which the hand was passed and the child brought away, followed by the membranes, the uterus contracting immediately on being relieved of its contents.

The parts were sponged from the little hæmorrhage which took place during the operation, and the extensive incision closed by the twisted suture. An anodyne was administered, and the patient, being placed in bed, remained in a tranquil state through the course of the afternoon and evening. Was visited at 6 P.M., when the pulse was 100. A saline mixture was then prescribed, and she was left in the care of an assistant through this and the subsequent nights during her illness.

Oct. 19th, 10 A.M.—Has had a tranquil night, with three hours' sleep; passed some coagula by the vagina. Pulse 120; countenance tranquil; skin moist; tongue clean; very slight thirst; complains of pain at the scrobiculus cordis; slight tenderness over the abdomen. Has passed no urine since the operation; the catheter was introduced, and ℥ij. of water drawn off; no evacuation from the bowels; lochial discharge continues. She remained during this day very tranquil till the evening.

6 P.M.—Pulse 130; tongue dry; countenance calm; bowels slightly evacuated by enema; passed urine freely; more coagula per vaginam; abdomen tympanitic, with tenderness. The bandages were removed, as causing uneasiness by their pressure.

Habeat Hydrargyri Chloridi, gr. x.

9 P.M.—Much in the same state; twelve leeches were applied to each side of the abdomen; enema repeated. Had two grains of opium, with three grains of calomel, which was repeated at one o'clock; a large poultice applied over the whole surface of the abdomen, and continued through the night.

20th, 10 A.M.—Has had five hours' sleep during the night; bowels relieved, and passed urine freely. Pulse 100; countenance tranquil; abdomen less tympanitic; tenderness much relieved; has thirst, with brown tongue. Bowels

continue open; skin moist; lochial discharge continues, and expresses herself considerably relieved.

6 P.M.—Pulse 100; perfectly tranquil; bowels acted freely; passed urine; lochial discharge continues; slight perspiration; gums a little affected; tympanitic feeling less; bears pressure better.

Pills of calomel and opium, and saline medicines continued.

21st, 10 A.M.—Has passed a comfortable night, with three hours' tranquil sleep, and expresses herself much refreshed. Bladder continues to perform its functions naturally; pulse 98; less tenderness of the abdomen; feels easy; no evacuation from the bowels.

To take a saline purgative.

6 P.M.—Pulse 126; skin moist; slight hiccup; no operation from the purgative.

Habeat Ol. Ricini, ℥j., and to have an enema. The pills of calomel and opium after the bowels have acted.

22d, 10 A.M.—The bowels have acted freely through the night, and has had some sleep. Pulse 120; irritable and restless; skin moist; tongue dry.

Febrifuge mixture ordered.

6 P.M.—Pulse 130; skin moist; tenderness of the abdomen; frequent eructations of flatus; bowels freely opened; tongue moist and furred; lochial discharge continues.

Repeat pills and mixture.

23d, 10 A.M.—Has had a restless night; pulse 140; eructations and hiccup.

Ordered a mixture containing sesquicarbonate of ammonia, with wine negus.

Mid-day.—Continues to get worse; stimulants freely administered, from which time she continued to sink, and died at half-past eleven at night.

Post-mortem examination on the 24th October.—The external wound was partly healed by the first intention; there were traces of considerable peritoneal inflammation. The uterus was of the usual size; the opening was closed, but there was no appearance of adhesion. From the presence of the friends, and the objections they made, we could not proceed further with the examination.

It must be remarked that, up to the morning of the 22d, it will appear by this report that the patient was doing

well; but on that day a drunken and uproarious quarrel took place between the brother of the deceased and the father of the child, in the adjoining room, from which time she appeared (from the agitation caused by the quarrel) to get into a state of great nervous irritability; and had it not been from the bad effects evidently produced by the above-mentioned circumstance, the patient in all probability would have done well.

I have omitted to mention that, after the operation, her diet was of a bland nature, consisting of sago and arrow-root with milk, and chicken-broth occasionally, all of which she partook of with a natural relish.

IRISH COLLEGE OF SURGEONS.

EDITORIAL NOTE.

[It will be observed that the subjoined letter, under almost identically the same signature, contradicts flatly and unequivocally almost every statement made in that of last week. Both have been authenticated to us by the names of the writers, and the author of that inserted this week (which we shall call No. 2) gives us permission to publish his name if we think proper. Under these circumstances we have withheld the second part of the communication of last week, and have determined to insert no more letters upon the subject to which they refer without the real signatures of the writers being attached to them, so that the responsibility as to the accuracy of the statements may rest exclusively with those who make them.—ED. GAZ.]

To the Editor of the Medical Gazette.

SIR,

I REGRET to perceive that you have been (no doubt unintentionally upon your part) led to insert, in your number of Saturday last, a letter, purporting to be from "A Member of the College of Surgeons in Ireland," in which the grossest and most calumnious untruths have been asserted with respect to the proceedings and views of that College. The letter itself being neither more nor less than a collection of direct falsehoods, I shall confine my reply to an enumeration and flat contradiction of a few of them, pledging myself to support my statements by reference to the minutes of the College and its committees.

Falsehood No. 1—States that the publication of an article headed "Irish College of Surgeons—Pharmacy Laws," and which appeared in your number of the 20th January, was not sanctioned by the College. This is absolutely untrue, the article in question having been agreed to and directed to be published by a large and open committee of the College, and the expense of its insertion in certain newspapers defrayed out of the common fund of the College.

Falsehood No. 2—States that "all the provisions which it is intended that the act of parliament shall contain, have not been made public." This is altogether false, as can be proved by the minutes of the committee, to which every member of the College has free access.

Falsehood No. 3—"The proposed change has not received due consideration or sufficient sanction from the College or the profession." In reply to this, I offer the following statements:—The proposed change has been under consideration since the commencement of the month of November last; the appointment of a committee to take measures for its accomplishment was, at that time, made in the regular way by motion in a full College, notice having been given at a previous meeting, and inserted in the printed summonses; a notice of the sittings of the committee was posted in the College-hall; a circular letter, explaining the objects in view, and asking for advice and private opinions respecting them, was addressed to every member and licentiate of the College throughout the United Kingdom—the postages of those letters were paid; statements of a similar nature were published in all the Dublin, and in many of the provincial papers; many hundred copies of those papers were purchased and forwarded by post to medical persons throughout Ireland, both in and out of the College; after all these measures for attaining publicity had been taken, and several weeks had elapsed, the College was called together by a printed summons expressly to receive the report of the committee. The report was unanimously agreed to at one of the largest meetings of the College known for years; and a second meeting was again called, by summons, at which a petition founded upon the report was also unanimously agreed to.

Equivocation No. 1.—"The College contains between 130 and 140 members,

and on no occasion has one-third of the number ever voted upon the question." The College does contain nearly the number of members stated, but of these there are altogether not more than 90 resident in Dublin, and of these again, more than one-third, in consequence of old age, indolence, &c., never attend the meetings, although they are upon every occasion invited to do so by printed summonses, stating the business to be transacted, and sent to the house of each member by the servants of the College.

Falsehood No. 4.—"The proportion of those (communications from members and licentiates resident in the country) favourable to the change, does not exceed a third of the entire." Ninety-seven or ninety-eight communications were received, of which two were adverse, one doubtful, and the remainder unreservedly favourable.

Falsehood No. 5.—"The licentiates of Dublin have actually protested against the measure." This is totally false! The licentiates, with very few exceptions, heartily concur in the proposed change. All of them advocate some change, but some of them think that before actually becoming *general practitioners* it would be advisable to ask the legislature to restrain the apothecaries to their counters and prohibit them from practising; the persons who adopt this opinion being nevertheless willing and anxious, in the event of such a plan being impracticable, to recur to the general practitioner system as an alternative.

Falsehood No. 6.—"They appear to me to be only a mask for another, as yet not avowed, design." I can only, on the part of the College, the committee, and myself, flatly deny this statement.

In addition, sir, I may be permitted to state that within the last three weeks a requisition was attempted to be got up for the purpose of calling the College together to reconsider its proceedings with regard to the matter in question, but although such a document only requires twelve names, these could not be procured, and the project fell to the ground. At the quarterly meeting, subsequently, held upon the first Monday of the present month, neither notice nor remark was offered upon the subject.

I trust that as you have been unwittingly led to circulate calumnious falsehoods respecting a public body, you

will permit as short a time as possible to elapse before giving equal publicity to this contradiction of them.

Your most obedient servant,
A MEMBER OF THE ROYAL COLLEGE OF
SURGEONS IN IRELAND.

Feb. 12, 1838.

MEDICAL GAZETTE.

Saturday, February 17, 1838.

"Licet omnibus, licet etiam mihi, dignitatem
Artis Medicæ tuæ: potestas modo veniendi in
publicum sit, decendi periculum non recuso."
CICERO.

THE UNIVERSITY OF LONDON.

TRULY, to borrow the language of brother Jonathan, the University of London "progresses, we guess." Ill-natured persons were disposed to marvel at its inactivity, and to insinuate that it had no character at all; that taunt, however, is no longer appropriate, for it has within the last few weeks established a reputation for discretion and consistency which cannot fail to endure—at least as long as the establishment itself. Seeing it was set forth in the charter that the University was instituted for the furtherance of "Religion," among other purposes, it was solemnly decreed, after much, and, as the result has shewn, very mature deliberation, that candidates for the honour of their degree should be examined on Church History, and on the New Testament in the original Greek. Nothing could possibly have been more judicious or discreet. The avowed purpose of the Institution was to make up for the exclusion practised at the old Universities, and to open a path to the honours of science alike to Jew and Gentile. Our readers, therefore, cannot fail at once to perceive the sound policy and discrimination which led to this sapient regulation, by which those of the Hebrew persuasion were to construe the Greek Testament.

But if there was wisdom in this decree—the first overt act of the University of London—how much more profound was the sagacity displayed in their second appearance before the public. This, which was made through the medium of a government paper, and doubtless on good authority, conveyed to the world the astounding fact that the decision allotted to had been reversed, and that no such test was to be required. Simple people might suppose that certain persons who were absent on the first voting had been present on the second, and thus led to a different result. But not so; for there is this unlucky addition to the announcement, namely, that on the second occasion just the reverse of what had before been passed by a majority was carried “almost unanimously.” There is something more in this than meets the eye—if philosophy could find it out. What could have wrought so sudden a change? whence came the new light which so quickly enabled the worthy Senators to see the error of their ways? Why simply this: a letter from Lord John Russell, to the purport that if *they* did not reverse the decree, *he* would; nothing more. Oh! there is nothing like an argument from the Home Office for settling a point of this kind—nothing like your modern improvements in political economy, for the promotion of liberty and independence. Formerly, the interference of a minister of the Crown with the regulations issued by a *Senatus Academicus* would have been thought arbitrary, and inimical to free agency; but now it is quite otherwise: the *Senatus* is free to discuss, the Secretary of State is free to decide, and so, most palpably, the amount of freedom is doubled. But perhaps we go too far; his Lordship does not decree, he does but suggest: speaking of the regulation passed by the

University, he just “hints a fault, and hesitates dislike”—but in the gentlest manner possible. He asks “whether it might not be possible to frame a rule which should leave it to the candidates for degrees to be examined in the Greek Testament and Church History *at their own express desire*?” which means, that if any candidate finds that he does not incur risk enough of being plucked on the ordinary examination, he may add to the danger by making its increased difficulty the subject of express solicitation. To be sure, this is a title different from making such examination a *sine qua non*, as had been previously decreed; but then, says his Lordship—and the argument was evidently not lost upon the Senate—it would be “most unfortunate” to make the University “an object of suspicion.” After which, he very pithily adds, “It would scarcely be less of a calamity were the authority of the Secretary of State to interpose a bar to regulations deemed essential by the senate!”—which we humbly submit, being interpreted, means, “Gentlemen Senators, you have made a most stupid and uncalled-for regulation, and if you do not vote black to be white (as some of your betters have offered to do for us in the House of Commons), and at your next meeting reverse all you did at the last, I—even I, by myself I—Lord John Russell, one of her Majesty’s Secretaries of State, will annihilate your decree with one stroke of my pen, and show you that the independence of your University consists in being always independence on the occupant of my present quarters in Downing-street.”

A hint so palpable was not to be misunderstood, and the senate accordingly voted as directed. Now we request it may be observed that we do not advocate the propriety of their first resolution; on the contrary, we think it was inex-

pedient, and at variance with the ostensible object for which the University was founded. To tell us, as some pretend to do, that reading the New Testament, in the original, was intended merely to test the candidate's knowledge of Greek, is absurd; for any one in the least acquainted with that language must be aware that it is quite unfitted for such a purpose. No; the simple truth is, that a majority of the senate thought it right to require some evidence as to the religious sentiments of candidates for their diploma; but their own opinions on the subject were not sufficiently decided to enable them to resist the *arguments* of Lord John Russell, backed by the "scarcely less of a calamity," which he so significantly hinted was to follow, in case they proved contumacious.

It really must be a very pleasant thing to belong to an University so constituted. Any one who is in a minority in the council chamber at Somerset House has but to make interest at the Home Office, and straightway the decision is open to be reversed. Another very pleasant part of the system is the mode to which it gives rise of filling up any vacancies which may occur. To suppose a case by way of illustration:—Mr. Warburton might be desirous of having a *protégé* of his own appointed: he is rather a troublesome customer in the "House," and the required nomination at once takes place. The astonished "senators" find they are thus provided with a colleague whose appointment cannot fail to be most gratifying, viewed, as it naturally comes to be, as indicative of the measure the noble secretary has taken of themselves--their rank, place, and position in their profession and in society.

But to carry the case a little further; still hypothetically, however, and by way of illustration. Suppose that a large proportion of the senate should think so

highly of themselves, or so lowly of their new associate, as to address a remonstrance to the Secretary for the Home Department, how gratifying, how satisfactory, how atoning it would be, to be told—"I really knew nothing about him; I made the appointment at the solicitation of his friend; but the matter shall be investigated." That is to say, the appointment is made first, and the inquiry about its propriety is to follow! But softly, gentlemen. Here is a royal charter, with the Queen's sign manual attached to it; and where and what, we beg to inquire, is the power, which can erase one letter which it contains? That the party nominated was the best fitted that could possibly have been found, we have not ventured to assume; we do but assert that if once appointed, nothing but his own resignation can displace him; and that, under such circumstances, to resign would be perfectly suicidal of his reputation. If any such case as we have supposed should actually occur, our readers will, we think, admit that, taken in conjunction with what we alluded to in the former part of the present article, it would fully warrant the high encomiums we have ventured to bestow on the foresight, wisdom, and consistency, which rule over the destinies of the University of London.

A RESURRECTIONIST PHYSICIAN.

A curious incident has lately taken place in France. A Dr. Belhas is asserted to succeed in the radical cure of hernia by obliterating the neck of the sac with some animal substance capable of being absorbed; he has cured several of the patients of Dr. Féron, a physician at Theuville-aux-Maillots, and returns to Normandy every year to renew his operations. Satisfactory as the operation had always been, the two friends still wished to know what was the exact physical change effected in the diseased part; but from their uniform success they had never enjoyed the opportunity. At last one presented itself. A man

named Cottard, who had been operated upon two years before by Dr. Belmas, died of an inflammation of the brain and stomach. Dr. Féron was desirous to have a post-mortem examination, and was well aware that it was requisite to obtain permission; but *where* was this to be got? To try to get it from the family was useless, so unpopular is dissection in that part of the country. He might have asked permission of the Mayor, a man of sense; but, unluckily, he was absent. It might have been granted by the Attorney-General; but the distance was great, the event of taking this step was uncertain, and time pressed; what was to be done?

What M. Féron *did* do is matter of history, and inscribed on the tablets of a court of record. At the favourite hour of unquiet spirits and eager dissectors, he betook himself to the church-yard, dug up Cottard's grave, cut out the region he wanted with his scalpel, and, having replaced matters as well as he could, reached home in safety.

The next day it was perceived that the ground had been stirred; the legal authorities visited the spot, the grave of Cottard was again opened, and the coffin exposed to public view. Dr. Féron being informed of what was going on, hastened to the spot, told what he had done, and begged them to put an end to the scandal by restoring the remains of Cottard to the earth. In spite, however, of the frankness of this declaration, and the purity of his intentions, legal proceedings went on, and he soon heard himself condemned by the Court at Ivetot to a week's imprisonment. Dr. Féron was surprised at the severity of the sentence, and appealed to the Royal Court at Rouen.

M. Grainville, his counsel, showed with the utmost clearness that a fine would be a sufficient expiation of the misdemeanor charged upon his client, and that it would be an unheard-of thing to imprison him for an action which should be looked upon only as the overflowing of his zeal for science. He seemed to carry the Court with him, when the advocate-general rose to support the judgment of the Court at Ivetot. Our reverence for the grave, he said, ought to predominate over the fanaticism of science; and he was foolish enough to add, that the structure of quadrupeds, and that of man, are sufficiently similar to enable us to dispense

with the dissection of human bodies. In fact, he asked, why should we be more indulgent in France towards those who perform clandestine post-mortem examinations, than they are in England to resurrection-men?

The judgment of the Court below was confirmed.

The French journal from which we borrow this account* is very angry with the advocate-general for saying that we may dissect quadrupeds instead of men; and this is really a *bêtise*, though the learned counsel would probably defend it by asserting that he knew whom he had to deal with, and that it was a good *argumentum ad curiam*. The Gazette des Hôpitaux is also much displeased with his comparing a zealous and enlightened physician to English resurrection-men, eager for gain alone. But when our contemporary talks of the affair as a *scandale amené par la seule ignorance*, we cannot quite agree with him, and must confess that science had a hand in the matter too. In other words, we feel ourselves unable to throw the whole blame on the lawyers, by considering M. Féron as purely passive; his excess of zeal at night, and excess of frankness in the morning, must surely pass for something. In the discussion of this delicate subject, the interests of common sense seem in danger of suffering from the violence of two opposite parties. The one would have us dissect animals instead of men; the other, equally unreasonable, would have us pursue dissection with such ardour as to forget the very purpose which makes anatomy and medicine valuable—namely, the diminution of human suffering; for we must take into account the pain endured by these old-fashioned villagers, on finding their sole practitioner converted into an unlicensed exhumator, just as we do their unmitigated sufferings from *gastrites* and *enterites* during the time that Dr. Féron is shut up. It appears that his seclusion for a week deprives several communes of their only practitioner for the same period. Hence we learn that France is not quite so overrun with medical men as this country; for in England it would be difficult to find a village which was not within a moderate distance of two or three practitioners.

* The Gazette des Hôpitaux of February 3d.

HUNTERIAN ORATION.

THE annual oration was delivered on Thursday by B. Travers, Esq. It consisted principally of a history of surgical science, pointing out especially the evident connexion in rapidity of progress which had existed between it and anatomy. Among several interesting facts, he mentioned the origin of the barber-surgeons, in the necessity under which the priests, who had previously been the only practitioners, were placed, of employing their servants as barbers, in consequence of a papal edict strictly ordering the observance of the tonsure. The servants, by their frequent intercourse with the priests, who then were the only possessors of any kind of knowledge, obtained some little learning, and as their masters were confined within the monastic walls, were employed by them to visit their patients at their houses, and to perform the minor operations which they were themselves forbidden to do. The pupils soon became themselves masters of their art, and went on to such an extent, that they obtained reputation in this and many continental kingdoms, and were here incorporated, and remained under legislative sanction till 1745.

The oration was delivered with a remarkable degree of clearness, and was well received by a numerous audience.

TRIBUTE TO MR. EARLE.

ON Saturday last a meeting was held at the Freemasons' Tavern, of the friends and pupils of the late Henry Earle, Esq., to consider the propriety of entering into a subscription for the purpose of recording his talents and many estimable qualities. Mr. Sergeant Atcherley took the chair, and, with Dr. Latham and Mr. Skey, addressed the meeting, which was fully attended. Before it broke up upwards of 120 guineas were subscribed by those present. We understand it is intended to place a bust of this lamented gentleman in some part of St. Bartholomew's Hospital, or the school attached to it.

THE BRIDGEWATER UNION.

WE beg to direct the attention of our readers to an important pamphlet entitled "Facts connected with the Medical Relief of the Poor in the Bridgewater Union," the second edition of which is now ready, and may be had as specified in the advertisement on our wrapper.

ON ANIMAL MAGNETISM.

No. I.

WE have given our readers an opportunity of acquainting themselves with the doctrines of animal magnetism, as set forth by M. Du Potet, the chief promulgator of that art in England, and with its practice, as exercised by M. Berna, its champion in Paris. In our number for Sept. 16th, 1837, we briefly stated our opinions on the subject; we shall now enter on the principal points of the evidence on which we had formed them. That we shall do this with impartiality we have given a sufficient guarantee, by having, in the interspace, afforded to the supporters of what we still believe to be erroneous doctrines, an opportunity of appearing before a large portion of that profession which is alone capable of judging correctly in this matter.

We will first consider the theories of animal magnetism that have been proposed by its chief supporters. We are aware that some of the modern practitioners allow that none of them is sufficient to explain the phenomena; but it is of importance that their absurdity should be seen, not merely because they exhibit the real character of their authors, but because the language of the theoretical explanations being adopted by all who have written upon the subject, gives an appearance of mystery and marvel to their facts, which mislead the unwary reader, if not the author himself. Stripped of such terms as *rapport*, *crisis*, *exterior* and *interior prevision*, *clairvoyance*, &c. many of their most rare histories will be recognized as facts of frequent occurrence; and it will be seen that the very existence of the art has mainly depended on the plumage which it has borrowed from the pompous phraseology of sciences.

Mesmer's perfected theory was as follows*:—There is a mutual influence between the heavenly bodies, the earth, and inanimate objects, by means of a subtle fluid universally existing, and filling all space, capable of receiving, propagating, and communicating, all the impressions of motion by mechanical laws at present unknown, and producing alternating effects, a flux and

* Mémoire sur la découverte du Magnétisme Animal, 1779.

reflux. The properties of matter, and of organized bodies, depend on this operation. This agent affects immediately the substance of the nerves into which it insinuates itself. The human body especially presents properties analogous to those of the magnet; it has opposite poles, which may be reversed, destroyed, or strengthened; it presents even the phenomena of inclination. The magnetic power operates at considerable distances, and without the help of any intermediate body. Glass reflects and increases it, like light; sound communicates, propagates, and increases it; it may be accumulated, concentrated, and transported. There are (though very rarely) animate bodies which have properties so opposed to it, that their mere presence destroys all the effects of the magnetism in other bodies. (We need not say, that this was the grand loophole-clause of the theory and unsuccessful practice of the art.) This opposite virtue, like the real and positive one, penetrates all bodies, may be reflected, accumulated, &c. The common magnet may be animally magnetized, or receive the opposite property, without exhibiting any change in its properties as a mineral magnet, attracting iron, &c.; and when it, or any similar agents, act in diseases, it is only by means of the animal magnetism communicated through them.

This theory (of the origin of which M. Du Potet says he is uncertain) was drawn from the doctrines of *spirits* and *emanations*, which were supported (as M. Thouret* satisfactorily shewed) under the name of magnetism, long before the birth of Mesmer, by Van Helmont, Paracelsus, Libavius, Santanelli, de Locques, Wirdig, Maxwell, Kircher, and many others. By all these, and especially by Maxwell†, the theories of the poles of the human body—of a flux and reflux of an universal fluid, forming a communication between the stars and organized bodies, and passing into the nerves—of the magnetic influence of music—of the *anima mundi*—had been treated of, and fully set forth. Indeed, Mesmer's Inaugural Thesis on the Influence of the Planets was a mere digest of these foolish notions, though it con-

tained all the chief points of the theory we have quoted.

But their theories were not the only things in which Mesmer agreed with the authors of emanations; their *facts* had a corresponding similarity. By magnetism, in its days of youthful vigour, if not in these degenerate times, persons separated by vast tracts of sea and land could, at fixed periods, hold sweet and secret converse; and a somnambule might, by a wish, have rid the world of evil and disease; while by the emanating spirit, and Sir Kenelm Digby's sympathetic powder rubbed on the bloody vesture, the wounds of men afar off were healed. Magnetism had its cures by passes, and the prescriptions of sleeping peasants; emanations had theirs by the royal touch for scrofula, by amulets and incantations, and the crossings of monks and other empirics.

And while magnetism was the *anima mundi*, and the cause of the important influence which the stars, at immeasurable distances, exercise upon us, it had also, long before Mesmer's time, been tangibly employed by vast numbers of physicians on the continent, as afterwards in this country by Perkins, as a cure for rheumatism, toothache, and various nervous affections. Mesmer's instructor in this part of his art was a Jesuit father, Hell, who had himself seemed to have been thus cured of rheumatism. The disciple at once seized his master's facts as examples of the action of the pervading fluid, and commenced the practice of mineral magnetism with zeal worthy of a better cause. It was but a step to consider that the fluid did not need a magnet to convey it from one human body to another; and he laid aside all apparatus, and using his fingers only as tractors, gave origin to animal magnetism. The inutility of magnets, which Mesmer only imagined, was proved by Dr. Haygarth, who soon after discovered that wooden tractors served the purpose equally well, and that the whole affair was a matter of chance, or the effect of faith or imagination in those operated on.

All this theory needs little refutation; the *facts* advanced in it require proofs rather than explanations. Let a man demonstrate the influence of the stars or planets on us, and then he may explain it; or let him prove that there are oppo-

* Recherches et doutes sur le Magnetisme Animal.

† De Medicinâ Magneticâ, 1679.

site magnetic poles in the human body, or that cures are actually performed by the magnet, without the possibility of any other cause being in action, and then he might theorize. But the truth is, that these things do not exist at all. The stars have no more power over us than we over them; nor the magnet on our nerves, or any part of us, than on the stars; while all Mesmer's facts admit of a ready explanation on other grounds. But what can we think of a theory, or its author, which so impudently provides for cases that would contradict it, by supposing two contrary fluids, each all-pervading, such that the presence of one (*which is itself inactive*) in one body, prevents the other from exercising its influence in others? What should we think of a man who said that his galvanic battery would not act, because there was some one in the room who did not believe there was such a thing as electricity? Yet M. Deleuze says that the presence of a person who does not believe in magnetism is sufficient to prevent the manifestation of extraordinary senses in somnambulism. In short, all that is intelligible in this theory is self-evident and utter nonsense; but we may draw from it valuable information. Mesmer is represented by his followers as a wondrous philosopher—a man who dared to break from the trammels of scholastic bigotry—who looked to nature alone for his guide; and they rank him (most impudently, if not ignorantly) with Galileo, Columbus, Harvey, and others, whose doctrines were at first rejected. They try to make of him a martyred sage. His theory shews him to have been an ill-judging plagiarist—an ignorant and credulous man, if he believed the things he affirmed—a designing impostor, if he disbelieved them—a visionary almost to insanity, or a mere juggler—"atrum horum mayis accipe;" in either case his personal authority is valueless, his statements must have corroboration by others capable of judging, and must even then be examined with scrupulous and almost incredulous care.

This theory was at first adopted, to a considerable extent, by Mesmer's pupils, but they seem soon to have found it too heavy for the slender collection of facts they had to support it, and they threw off the planets and stars. Most

of them still talked of a subtle fluid (a condition of matter of inestimable value for theories), and they drew various analogies between animal magnetism and electricity; a science which then consisting of little more than separate facts connected together by very extensive theories, was well adapted for their purpose, because, when they used the same explanations for their assertions and the facts of electrical science, each appeared to have the same degree of truth. We would here point out the different progress of the two sciences, if both can be fairly so called in the same sentence. In Mesmer's time, animal magnetism and electricity might be regarded about in an equally advanced condition. Why has one remained stationary, or even retrograded, while the other has made the most extraordinary progress known in the history of sciences? The bigotry of learned societies, the early prejudices, and the senile objection to novelty, which are said to have marred the one, can scarcely have made the other. The magnetizers point to the discoveries of Harvey, and Galileo, and Jenner, and others, as proofs of the opposition which facts meet when they are first announced, and they would make their art another of those persecuted sciences; but we would point to these facts as evidence *against* them. Let them tell us of any other discovery (especially during the last century) which has made no progress for 50 years, or, taking this from its real origin, for 300 years, but has rather retrograded by becoming more confused; and particularly we would point to the history of vaccination, of electricity, in its various forms of galvanism, electro-magnetism, thermo-electricity, &c.; of the applications of steam, of the stethoscope, of the atomic theory, and of a hundred others, which in their infancy during Mesmer's time, or born since his death, already present the giant forms of exact sciences. Never, indeed, were times so favourable for the development of discoveries—men's minds are athirst for novelties; the floods of knowledge that have continually poured in have not satiated, nor have the streams of quackery nauseated them. Why is animal magnetism still rejected and ridiculed?

But to proceed:—The most exclu-

sively electrical in his views was Petetin. He thought that a woman could neither hear nor see with the pit of her stomach when a piece of glass, sealing-wax, or other non-conductor of electricity, was placed there, and he found hysterical girls, and his somnambules, much affected by thunder-storms, or electrical shocks. He considered that the electric fluid was elaborated in the brain, and passed thence along the nerves, and by the *nervus vagus*, to the stomach, where, accumulated in cases of hysteria, it produced so marked a degree of sensibility that the nerves there could perceive the impressions generally perceived by those of special sense only—as of sight, hearing, &c. But in all this we look in vain for a fact: there is no proof that either an electric or any other fluid is formed in the brain, or passes along the nerves, nor that, if there were, it could be accumulated in any part, or then produce extreme sensibility; nor that the special senses require more concentrated nervous power than common sensation. Besides, all these histories of extraordinary vision, hearing, &c. are, we shall see, but idle tales; and as far as M. Petetin's authority on the subject is concerned, we would observe that that part of his history which he deems most important, and must therefore be supposed to have examined most carefully—viz. that non-conductors of electricity prevented the *clair-voyance*—is contradicted by other magnetizers; who say that the interposition of these, or of any other substances, has no influence whatever. One who could make so great a mistake on one part, can scarcely be regarded as an unquestionable observer on the rest.

M. Puysegur, the discoverer of the somnambule state, took great pains to overthrow M. Petetin's theory, and to establish one of his own; which may be called the theory of faith, hope, and charity. Expressing his belief in the facts which M. Petetin had advanced, as quoted above, he says that they were the result of his preconceived ideas of, and desire for, what would take place; and adds, "if by chance, or by having adopted beforehand some other system, he had imagined that he could not obtain an answer from his cataleptic patients, except through a rose-leaf, or at the decline of the moon in May, these

unhappy creatures, alas! would have remained dumb eleven months and a half in the year; then, at least, with some appearance of reason, he might have cried out, what a prodigy! and many people would have been of his opinion." We confess we should too; and we agree with the author in thinking M. Petetin's facts were the result of his wish, only we would say that "his wish was father to his thought," while M. Puysegur would have it the progenitor of facts. His theory is, that the fluid which he takes from Mesmer to be universal, subtle, penetrant, &c. may be moved by the thought, or will, in any direction, or to produce any purpose that any person desires; and he writes in a most exalted strain on the moral and physical influence which we may by its means obtain over others; coming at last to the fortunate conclusion, that the empire of the magnetizer over the patient extends only to the conferring of benefits on him, because, if he were meditating harm, the patient would have a sensation as if mischief were in the wind, and would wake from the somnambule state. He says, very gravely, that he asked a young lady one day whether he could not, by the power he possessed over her while in somnambulism, make her undress herself; but she said, "No, you can never force me to take off all my clothes; my shoes and my cap as much as you please, but beyond that you will obtain nothing*." Another girl, very properly, expressed the same decision; and a man insisted on it that he should wake directly, if any body were to try to make him sign a blank acceptance while he was somnambulizing.

Need we refute such a theory as this, which in plain words declares that by wishing well, with faith, we may produce any beneficial change in the bodies of the sick? Surely not. M. Puysegur was the most credulous of philanthropists: from the style of his book, and his having no motive to deceive, we do believe that he actually believed his own theory, even though in spite of the opinion of his favourite somnambule, that by a wish he might rid the world of evil, disease still made its ravages. But

* *Mémoires pour servir à l'Histoire du Magnétisme Animal*, p. 196.

what can we think of the statements which a man who could invent and believe such nonsense sets forth as facts? or what of an art in which such a man is revered as a chief discoverer, and one of the brightest ornaments?

M. Deleuze had scarcely a theory. He believed all the statements presented to him (his occupation was not such as to make him a competent judge in cases of this kind), and traced apparent analogies between them and the facts of other sciences, however remote, which seem to have been taken for explanations.

The only remaining hypothesis which we need notice is that of M. Rostan, the most plausible of any, because it pretends to be founded on physiological facts, and comes from a practical physician. We take his views as given by M. Du Potet, at p. 375, *et seq.* He says, "Every thing leads us to consider the brain as an organ secreting a peculiar substance, the principal property of which is, to transmit and to receive volition and sensation, and which appears to circulate in the nerves." Now we affirm, that *nothing leads us to consider any such thing*; the brain has not the structure of a secreting organ; no peculiar secreted substance is visible; no motion of fluid, or any thing else, is perceptible in the interior of the nerves; they do not communicate as the vessels do, so as to permit any fluid to circulate. Nothing, in short, has ever yet been seen of a fluid circulating, or of an agent propelling, or of a canal conveying. The whole of these things, which are the fundamental facts of M. Rostan's theory, had been themselves imagined by others, in order to explain some difficult phenomena of nervous action. Again, "The labours" of M. Bogros seem to prove materially the existence of canals containing a fluid in the nerves. M. Bogros, it is now well known, had injected mercury between the nerve and its neurilema, and had thought it had passed into a canal in the nervous filament itself.

Again, he says that "MM. Prevost and Dumas have demonstrated that muscular contraction is the result of a real electric shock;" and, "Beclard affirmed that he had caused the magnetic pole to deviate by bringing it into contact with an exposed nerve in a living animal;" and he mentions other

things which he thinks prove the identity of the nervous agent and electricity. But, in fact, a far greater number of better-determined facts prove them to be different. The phenomena of the deviation of the magnetic needle, when they did occur (which was very rarely,) were referable to the influence of thermoelectricity. Galvanism acts merely as a powerful stimulus to the nerves, and has no different power over them to that of any other irritation. The isolating property of the neurilema is altogether hypothetical, and nerves are no better conductors of electricity than other equally moist tissues, for it always passes by the shortest passage from one part of the body to another, and diffuses itself in all the surrounding tissues on its way.

But leaving this question as unsettled, or even, for argument's sake, granting the identity of the two agents, the next supposition is altogether unwarranted: that "this agent does not confine itself within the muscles and the skin; it throws itself off with a certain degree of force, and thus forms a real nervous atmosphere, a sphere of activity absolutely similar to that of electrified bodies." Humboldt had invented the basis of this theory, to explain the fact that if a nerve were divided, and its extremities placed near each other, a powerful galvanic current would sometimes pass along the upper portion, and across the interval into the lower portion, so as to produce contraction of the muscle supplied by it. But this is clearly owing to the electricity being conducted through the moist tissues, or the vapour which existed between the divided portions; and the fact itself makes the difference of the nervous and electric agents more probable, because the action of the former cannot be exerted across this interval, as it should be if there were a nervous atmosphere similar to an electrical one between the divided extremities. This phenomenon was the only one which could lead Humboldt to the idea of a nervous atmosphere; it is clearly explicable in the manner above stated. Besides, when, to help his theory, M. Rostan assumed an isolating property in the neurilema, which does not possess any thing of the kind, why did he forget that the cuticle has really such a power in a slight degree, and would be sufficient to prevent the trans-

mission of electricity of so slight intensity as the nervous agent must be? But a pseudo-fact, theorized on by one of so good a name as Humboldt, was too good for the magnetizers to lose, and carefully avoiding any examination of it, M. Rostan has built a lofty theory on it; thus, "the nervous and active atmosphere of the magnetizer, increased, no doubt, by the impulse of his volition, mixes, and is brought into contact with the nervous atmosphere of the magnetized person;" and so on. To refute it all in a word, the ideas he takes of the structure and functions of the brain and nerves are *all erroneous or imaginary*; the identity of the nervous and electric agents is *improved*; the existence of a nervous atmosphere is *disproved*; the influences of the nerves on mind, on any thing beyond the body, is suggested only by what they are wanted to prove—animal magnetism.

We shall quote no more theories; taking these together, even in these slight sketches (to the accuracy of which, though slight, we pledge ourselves) we think it will be agreed that no one art or science ever produced such a set, such an assemblage of errors, based on such unsupported imagination. There is not one which bears even a semblance of scientific reasoning in it. We should have passed them all over in the silence they justly merit, had it not been desirable to show the origin of the mysterious and obscuring phraseology of the art, and the characters of its principal supporters. With men who could form such opinions, and obstinately uphold them, every one is privileged to deal severely: if they believed them, their credulity must have been too great to permit them to have been fair judges either of the apparent evidence of their own senses, or of the probable truth of what was reported to them; if they did not believe them, they were impostors, and their *facts* must be treated, like their theories, with ridicule and contempt. In any case, the facts (so called) of animal magnetism, must be received with the greatest caution; statements contrary to common observation, or common sense, may without compunction be disbelieved, when the authority of the witness has no greater weight than that of most of those we have quoted from.

It may be said that bad theories may be formed to explain facts, as those of the

ancient physicians and philosophers, whose statements are not disbelieved because explained by absurd hypotheses, and couched in their language; but we answer, that these theories were the best that the facts then known permitted them to form; and that many of their statements are really so perverted to suit their theories, that they are, though not intentionally false, yet not wholly true. But the theories of the magnetizers are contrary to facts established at the time they wrote, which, if they were acquainted with, they must have wilfully overlooked; while, if they were ignorant of them, their statements must be received as those of illiterate men.

We must repeat, the authors of the facts on which this theory is based are men on whose evidence we cannot implicitly rely. Many have been proved impostors, or to have been actuated by interested motives; many more have been ignorant of the sciences by the laws of which the phenomena they witnessed must be judged. They are but a few whom we would not include in one of these classes.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Feb. 13, 1838.

THE PRESIDENT IN THE CHAIR.

A PAPER—

On Aneurisms of the Heart; with Cases,
By JOHN THURNAM, Esq.

partly read at the last meeting, was concluded.

Mr. Thurnam's paper consists of three essential parts, which treat of aneurism of the ventricles, aneurism of the auricles, and aneurism of the valves, respectively; and, together with the necessary introductory and supplementary matter, and an appendix of cases, constituted the essay.

After explaining that, by aneurism of the heart, the author means partial dilatation and sacculated aneurism of one or more cavities of that organ, the author proceeds to sketch the literary history of the affection. The first satisfactory account he finds to be that of Breschet, published in 1827; and the first correctly described case that of Galeati, in the *Comment. Bononiensis* for 1757. With regard to the anatomical characters of the affection, the author maintains that they are neither exclusively those of what is

called, in the diseases of arteries, true aneurism, or those of false aneurism; but that they occur of both kinds, and are each produced by various causes; after an elaborate discussion of which, as well argumentative as historical, the author enters upon the history of the first and principal species of the disease—viz. aneurism of the left ventricle. After noticing the fact that the right ventricle is, comparatively with the left, very rarely if ever the seat of aneurism, and discussing the reasons of the exemption, which he finds to be partly mechanical and depending on the structures of the mitral and tricuspid valves, and partly vital and depending on the difference of quality between the venous and arterial currents, he proceeds to detail seven cases of partial dilatation, or aneurism, of the left ventricle. To those details he subjoins a history of the disease, after the numerical method of Louis, and founded on the seven aneurismal cases above referred to (of which four are from the MS. of Mr. Hunter), and on fifty-four more, to which references are given in the Appendix. Aneurisms of the heart he finds to vary much in shape and magnitude, and in their relation to surrounding parts and to other lesions of the heart or its teguments; and of all the pathological changes connected with aneurism, he finds the principal agents to be modifications of nutrition, constituting inflammation and its effects in one class, and simple hypertrophy in the other. The author finds the disease to predominate in the male sex, and in the adult age, and that its principal remote causes are, intemperance, rheumatism, external violence, respiratory efforts, &c. The author then, after a variety of observations on the symptoms and genera of aneurism of the heart, its mode of terminating life, and its organic complication, proceeds to the consideration of the diagnosis and prognosis of the disease. Under the former head the author communicated some observation from the pen of Dr. C. J. B. Williams, contained in a letter to the author, after which the subject of treatment is discussed. The author then proceeds to the history of aneurism of the auricles, after which he considers dilatations of the valves, founding his observations on the latter, on three cases, of which particulars are given. He closes his communication by an appendix, in which are given references to a large number of cases recorded in published works, or in the catalogues of some of the many pathological collections visited by the author in the course of his researches.

ROYAL INSTITUTION.

Friday, Feb. 9th, 1835.

On the Formation of Shells.

MR. GRAY, whose papers in the *Philosophical Transactions* are well known, delivered an interesting lecture on the growth and structure of shells. He explained how shell is secreted by a portion of the skin of the animal termed the mantle, at the edge of which, termed the collar, the process usually goes on with the greatest vigour. The simplest form of shell known to us is that of a more or less flattened cone, as exemplified in the common limpet, and all other varieties of form may be shown to be modifications of this type; the whorled structure of shells being caused by the animal's growing most to one side, and thus turning round a spiral axis. Shells are subject to periods of unusual increase, at which time strong bands or ribs are secreted, which add materially to their strength; the causes of this phenomenon are however unknown. In many shells we find large spines; these at one time corresponded to certain processes of the body of the animal. But when a shell continues to increase in size, and to turn round its axis, these spines might form a serious obstacle to its further growth. To provide, then, against this inconvenience, nature has bestowed on the animal a power of removing such spines as may be in its way. Mr. Gray did not explain how this is effected; we presume he means by some process of absorption, but we may remark, that the question as to the mode of this removal is not as yet set at rest by naturalists. At different periods of their growth, some shells vary so much in their appearance, that naturalists generally, even including Lamarck, have believed that the animals had the power of casting their shells, like many of the crustacea. This opinion has, however, been satisfactorily disproved: and the great change in their appearance is caused by the animal's turning back the alæ of its mantle over the edges of the shell, and secreting a fresh layer of shell, as in the case of the cowries. The beautiful variety of colours in shells is produced by a number of glands which secrete colouring matter, and are usually seated around the neck of the mantle. If these glands secrete constantly, then the shell is marked by coloured bands, but if they do so only at times, spots are produced; and thus every variety of colouring and marking may be explained.

When the minute structure of shells is examined, it is found that the materials are in many cases not deposited in parallel layers, but in a method which greatly increases their mechanical strength, and

which has been applied to the timbering of ships. Some shells are provided with an external membrane, which may be considered as analogous to the periosteum of bone, and has been termed the *periostæum*, while others are devoid of it. The animals inhabiting shells are very generally provided with a flat round layer of bone situated on the inferior surface of their bodies, with which they close the aperture of their shells, when their bodies have been retracted within. These have been termed *opercula*. Mr. Gray concluded by alluding hastily to the structure of the bivalves, whose shells are constructed on the same plan as those of the univalves; and by explaining the beautiful provision in their hinges, by means of which the two valves are kept open by ligamentous bands when the animal is in its natural situation with its body exposed to the waters of the ocean, while by means of a mouth it is enabled to counteract the natural action of the ligaments, and to close its shell at pleasure.

MEDICAL DIRECTIONS FOR THE USE OF PEDESTRIAN TRAVELLERS.

If any one intends to make a long tour on foot, it is necessary to take some precaution. I need not say that English shoes are the best. I do not mean new shoes, but those of which the upper leathers are good and soft, and have been worn to fit the shape of the foot. To such a pair of shoes let an additional sole be put, with small nails at the toes and sides, care being taken that the heel be not either too high or heavy. Let them be laced a short way up the instep, and of a size to allow the foot to sit easy without being loose, when a woollen stocking is on. Of these strong shoes have two pair, and a third pair, not of such strong material, to be worn when you come to the end of your journey. As to the stockings the greatest care must be taken in the choice, as such as are generally sold in shops are sure to cause blisters both at heel and toe. If you examine the ordinary qualities of stockings in shops, you will find that the threads are drawn together to a point in the middle of the heel, and about the ball of the big toe. Avoid such stockings, as they are sure to cause misery. The stockings made by old women on wires are the best, and the finer the wool the better. Of these there should be four pairs; and if a stocking be put over each shoe, (the outside innermost,) they will not take much room, and will at the same time prevent the shoes from soiling the other things in the knapsack. As to other requisites, the first to be provided is a good knapsack of the best oil-skin. It

is to be had in all the military store-shops in London. Care should be had to have the straps of the best patent leather, and a degree broader than usual. The proper breadth for ease is the regulation strap for the Guards' knapsack. They should be so long that you can use them in the foreign manner if you choose. By this I mean, that in the foreign knapsack the fixture from which the shoulder straps play, is placed in the centre of the knapsack, while the English fixtures are placed on the points of the shoulders, just in a line with the shoulder straps, so that the whole weight of the knapsack is on the upper part of the arms, instead of being divided over the back. In the French manner the knapsack sticks closer to the back, consequently you do not feel its weight so oppressive.

When provided with a knapsack, get a wide cloak, (so wide as to go over the knapsack,) of the very finest silk oil-skin, long enough to reach to the middle of the thigh; likewise an oil-skin to the hat. Caps are recommended, but a hat is preferable, as you can carry things in the hollow of it. For a cat, nothing is so good as a surtout made of the finest cloth; it should button up close to the neck to avoid cold: the oil-skin cloak can be used either for sitting or laying on the ground. Have two pairs of trowsers of dark gambrin. As it is of consequence to walk cool, if possible, march without drawers, but be sure to put them on at the end of the journey; one pair is enough; they can be washed and dried while you are in bed. As to shirts, have one in the knapsack, and a very long night shirt made of the finest and lightest cotton, which will be found of the greatest benefit, when you are not sure of the cleanliness of the bed. If your trowsers are wide you can even wear it at the end of a day's journey. Of course a fresh flannel nether-vest must always be in the knapsack. The best gaiters to wear, are those used by the French when shooting. They are made of the strongest soft leather, with straps to tighten, if necessary. They should be as high as the knee, and buckled over the trowsers, so that, however dirty the roads may be, on throwing them off, you find your trowsers quite clean and dry. The great difficulty in walking is to keep the feet in good order. This can be done if a little attention be paid at first. For some days before starting dip your feet in hot water as often as possible for a few moments, and then rub them quite dry. Let this be done morning and evening, till you find the feet quite free from a damp feeling. Provide yourself with a good sized tin box, full of the best yellow, or, as it is called in some places, soft soap. It has something the

appearance of honey in the comb. Before starting in the morning, rub the soles of the feet, especially about the heels and toes, with the soap, until it has the appearance of a good lather for shaving, and then put your woollen stockings on. Let this be done every morning before starting, and you will find, even in the hottest or wettest weather, you will be able to do a great deal of work, and at the end of the day find your feet cool and free from blisters. Instead of washing the feet at the end of a journey, rub them first with a damp cloth, and then dry them completely. In some places on the continent, it is not possible to get this soap; but in almost every apothecary's shop you can purchase stag fat, which does very well; and if you cannot get stag fat, buy goose fat or hog's lard. With these fats, I first rubbed the feet with spirits, which is an improvement; but nothing can stand comparison with yellow soap. Have your stockings washed as often as possible; and if they have not time to dry during the night, they can be easily buckled on the outside of the knapsack. By attending to these directions, and by instantly rubbing yourself dry, and putting on fresh flannels and linen at the end of your day's work, and eating as much animal food as possible, yet drinking no more than is necessary, both body and feet will be in the highest condition.

To prevent thirst in hot weather, nothing is better than to take a great quantity of fresh butter with your bread for breakfast. Avoid drinking water as you would poison; in short, drink as little as possible of any thing, and do not give way to the first sensation of thirst. I strongly recommend starting at day-break, having previously taken breakfast.—*Dublin Journal, from Colonel Shaw's Memoirs.*

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, Feb. 13, 1838.

Abscess	3	Inflammation	25
Age and Debility	57	Bowels & Stomach	3
Apoplexy	3	Brain	3
Asthma	29	Lungs and Pleura	18
Cancer	1	Influenza	1
Childbirth	2	Insanity	5
Consumption	45	Liver, diseased	1
Convulsions	30	Measles	10
Croup	1	Mortification	3
Decidition or Teething	1	Paralysis	4
Diabetes	1	Rheumatism	3
Dropsy	9	Scrofula	1
Dropsy in the Brain	8	Small-pox	11
Dropsy in the Chest	1	Sore Throat and	
Epilepsy	2	Quinsey	1
Fever	16	Thrush	1
Fever, Scarlet	8	Unknown Causes	43
Fever, Typhus	7		
Heart, diseased	2	Casualties	6
Hooping Cough	16		

Decrease of Burials, as compared with }
the preceding week } 57

HOLLOWAY'S UNIVERSAL OINTMENT.

To the Editor of the Medical Gazette.

SIR,

HAVING lately seen a printed hand-bill, being an advertisement of "Holloway's Universal Ointment," which contains what is represented to be a copy of a letter from me, in recommendation of this nostrum, I think it due, both to the profession to which I belong, and to myself, that I should make known the following circumstances:—

Several months ago a Mr. Holloway, of whom I was led at that time to believe that he was a respectable person, sent me a pot of ointment, which he said had been sent to him by an acquaintance, from abroad, requesting me, at the same time, to make a trial of its medicinal properties. Accordingly, I took it with me to St. George's Hospital, and made use of it in some cases of ulcer. Some time afterwards I received another communication from Mr. Holloway, inquiring what were the results of my experience as to its effects. I wrote to him in answer, that I had not found it to be at all superior to the (commonly-called) digestive ointments already in use, and that I preferred using the latter, as he had neglected to inform me of the composition of the new ointment.

I suppose that this letter has been used as the foundation of that which is published as mine in the printed hand-bill; and I think it not improbable that there may be no words contained in the latter which are not to be found in the one which I actually wrote. The greater part of my letter, however, has been omitted; and the result of this piece of effrontery is, that the sense of it has been entirely perverted, to suit the purpose of those by whom this nostrum is vended.—I am, sir,

Your obedient servant,

B. C. BRODIE.

14, Saville Row,
Feb. 13, 1838.

METEOROLOGICAL JOURNAL.

Feb.	THERMOMETER.		BAROMETER.
	from 38	to 47	29.08 to 29.95
Thursday	8	43	28.74 28.93
Friday	9	31	29.12 Stat.
Saturday	10	24	29.25 29.48
Sunday	11	19	29.51 29.52
Monday	12	15	29.50 Stat.
Tuesday	13	12	29.57 Stat.
Wednesday	14	11	

Winds N.E. and N.W.

Except the 8th and two following days, generally clear, rain on the 8th and 9th; a little snow on the evening of the 13th.

Rain fallen, 2.125 of an inch.

CHARLES HENRY ADAMS.

WILSON & SON, Printers, 57, Skinner-st., London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, FEBRUARY 24, 1838.

LECTURES
ON THE
PHYSIOLOGY AND DISEASES OF
THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL
AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of
1836 and 1837,

At the Anatomical School, Kinnerton-Street,
near St George's Hospital,

BY CHARLES J. B. WILLIAMS, M.D. F.R.S.

LECTURE XIV.

Diseases of the Air-tubes (continued)—Altered Sensibility or Mobility—Spasmodic Asthma; Pathology and Course; Physical Signs; Extent and Effects of the Bronchial Spasm—Paralytic or Atonic Asthma; Mode of Production and Signs—Treatment of Spasmodic Asthma; Treatment of the Paroxysm; General Treatment; Signs of Improvement; Treatment of Paralytic or Atonic Asthma—Treatment of Increased Sensibility of the Bronchial Membrane.

WE have surveyed in outline the features of those affections of the air-tubes which are either inflammatory or characterized by a change of their secretion. But besides such affections, there may be diseases of the *sensitive* and *moving* functions of these tubes. There may be an excess or a diminution of their sensibility, and of the contractility of their muscular fibres, which may vary with the sensibility, or independently of it. We have already noticed these modifications of properties as producing certain varieties of cough; excessive contraction of the bronchi giving the wheezing and whooping characters to cough, and their defective contraction oc-

causing it to be hollow, sonorous, and without expectoration. We have now to advert to the same modifications extending to the ordinary acts of breathing, and constituting what is commonly called *nervous* or *spasmodic asthma*. But from what I have just said, you may perceive that such disorders may be of very opposite kinds, and that the term *spasmodic asthma* will apply only to those cases in which the sensibility or contractility of the air tubes, or both, are exalted, and that there may be another class of cases depending on a diminution of these properties, and to which may be applied the name *relaxed* or *paralytic asthma*. We have both spasmodic and paralytic affections of other muscular organs and canals—of the bladder and urethra, the intestines, &c.; and analogy favours the supposition that we may have similar conditions of the muscular fibres of the air-tubes. But this subject has not yet been duly investigated; and although I have seen many facts which support the view thus presented by analogy, further research both at the bed-side and in the dead-house will be required to establish it in a practicable manner. That very able physician, Dr. W. Stokes, has drawn the attention of the profession to this subject, in connexion with dilatation of the air-cells; and, if I mistake not, we shall do well to think of it in connexion with the function of the tubes also. Let us first, however, attend to the better-known complaint, spasmodic asthma.

The term asthma is generally given to dyspnoea occurring in paroxysms; but we have seen that attacks of bronchial congestion, and bronchial flux, or dry and pituitous catarrhs, may come on suddenly, last a longer or shorter period, and cease in such a manner as to merit the name asthma; by which term, in fact, they are generally known in this country. In the greater number of cases of asthma there is good reason to suppose that one or other of these affections, or some degree of in-

flammation, is present, and by increasing the irritability or the irritation of the bronchi, causes an undue contraction of their circular fibres. An increased vascularity of the bronchial membrane may heighten its sensibility, and augment the contraction of those fibres that are in relation to it, and the same result may ensue from the irritation of an unusual quantity or quality of secretion within these tubes. So, on the other hand, the continuance of inflammation, the thickened and altered condition of the membranes which it induces, may tend to impair their sensibility, and to injure in proportion the contractility of the tubes. In all these cases the modification of the sensibility and contractility of the air-tubes is secondary to other lesions which are more essentially vascular. But there are also cases of a purely nervous character, in which the disease is truly a neurosis; and the temperament of the patient, the nature of the exciting causes, the very sudden attack and removal, and the irregular duration of the affection, sufficiently point out this character. Thus they commonly occur in nervous or hysterical persons. The attacks are excited by strong or peculiar odours, the smell of a stable, close rooms, particular conditions of the atmosphere, irritations of the stomach, or mental emotions; and these causes often quite suddenly bring on the attack, which, if severe, obliges the patient to assume a remarkable and very characteristic attitude, with the body bowed forwards, the arms resting on the knees, the chest contracted, with the feeling of a tight cord or heavy weight on it, the face suffused with an expression of great distress, the veins turgid, and perspiration soon beginning to flow freely; whilst all the muscles of respiration, ordinary and supplementary, are trying their utmost to introduce air into the chest. With what success these efforts are made, we may learn by applying our ear to the chest, where, in spite of the force of the motions, scarcely any sound of passing air is heard. The contractions of the muscles often give an external muscular sound; but within the chest there is only a very weak respiratory murmur, with occasional wheezings or whistlings. The violent action of the muscles of inspiration seems to diminish rather than to increase the entry of air; when the efforts are less violent, especially towards the end of the paroxysms, now and then the air will be heard to enter quite well, as if the obstacle were suddenly removed, but at the next breath all is as obscure as before. At these instants we must suppose that the spasm of the bronchial muscles is momentarily relaxed, and Laennec has pointed out a method of causing at will this relaxation,

which may be useful in enabling us to determine the nature of the disease. If we desire a patient, who labours under the asthmatic spasm, to restrain his efforts of breathing, and to hold his breath altogether for a few seconds, or what amounts to the same thing, to count with his voice as many numbers as he can without taking breath, and then as quietly as possible to breathe again, the air will then be heard to enter freely into every part of the lungs, but in a breath or two after the spasm regains its hold, and the respiration becomes as obscure as ever.

Now how does this happen? How does a moderate inspiration, *after holding the breath*, introduce air freely, when the strongest efforts are otherwise unavailing to produce the same effect? Laennec used to say that the spasm was thus overcome *by surprise!* This explanation is too poetical to be received in physiology. A surprise of the bronchial muscles conveys no definite idea to us, unless we suppose the tissues to be endowed with the mental faculty of *marvellousness*. I do not see how we can explain the phenomenon in question under any other supposition than that there is, as Laennec supposed, a temporary relaxation of a tonic spasm of muscular fibres; and this relaxation I should ascribe to an increased degree of the same cause, which usually and naturally effects the relaxation of these fibres. We have before dwelt on this point in speaking of the physiology of respiration. We were then led to consider the contraction of the circular fibres, excited by a certain degree of foulness of the air within them, as an essential part of normal expiration. Now the foulness of the air being increased by holding the breath long, would stimulate these fibres to their utmost contraction—a contraction even beyond the state of asthmatic spasm: their irritability is thereby for the moment exhausted, and the spasm becomes consequently relaxed, and the air is heard to enter freely; but after a few moments' relaxation the irritability is again restored, and the exciting cause of the spasm remaining, the next breath may find the contraction as strong as ever.

Such I believe to be the true explanation of the phenomenon in question; and, did time admit, I should like to illustrate the principle on which it is founded by several examples of other spasmodic diseases, for it is one of much practical interest, and has not been sufficiently recognised. It is in this way that electricity will sometimes relax a spasm: it stimulates a muscle to a contraction still more forcible than that of the spasm; the irritability of the muscle is thus, as far as relates to the exciting cause of the spasm, exhausted; and a continued application

of the same stimulus may remove the excessive irritability, or subvert the morbid relations that habit has established between the contraction of the affected muscle and some irritating cause in the system. Some of you have heard Sir Benjamin Brodie, on the same principle, explain how a spasmodic constriction in the urethra may be relaxed by the application of a bougie: the stricture refuses it passage at first; but by keeping the end of the bougie in contact with the stricture for a few minutes, the irritability of the contractile fibres is exhausted, the spasm is relaxed, and the bougie passes.

The distinctive physical sign, then, of spasmodic asthma is this imperfect sound of the respiratory murmur, even with forcible breathing, *except after holding the breath*, when it becomes as loud as, or louder than, usual. When the bronchial spasm is considerable, especially during the paroxysms, the chest sounds ill on percussion, not with the absolutely dull *mat* sound produced when solid or liquid is in the chest, but a short, tight, unresonant sound, like that which the chest yields on a forced expiration. This is caused by the contracted state of the lungs when under the influence of the bronchial spasm; the walls of the chest, therefore, being pressed inwards by atmospheric pressure, are not so free to vibrate as usual, when there is more of a balance of pressure on either side. You may generally obtain a better sound by striking on a *finger* or pleximeter pressed on the chest strongly enough to exceed the contraction of the lungs; this restores to them their springing resistant quality, by which they give a regularity to the vibrations of the thoracic walls. The same contraction of the lungs, when excessive, sometimes causes the diaphragm to rise higher than usual in the chest, and to produce a remarkable hollow at the epigastrium; and the whole chest presents a tight and contracted appearance.

Now you must perceive from all these signs that the contraction or spasm of the air-tubes must be very general, and extend to their very terminations; for spasmodic constriction of the large and middle-sized tubes cannot be complete on account of their cartilages; and with partial obstruction of these, the smaller ones and their terminations being free to expand, you would have the respiratory murmur more noisy than usual, and accompanied with all sorts of dry rhonchi, produced by the resisted passage of the air to these expandable parts, as we find in the first stage of bronchitis. But in spasmodic asthma there is very little sound, and so little expansion of the chest, that we are constrained to suppose that the smallest tubes

are constricted likewise, and that either from their almost total occlusion, or from the presence of spasm in their very terminations, the vesicular structure itself is but imperfectly expanded. It may occur to you that I am assuming largely, in attributing these great effects to the operation of the tiny circular fibres of the air-tubes, whose muscularity has been questioned by many, and whose very existence in the smallest tubes is a matter rather of analogy than of demonstration. Well, I admit that we want further information respecting the physiology of the lungs, before we can put forth these views with confidence; but assuming that the whole system of the air-tubes does possess a power of muscular contractility (which I do on the authority of Vernier and Wedemeyer, who saw the smaller tubes contract almost to obliteration on the application of a mechanical or chemical stimulus), it is plain that a very minute force, contracting the caliber of each tube, will be enough to countervail a great force exerted by the muscles of inspiration. Recollect that the muscles of inspiration expand the lungs only indirectly, by atmospheric pressure; that this atmospheric pressure amounts to a weight of fifteen pounds to the square inch; that this amount of pressure is never realized by the power of the muscles; and that which is exerted, when distributed over the vast bronchial surface, becomes very small in each tube,—and you will be able to understand that the bronchial muscles, delicate as they are, may be sufficient to command the passage of the individual tubes, and to resist collectively the full introduction of air into the lungs.

Those who suffer much from spasmodic asthma are seldom free from a shortness of breathing in the intervals; and the frequent recurrence of the paroxysms generally brings with it more of this habitual dyspnoea. If we examine their chests we find the same diminution of respiratory sound as during the paroxysm, but in a less marked degree; and the test of holding the breath proves that spasm exists here also, having become in a measure habitual. No doubt the frequent recurrence, or long continuance of these spasmodic contractions of the tubes, must lead to a permanent diminution of their caliber, and the other tissues change and fix them in this constricted size. We see the parallel of this in the irritable bladder, which, after long continued attacks of spasm, eventually becomes permanently contracted. Where the disease is purely spasmodic, this more lasting change might not ensue for a very long period: but I have before mentioned to you, that with spasm of the circular fibres there is so commonly associated congestion, irritation, or inflamma-

tion, or these are so frequently induced by the spasm, that the phenomena of these pathological conditions are very commonly combined with those of spasmodic asthma. Hence, in asthmatic subjects, you may have an attack of bronchitis, of dry catarrh, or of humid catarrh, assume in its course a spasmodic character; and you may have a paroxysm of asthma, which came on suddenly as a spasm, terminate by a copious catarrhal secretion. The latter seems to be a common course of the asthmatic paroxysms, which are sometimes associated with organic diseases of the heart. The congestion which these determine in the membranes and structures of the lungs exalts their sensibility and irritability; and where the circular fibres are naturally disposed to spasm, this most readily excites it; and this spasm may not be entirely relaxed until the congestion is relieved by a free secretion from the bronchial membrane.

Hitherto we have considered only the spasmodic form of nervous asthma, or that dependent on an *excessive* sensibility and contractility of the bronchial tubes; but as, in examining the elements of dyspnoea, we found that a *defect* of these properties would disorder the process of breathing, we are led to inquire whether there may not be a nervous asthma or dyspnoea of this kind, from weakness, or a *paralysis* of the circular fibres, or of the nerves, whose sensibility guides their contractions? We have parallel affections of the alimentary and urinary passages, when, from local or general causes, their moving fibres become torpid or paralyzed; and if I am right in supposing that the action of the circular fibres, and the elasticity of the longitudinal fibres of the bronchi, are essential to the effectual performance of the act of expiration, defects of the properties of these tissues must cause a proportionate imperfection in this act. There are some facts which seem to me to bear directly on this point. I mentioned to you before, that Mr. Swan found, that in animals in which the par vagum had been divided in the neck, the act of inspiration became imperfect, and the lungs permanently distended. Laennec remarked, that the lungs of persons who died from suffocation in sewers, appeared, on opening the bodies, uncommonly distended, and did not collapse as usual; and he proposes it as a query, whether this may not be from a sudden dilatation of the air-cells. Now, sulphuretted hydrogen gas, which is a chief component of the exhalations of sewers, is remarkable for its power to destroy muscular irritability; and it seems very probable that its direct paralyzing effect on the bronchial fibres may be the cause of the distension

remarked by Laennec. We shall have occasion to return to this subject, in connexion with the lesion *dilatation of the air-cells*, termed by Laennec, *emphysema of the lungs*; and we shall then advert to the view of Dr. W. Stokes, which embraces a similar pathological notion. The phenomena of impaired contractility of the bronchial fibres would be the converse of those of spasmodic asthma; the chief difficulty being in the act of expiration, which would be performed imperfectly and with much wheezing and effort, whilst inspiration might be short and comparatively unembarrassed. Now we do meet with many instances of dyspnoea, generally with old bronchitic affections, but occasionally in cases more of a generally nervous character, and in hysterical females, in which this difficulty of expiration is the prominent feature. So, also, we see the defective action of the contractile fibres of the intestinal tube arise sometimes from previous over-irritation, and sometimes from more directly weakening or paralyzing causes, and sometimes from that irregular distribution of the nervous influence that produces the phenomena commonly called hysterical. Nay, if we consider that irritations and inflammations first exalt, and afterwards injure the contractile properties of hollow organs or tubes, and that these irritations or inflammations affect successively different parts of the same tubes, we can see that spasmodic and relaxed asthma may co-exist in the same person, one part of the bronchial tubes being unduly contracted, and another unduly relaxed, from an irregular distribution of the property of irritability. We have not time to pursue this subject further at present, but we may revert to it by and by in connexion with dilated bronchi and pulmonary emphysema; and, in the meantime, I recommend you to bear it in mind when cases of asthma or dyspnoea come under your notice.

The treatment of spasmodic asthma may be considered in relation to the paroxysm, and to the general state of the body in the intervals. One point is to counteract the exciting cause of the spasm; the other, to remove or destroy this cause altogether. To relax the spasm of the bronchial tubes, various measures may be suited, according to the immediate cause of the spasm. When this is purely nervous, with little or no bronchitic or catarrhal complication, such antispasmodics as aether, valerian, asafetida, opium, belladonna, and the fumes of stramonium or tobacco, will sometimes succeed, and each one of these has proved more successful than the others in particular cases, but seldom retains its efficacy long. A more generally and per-

manently successful remedy is strong coffee, made by infusion, long ago recommended in this country by Dr. Bree, and much extolled by Laennec. I have known some asthmatic persons who relied so much on its efficacy, that the very idea of being out of the reach of it would be enough to bring on a fit; and they most scrupulously avoided using coffee as an ordinary beverage, lest this should impair its efficacy as a remedial agent. This was a good rule, for even this is not free from the tendency of antispasmodic and narcotic remedies in general, to lose their power by frequent repetition. In some cases, sudden strong impressions on the system, such as by dashing pails of cold water on the body, or passing moderate electric shocks through it, have been known to stop a paroxysm of asthma; and their mode of action must be referred to the principle of which I have spoken before. I know of one remarkable case, in which there is apt to come on quite suddenly a loss of voice, and even of the power of articulation, from a spasm of certain laryngeal muscles, and to some degree of the bronchi also. The subject is a lady of strong mind, and by no means fanciful, or (according to the general application of the term) hysterical. For a considerable time relief was instantaneously given merely by her taking a few electric sparks with her fingers. This remedy afterwards lost its efficacy; and even shocks failed to relax the spasm. Subsequently it was found that holding a lump of ice in the back part of the mouth was as effectual in loosening the tongue and the breath as the electric spark had been. This affection was purely nervous; it could be excited at any time by a strong mental emotion, or a slap on the back, and always ceased as suddenly as it came on.

If the asthmatic spasm be complicated with bronchitic or catarrhal affections, which is very frequently the case, the remedies recommended for these may often be advantageously combined with some of those just named; and when the nervous affection does not form the chief part of the complaint, it is probably dependent only on the altered condition of the membrane, which is either inflamed or congested, and to which, therefore, the remedial agents must be chiefly addressed.

The fulfilling of the second indication, to diminish excessive irritability of the bronchial muscles, or to remove the causes of irritation by which they are excited, will be best aimed at by various means which tend to restore a proper balance of the functions of the whole system, and to improve its general health. Of these the most effectual are those of diet and regimen.

Particular rules can scarcely be laid down, as the proper measures will vary greatly, according to the circumstances; but the experience of the patient will generally give a clue to the most eligible plan. The shower-bath, and moderate exercise in the open air, avoiding walking against a strong wind, are very beneficial in most cases. Of medicinal agents, besides those necessary to regulate the secretions, the metallic tonics sometimes do good, by diminishing the morbid mobility of the bronchial muscles, or the sensibility of the nerves that influence them; and I have known, in various instances, the sulphate and oxide of zinc, the subnitrate of bismuth, the nitrate of silver, and the sulphate of copper, severally beneficial in diminishing the tendency to the recurrence of the paroxysms. Probably these remedies act through the nerves of the stomach (the *par vagum*), which are so closely associated with those which influence the bronchial fibres; and they may do this directly, or indirectly, by improving the condition and function of the stomach, disorders of which, in some form or other, are so commonly associated with spasmodic asthma. It has often occurred to me that *bad*, which is known to exert such a paralyzing influence on the muscles of both animal and organic life, might be capable of lowering their irritability when excessive and excited to spasm; but its administration would require much judgment, and I have not had an opportunity of trying it.

The signs of improvement are (besides the less frequent occurrence, diminished severity, and shorter duration of the paroxysms), a freer state of the respiration in the intervals, so that the vesicular murmur is pretty audible, without much admixture, throughout the chest, and is increased in loudness by quicker and deeper inspirations, not stopped or impaired as during the continuance of the asthmatic tendency, when additional effort will often at any time excite the spasm. In the cure of this, as of other spasmodic disorders, it is very necessary to watch the circumstances that excite the paroxysms, in order to be able to avoid them; for the frequent occurrence of spasm increases the chance of its recurrence, until it becomes habitual, and may be excited under almost any circumstances. The evil of an habitual asthma is not only the inconvenience and distress occasioned by the paroxysm itself, but also the permanent changes which it may induce in the structures of the lung—such as contraction and rigidity of the air-tubes, congestions and other lesions of the parenchyma, diseases of the heart, &c.

I have little to say on the treatment of

the dyspnœa arising from paralysis, atony, or weakness of the contractile and elastic fibres of the air-tubes. Depending, as this affection usually does, on previous inflammatory lesions, the remedies generally useful at the decline of those lesions are such as may be supposed to act, in some measure, by stimulating or giving tone to the bronchial fibres. Thus ammoniacum, myrrh, benzoin, the balsams of copaiba and Peru, and the inhalation of tar and other stimulating vapours, besides their operation on the secretory function of the air-tubes, may probably have an influence of this kind on their moving fibres; and they may thus improve their condition, in relation to the act of both expiration and expectoration; the difficulty of which often forms the most prominent feature in many protracted cases of inflammatory and congestive disorders of the air-tubes.

Most commonly, when the sensibility of the bronchial tubes is increased, their contractility is so likewise; when the inhalation of cold air is painful, it also seems to take away the breath: but I have before hinted, that the animal sensibility and organic contractility may not always be in proportion to each other. Thus spasmodic asthma may be unattended with any other pain than that common to dyspnœa; and the relaxed state of the bronchial tubes, marked by difficult or imperfect expectoration, is sometimes accompanied by an increased sensibility of the bronchial membrane, so that the breathing of cold or irritating air becomes unusually painful. In a few such cases, I have known relief to be afforded by the inhalation, twice or three times a day, of the vapour of hot water, to which a few grains of camphor have been added. When this fails, I should be disposed to try other narcotics, such as hyoscyamus, or conium; which may be diffused through the water, in the form of recent extract, or saturated tincture, rendered more volatile by a small addition of liquor potassæ. The proportions must be determined by experience, beginning with small quantities. In these cases, and others in which soreness, or rawness, of the stomach is complained of, much comfort may be derived from the use of *Jeffreys' Respirator*; or if the patient do not choose to be so muzzled, some of the same benefit may be obtained by tying a porous silk handkerchief, or a piece of fur, over the mouth; or, in the case of ladies, even by wearing a thick veil.

LECTURES ON TUMORS;

Delivered at St. George's Hospital,

BY CÆSAR HAWKINS, ESQ.

Definition of Tumor.—Modes of Formation.—Causes.—Inflammation of.—Destruction by Ligature of Vessels.—Rapidity of Growth.—Texture influenced by Situation.—Analogous and Heterologous Formations.—Physical Characters.—Malignancy.—Classification.

I. Order, ENCYSTED TUMORS.

I. Genus, *Sebaceous Encysted Tumors.*

I HAVE now to bring under your consideration the subject of *Tumors*. But what, in surgical language, is a tumor? Strictly speaking, it may be, perhaps, defined as a new formation—an addition to some part of the body of a substance organized, or partly organized, and not the result of inflammation only. A tumor is not a mere swelling, you will observe, however large. I have seen the breast enlarged so as to weigh more than eleven pounds; but this is hypertrophy only. I have seen the scrotum immensely enlarged; it has been removed, in fact, when of the weight of 100lbs.; but this, too, is a kind of hypertrophy of the skin and subjacent textures. You may here see the tibia twice as large and thick as usual, but this growth is only the enlargement of inflammation; by the side of it is a preparation of the humerus, with some exostoses upon it. Now these, however small, constitute a form of tumor, according to the definition just given.

There is, however, a great difference in the mode in which tumors originate.

1. A tumor is, at one time, a *simple addition* to the original texture: such is sometimes a fatty tumor; such are some forms of exostoses, in which the natural bone is unaltered, and the new formation possesses the same structure nearly as the bone to which it is attached; such is sometimes a cancerous tumor, when attached to the side of the breast, or other organ. And these new formations, you will observe, may sometimes be similar, or at least analogous, to the tissue in which they arise—as the exostoses, or fatty tumor, to the natural bone, or fat—or they may be entirely dissimilar from every natural texture, and not present even an analogy in their structure to any normal part of the body; as in the instance of the cancerous tumor just alluded to. Hence arises the division which has been made, of new or accidental formations, into *analogous* or *heterologous* structures, accord-

ing as they resemble or are unlike any of the natural textures of the body.

2dly. A tumor is, at another time, the conversion of an original tissue, or structure, into a new one. A gland inflames, and is enlarged by the usual deposits of inflammation; it may, however, have a new action established in it, its natural textures may be absorbed, and a new substance deposited in their place, and it will then constitute a tumor; it may be of a carcinomatous nature, or a medullary structure, and so on. Such conversion of one substance into another is generally a change of the natural textures of the part into something not known before in any part of the body—i. e. the tumor formed by change of structure, instead of by simple addition, is generally, though not always, a heterologous tumor. You will perceive, moreover, that, commencing in this manner, a tumor may actually be less in size than the organ whose place it has usurped.

3dly. A tumor may be formed thus: simple enlargement has taken place (in the breast, for instance) in consequence of inflammation; lymph has been organized, so as to leave a swelling for twenty years perhaps: then a new action ensues; there is a deposition of new structure, fungous or scirrhous, and it grows rapidly, and the mere swelling becomes a tumor.

Thus, then, a tumor may originate in three different ways, all differing from inflammation. This distinction was first made, I believe, by Mr. Abernethy, and it is of great importance in practice. For the most part, all simple enlargements, or mere swellings, are more or less under the control of remedial agents, and seldom require a surgical operation. A gentleman, for instance, under my care, had a swelling of the testis, which was at least eight inches long, ulcerated, and with large fungous projections, like those of a malignant disease: but by a course of mercury of six weeks' duration all this was cured. On the other hand, almost all new formations resist treatment, and most of them require the removal or destruction of the tumor.

Many speculations have been entered into with regard to the causes of the growth of tumors. Mr. Hunter and Mr. Abernethy supposed that they began by the organization of a clot of blood accidentally effused, or of lymph deposited by inflammation. A preparation is seen in the museum of the College of Surgeons, to show the first organization of a coagulum in this way; but it is to me any thing but satisfactory, and is probably a mistake altogether; and certainly there appears, in the majority of cases, no proof of either circumstance, and in many the tumor has

clearly not originated in these ways. With regard to the organized lymph as a cause of tumors, we see constantly the thickening around the joint, or the hardness in the breast, produced by lymph, but it is only now and then that a tumor originates in this thickened part. It has been well remarked by Mr. Lawrence, that if the conversion of either blood or lymph were the general cause of tumor, we ought to be able to see its several stages; whereas a tumor of the size of a pea is as distinct and well defined, in many cases, as one of 20lbs. weight. Something more, then, is necessary to make the organized lymph a tumor, and we are, for the most part, ignorant of the ultimate cause of their growth, whether it be in the state of the part or the condition of the general system.

Inflammation is, in fact, an occasional cause only of the growth of a tumor; and the effects of inflammation are a kind of excitant: a tumor grows in the inflamed part from some peculiarity (generally a constitutional peculiarity); and the inflamed and thickened texture is selected for the situation of the tumor, as the weakest; just as if a tendency to ulceration is induced by depression of the constitution, a cicatrix, or other newly-formed part, will usually be the seat of the ulceration.

Sir Edward Home imagined that an injury was always the cause of the growth of tumors; in which injury there was an action more than enough to repair the injury. But an injury, also, is only an occasional cause. A person received a blow on the femur, and this cartilaginous exostosis was the result; but a hundred other persons would only have inflammation established by the blow. A groom, in catching a horse, ruptured a few fibres of the pectoral muscle, and this large tumor of fungus hæmatodes appeared in the part in a short time; but how many persons will rupture their muscles without any such consequence!

Although, then, inflammation, or an injury, will now and then be a local cause for the formation of a tumor, yet, in the greater number of cases, with all the fondness of our patients for assigning a reason for their diseases, no evident cause can be detected. They grow for some time before they are discovered, and no pain, nor redness, nor other sign of inflammation, is perceived; they may increase to 20lbs. weight, and yet no symptom whatever of inflammation is shown during the whole of their steady growth. They produce pressure on the surrounding parts, and its usual effects, condensation of the cellular texture, and consequently, in most cases, a cyst, more or less perfect, around them; absorption of the neighbouring substances, even of the bones which are in contact

with them in the interior of the body; stretching of the skin which covers them, and attenuation of its substance, till it at last gives way by ulceration; and yet, perhaps, no inflammation will have been brought on.

Generally, however, there is a period in the growth of every tumor when inflammation is established, and at this time a sudden increase of the growth of the tumor is observed with all the usual signs of inflammation, and the occurrence of further changes in the new textures—ulceration, suppuration, and sloughing. The disposition to such alteration depends partly on the original texture of the tumor; it takes place early, and pretty certainly in cancer, for instance, while a fatty tumor may have shown no tendency to inflammation when it weighs 40 lbs. It depends, however, partly on the situation of the tumor, and its power of obtaining skin from the contiguous parts. A tumor on the hand would, therefore, generally ulcerate sooner than one on the thigh or shoulder; and one behind the jaw can scarcely gain so large a size without ulcerating as another situated low down in the neck. You must not, on this account, reckon ulceration over a tumor as a certain sign of its malignancy, or of its fatal tendency.

However they are originally formed, tumors are supplied with blood from the vessels around them; sometimes one or two arteries only enter at the base of the tumor, and it increases uniformly towards the surface; at other times vessels enter the tumor in every direction, and every cut of the knife, in an operation, induces great hæmorrhage. Attempts have been made by Maunoir, and other persons, to cause the destruction of tumors by tying the principal vessel which supplies them, and thus deprive them of blood altogether, or to such a degree as to stop their growth. The effect, however, has usually been only temporary, just as when this operation has been done for bronchocle; you close one vessel, but others very shortly increase in size, so as to supply the loss. Sometimes, again, changes are produced by the operation, making the tumor ulcerate and slough, from its possessing less vitality than the original textures of the body; but a successful result from this method of treatment is very rarely met with, and in most instances no difference at all is produced by it; the new action once established, goes on nearly uninterruptedly, since the original cause is uninfluenced by a diminution of arterial supply. The rapidity with which they grow is, however, very various. I have seen a tumor in less than ten weeks increase to a greater bulk than that of two persons' heads joined

together; while another, at the end of twenty years, may not be larger than a pea. Most tumors increase with moderate rapidity for a certain time, and then suddenly enlarge in a much greater ratio, so that a few weeks will make a greater difference in their size than as many preceding years.

The tendency to the formation of tumors arises in part from original texture; thus, glandular organs, such as the breast or liver, are more liable to them than the heart or lungs; but such a rule is not universal, for the salivary glands are scarcely ever thus diseased; the more vascular tissues, such as the skin, are also more disposed to the formation of accidental growths than simple parenchyma.

I have explained to you the meaning of analogous and heterologous tumors. Mr. Hunter's definition of a tumor is, "a circumscribed substance produced by disease, and different in its nature and connexions from the surrounding parts." Now this is true—but in a limited sense. For it is curious that most tumors are assimilated to a certain extent to the organ or tissue in which they reside; thus the same cause in action upon the bone will produce some form of bony tumor, which, influencing the adipose tissue, will cause the new structure to assume the nature of fat. Thus it happens, that certain structures are confined to certain situations; polypi to the mucous membrane; epulis to the gums, and so on; so that I shall leave these special diseases to be described when we speak to you of the diseases of such parts in subsequent lectures. So, also, many of the analogous formations are so from their situation, such as this mass of bone and cartilage which was attached to the femur; but in other cases their structure does not depend on any local cause, such as this cartilage from a gland, or this mass of bone from an ovarian cyst. The heterologous formations are more universally alike, and present nearly the same structure wherever they are situated, whether in the breast or arm. Whether you examine a fungus hæmatodes tumor in the lungs or kidney, or brain, there is no great difference; it is not only unlike the texture in which it grows, but like nothing you will observe which is met with in any natural part of the body: such, for instance, is this melanosis of the lungs. Even these tumors, however, are modified slightly by the textures around them, and fungus hæmatodes of the bones, or of the lungs, presents some points of dissimilarity. Of course, two exactly similar structures will occasion various symptoms and appearances of form and size, according to the organs they reside in, and whose functions they interfere with, whether it

be the lungs or brain. I shall, therefore, confine myself at present to a general description of such tumors as will require, hereafter, a more minute account, from the several organs in which they may be met with; and most of what I have to tell you will apply to such formations wherever they may be found.

New structures situated in the internal organs for the most part affect the form of tubercles, and these are generally of a defined circular form, e.g. fungus hæmatoles, or scirrhus of the lungs or liver, in the cellular tissue of these organs, where they fall under the care of the physician; in external organs, on the contrary (with which we have chiefly to do,) from obvious reasons, such as pressure, and so on, tumors are more frequently irregular in figure, of indefinite size, and with various relations to other parts. Observe, for instance, this cancerous tumor of the breast, and these cancerous tubercles in the lungs of the same individual. These, however, are trifling distinctions compared with other circumstances. A great difference between analogous and heterologous tumors arises from the latter being frequently more universal in the several parts of the body; frequently appearing in several organs nearly at the same time, or in some parts in consequence of their formation elsewhere. With analogous structures, on the contrary, this is never met with; though they may be fatal, it is from local circumstances; and though they may have taken place in several parts at once, yet none are affected by absorption from the first.

This brings us next to the most important of all distinctions between different tumors; the separation of those which are innocent from those which are malignant. What then, you will ask, are we to understand by malignancy? Mr. Hunter says, a malignant disease is one in which "the destructive action overbalances the restorative;" and such is the common sense in which the term is employed; as synonymous, in fact, with an incurable ulcer. Thus lupus, or the corroding ulcer of the uterus, are often called malignant. But it seems to me that something more is necessary to constitute a malignant disease, than its being incurable, or proceeding rapidly to a fatal termination. I would limit the term malignancy to such diseases as are *incurable from their possessing a new formation, capable of generating the same or an analogous disease elsewhere*. This contaminating property is exerted in several different ways. 1st. The new structure produces a similar disease in the surrounding textures; 2dly, the absorbent glands are affected by absorption from the new substance, so that similar tumors are formed

in them; 3dly, both these influences may be going on at the same time; 4thly. Without any apparent alteration in the absorbent glands, the whole system becomes contaminated by the influence of the disease; doubtless by a poisonous effect upon the blood, so that tumors of the same kind as that first formed appear in other organs or distant parts of the body; or lastly, all these effects may be produced at the same period. By the limitation I have laid down, then, lupus cannot be considered as a malignant disease, since there is no new formation independent of the deposits of inflammation, and therefore the disease may be arrested or cured sometimes, and there is no contamination of the absorbent glands, nor of the whole system; even if half the face is destroyed by the ulceration, yet there is no evidence of any poisonous influence upon the constitution.

Mr. Lawrence says that "tumors which in their regular progress destroy life by the changes occurring in the affected parts, such as ulceration, bleeding, sloughing, or by causing similar productions in other parts of the body, more particularly in important internal organs, or by both together, are considered malignant." Now this definition also, you will observe, is less precise than I could wish, unless the word *or* were changed into *and*. A tumor is not malignant, in my sense of the term, unless, besides its power of destroying life by the local changes which take place in it, it is also capable of inducing contamination in other textures, either around it or in the glands connected with it, or elsewhere. Observe the broad distinction this makes in your practice; you remove a tumor which is about to become fatal, by bleeding, sloughing, and so on, but your patient is perfectly safe after the operation, and you have no fear of any return; but not so if it be malignant. There may be no evidence of contamination, and no ulceration or sloughing of the tumor, and yet the blood may be already poisoned, though you cannot detect it, and you cannot be at all confident that the patient will not die from tumors in other situations, of the same character as that you have entirely removed from the part first attacked.

It is, indeed, sometimes difficult to decide, from appearances, if a tumor be malignant or not—whether a crop of warts, for instance, are of the common kind or are cancerous warts; and in diseases really malignant, and apparently alike in appearance, there may be a considerable variety in the degree of malignancy.

1st. I have described a disease in the 19th vol. of the Medico-Chirurgical Trans-

actions, under the name of the verrucous tumor of cicatrices, which resembles in some respects the cancerous warts of the penis, but they are of a very different degree of malignancy; the former being so in the very lowest degree, semi-malignant, as it is sometimes called, affecting the neighbouring skin only with the same new structure, and not contaminating the glands, and therefore capable of being removed with almost a certainty of success; while, on the other hand, the operation is very precarious when performed for cancer of the penis.

2ndly. Cancer of the scrotum is a malignant disease in a more extensive sense, since it does contaminate the glands, and is followed by the formation of cancerous tumors elsewhere, and yet the system may be uncontaminated 40 years after the disease first showed itself. I have seen it thus in an old chimney-sweeper, who was in the hospital at the same time with his son for this disease, who had had a cancerous tumor removed three times, and yet not even then were the glands in the groin affected. So that in tumors even of this degree of malignancy an operation offers a very fair prospect of permanent success.

3rdly. Look to the progress of cancer of the breast, on the other hand, and you will almost always find the glands contaminated, and the whole system affected by the poison, so that an operation for cancer, which is malignant in this high degree, presents a very feeble chance indeed of a successful result.

And yet, 4thly. Even the most malignant tumors, which spread their contaminating influence in all three ways, seem to be very different from one another; some passing their whole progress in the course of a few months, while I have seen other cases in which several parts, including the glands, have been at once affected, and yet little suffering had been occasioned to the patient in more than 20 years from the commencement of the disease.

There is such great variety in the structure of tumors, that I can only lay before you the leading facts concerning them in the description which I shall attempt, and any kind of arrangement of them must be imperfect. The classification proposed by Mr. Abernethy, and which has been most followed, is founded on the internal structure of tumors, and it has been much objected to, because the diagnosis can only be verified after death. In a very recent work by Dr. Warren, an American author, he has adopted a classification founded on the organ or texture in which the tumor originated—tumors of the skin, muscles, arteries, and so on—which to me appears

much more objectionable, since the same kind of tumor must thus come under notice many different times, as they occur in the lungs, liver, or uterus, though they are exactly of the same nature, such as cancer, fungus hæmatodes, and such like, in each part. I must say I think a division founded on their anatomical nature is the best, which will also sometimes designate their origin. It is true we cannot be quite positive of the nature of a tumor till we have dissected it, or examined the whole body; but such is also the case with regard to ulceration of the bowels, or disorganization of the kidney; and yet, who scruples to speak of such diseases as if he knew their existence, and to give his prognosis accordingly? In the same manner I think, with care, we may generally be right as to the nature of tumors, if we attend accurately to the history of their occurrence and growth, to their form, colour, size, weight, solidity or fluidity, and the symptoms they occasion, or are accompanied with. To borrow the mercantile phrase, no one can pretend to be infallible, but I think we may generally be right, errors excepted. I shall select also, as far as possible, the terms already in use, though unfortunately the same term is sometimes employed in totally different senses: mammary tumor is, for instance, meant to designate an innocent tumor in the breast, by Sir A. Cooper, while others, following Mr. Abernethy, intend by this name to describe a kind of malignant fungous tumor like the breast, and situated any where. In fact, it is so difficult to describe accurately in words, or to delineate by drawing, the appearances of different tumors, that it is not easy to know what various pathologists are discussing in the classifications hitherto followed.

The class of tumors may be divided, then, into three orders:—

I. Encysted tumors.

II. Osseous tumors.

III. Sarcomatous tumors.

The latter term was invented by Mr. Abernethy, and is intended to designate not tumors resembling flesh, but tumors more or less solid, in which neither cysts nor ossific matter form the chief characteristic. Each of these orders, again, contains several genera, and some of these may be divided into distinct species, and occasionally we shall find more than one variety of the same species. But although some tumors possess so well-marked a structure, that their genus and order become at once apparent, yet all tumors are so far allied that the several structures are sometimes intermixed; some tumors, called sarcomatous, have cysts mixed with the solid substance, either surrounding it,

or connected with it; and so some osseous tumors are mixed with cysts occasionally, or with solid substances not osseous; and some genera are, of course, still more like one another, and pass insensibly into each other in appearance. We shall find that this circumstance is especially the case with malignant diseases, so that tumors belonging to two or three genera are often seen at once in the same person, or the same tumor may, in different parts, have a mixture of two or more kinds of structure—of cancer, with fungus hæmatodes or melanosis; or a tumor of one kind having been removed, of the nature of cancer, may, on returning in the same part, exhibit an alteration of its character to fungus hæmatodes, and *vice versa*.

Some tumors, lastly, may possess such distinct characters that their place is evident; others, on the other hand, may be of so ill-defined or uncertain a structure, that you may be obliged to place them under the head of tumors to which they bear most similarity, without being quite confident of their real nature. Any division that I could form, must, in fact, in the present state of our knowledge, be an approximation only to the truth, for the purpose merely of assisting your diagnosis, and guiding your practice.

I. The first order of tumors, then, is that of *encysted tumors*, by which I mean not merely the condensed cellular tissue that may give a covering to any tumor, or to an extraneous body, but a cyst which has a secreting internal surface, by which it is filled with various contents, for the most part fluid and unorganised. They may be divided into five distinct genera, that are not always sufficiently distinguished from each other in practice.

1. The most common genus should be called *sebaceous encysted tumors*; they are situated immediately under the skin, are globular, or oblong and flattened, elastic, seldom fluctuating (even when fluid), from the thickness and distension of the sac, but more elastic than sarcomatous tumors. The species are named from the nature of the contents.

a. Milicerous, or like a mixture of honey and wax.

b. Atheromatous, a half-fluid substance, unctuous, like pap in consistence, sometimes rancid and offensive.

c. Steatomatous, or more of the consistence of fat than the rest, and almost solid in appearance, till it is pressed out and found to be unorganised.

These three species, which are most common, are all of the same nature, and originate in obstruction of the sebaceous follicles, and the consequent retention and alteration of the natural secretion. The

cause of their formation was first ascertained, I believe, by Plenck, in his *Systema Tumorum*, and has more recently been pointed out by Sir Astley Cooper, not aware probably that it had been previously described. You will constantly see little black specks arising from partial obstruction, from which you can press out the thick oily substance designed to lubricate the skin. A gentleman, whose sister had died of cancer of the breast, came up to town from Oxford, and begged I would immediately visit him; and I found him in a great fright from the discovery of a blue tumor of the size of a nut on the skin of the abdomen, which he thought was cancer; but I was able to relieve his fears by opening the orifice with a probe, and picking out the hardened matter contained in the cyst, which was thus seen with its duct on a magnified scale. And if the opening is distinguishable, as it occasionally is in this condition, you may palliate the complaint by giving exit to the contents of the tumor from time to time. Sometimes the contents are different from those of the three species already mentioned.

d. Hair is occasionally found rolled up to a great length, and generally without the bulb of ordinary hair, though formed, no doubt, by the hair having been obstructed in its passage through the cutis, at the same time that the atheromatous sebaceous matter with which it is generally mixed became confined.

e. At another time horns are formed by the hardened sebaceous matter projecting through an enlarged follicle or ulcerated opening, to the length of several inches, occasionally by the recently secreted matter pushing out that which had previously been formed. These horny sebaceous tumors are often seen on the forehead, where no one, I presume, would wish to exhibit them openly, however quietly such appendages are sometimes worn in private.

f. Sometimes, again, the interior of the cyst ceases to secrete, and becomes lined with cuticle, so that a large orifice is thus left leading into the cavity of the former tumor, below the level of the skin.

Sebaceous encysted tumors are, of course, most common where the follicles abound, especially on the head and face and back. Numerous hard little bodies are met with on the penis, and in the eyelids, which originate in the same way; and there are often a considerable number in the same person, as in this plate of Alibert's*, or in a patient of Mr. Keate's, whom you have lately seen operated on.

* Most of the circumstances described in these lectures were exhibited by preparations and drawings; but, to avoid repetition, we shall not often allude to them.

These tumors often grow to a considerable size, and exist many years, before they occasion such pain or inconvenience as to lead to any surgical assistance being applied for. When large upon the head they will sometimes produce headaches, and by their pressure have more rarely still led to the absorption of part of the bone below them. Occasionally, also, when rubbed by the hat or otherwise irritated, they inflame and increase in size; and then the fluid is partly absorbed again, and they return to their former bulk; or the orifice opens, or ulceration ensues at the apex, and the contents are partly discharged, as from an abscess, but with a small quantity only of pus mixed with the greasy secretion of the cyst. Sometimes there is more severe inflammation still, and the sac sloughs away, and the ulcer heals, so that a natural cure is effected.

It is wrong, however, to allow this, if the removal of the tumor is permitted, as the ulceration is sometimes very troublesome, and produces severe irritation. It is wrong also to attempt to imitate this process, and to open the cyst with a lancet, or pass a seton through it, or destroy it with caustic. If, indeed, the tumor is small and uninfamed, and the patient refuses to have it removed by the knife, you may puncture the cyst, and let it heal, from time to time.

But the removal by the knife is the best and safest method of treatment, before the tumor has attained any great size, and before inflammation and ulceration have taken place.

The incision is generally very easy, when the tumor is of a moderate size, if it is done in this manner:—Make an incision through the skin rather longer than the diameter of the tumor, down to the surface of the cyst, but without wounding it; then with your thumb-nails press back the skin on both sides from its surface, and insinuate them under it, when it will start out like this, which I thus took away from the head of a lady; or this larger one, which I have subsequently opened to shew the cyst. There is little bleeding, and the incision heals by the first intention, so that you need not with small ones have the hair shaved off, but make a little pressure with lint after oozing has ceased. A large one will more probably suppurate, so that you have to open the wound again; and therefore it is better to shave the hair previous to the operation. If you accidentally open the cyst, then squeeze out the contents, and with a pair of forceps tear out the cyst from its cellular bed, the edges of it being easily seen, and if the cyst is thin or large, the best way is not to try and remove it whole, but cut at once through the skin and the front half of the

cyst, and then drag it out in the same way. If the skin be adherent, or the cyst has inflamed so as to adhere at its base, which is less common, a little dissection becomes necessary; but take especial care not to cut into the occipito-frontalis tendon, lest you produce a troublesome and dangerous form of cellular inflammation. With this tumor, which I removed after a very successful operation for cataract, in an old man of 75, I was unlucky enough to have an attack of erysipelas, which you must sometimes expect, and endeavour to guard against, by looking to the condition of the system before this apparently trivial operation.

In the cheek or temple the cyst is almost always very thin and transparent, so that you need seldom try to dissect out the cyst entire, but divide it partly in the way I before mentioned; or if it is very small, you may divide the skin, and raise the tumor with a tenaculum, and cut it out along with a little fat around it; recollecting, of course, the situation of the parotid duct, and facial nerves.

When the part has inflamed and suppurated, you may lay the cyst open; and if it adhere so as to render the dissection difficult, you may rub the interior slightly with potassa fusa, or nitric acid, and leave it to slough. The great point in any plan of treatment is to leave not the least portion of the cyst behind, since it will then return again. A gentleman, for instance, had an encysted tumor removed in Dublin, and it came again in the cicatrix a few months afterwards; it was of small size indeed, but being on the back of the neck it inflamed after hunting one day, and I dissected it out while suppurating, notwithstanding which the wound healed by the first intention. When situated on the back or limbs, pressure will almost always have produced adhesion, so as to render dissection of the skin from it necessary.

RECOLLECTIONS OF CHOLERA;

ITS NATURE AND TREATMENT.

BY WILLIAM GRIFFIN, M.D.

Limerick.

Treatment of Cholera.

No. VII.

THERE has been, as we have seen, a very general agreement in the experience of all countries with respect to the natural course of cholera, and an almost universal admission that its ter-

mination is always fatal when it appears as an epidemic, unless medical treatment is resorted to. The recoveries, it is true, which occasionally took place in this country, sometimes under the most inert, sometimes under the most contradictory treatment, seemed more reconcileable with the supposition that nature had, in these instances, effected the cure, than that any disease could be amenable to so many different plans of management. But this difficulty may be explained by simply retracing the natural progress of the disease through its consecutive stages. Cholera has always, at least in this country, been preceded by premonitory diarrhoea*, which, if left to itself, ran, in the majority of cases, into the confirmed disease, characterized by rice-water vomiting and purging. When once this confirmed disease commenced, it invariably ran on to pulseless collapse if unimpeded by remedies, but in the great majority of instances was arrested under judicious treatment. When once collapse had taken place, and the pulse had ceased at the wrist, all treatment was apparently useless, and sometimes worse than useless. If the patient was let alone, reaction took place by the unaided efforts of nature in from three to five cases out of ten—that is, from five to seven died in the state of collapse, while from three to five revived and ran into consecutive fever. In those who had, previous to the cessation of the pulse, been judiciously treated, the state of collapse was generally less deep or prolonged, and a greater number of such persons (probably seven out of ten) revived, to encounter the after-dangers. This was the state to which, from the frightful phenomena it presented, public attention had been chiefly rivetted, as if it formed the whole of what was called cholera; the state in which the most inert or the most opposite modes of treatment appeared to be equally successful; and after all our experience of the disease, it remains to be proved what plans or remedies, or whether any, possess an effective influence in altering its natural results up to the period of reaction.

When reaction had commenced, when consecutive fever had begun, the patient became again susceptible of the influence of remedies. These could not, perhaps, effect much, but they effected

something, and without them *all died*. There were many natural recoveries from collapse—that is, from three to five revived out of it, unaided by any remedy, but there was no natural recovery from consecutive fever,—I mean while cholera was epidemic. To sum up the natural history of the disease, all cases of cholera, not subjected to treatment, run without exception into collapse; out of the collapse, three, five, or perhaps seven, revive and run into fever; and if no judicious treatment be still instituted, all these die. There is, therefore, no recovery from cholera when epidemic, unless medical treatment be resorted to.

I cannot help thinking, that the variety of opinions as to the proper treatment of this disease, the diversity of remedies, the indefinite and perplexing conclusions to which all our experience has led, may be attributed almost as much to the scientific zeal and efforts at unattainable perfection in a very talented portion of the profession, as to the indiscriminating empiricism of the remainder. All the pathological investigations which have taken place, either in India or in Europe; all the minute examinations of morbid appearances in the military hospitals at Calcutta or Madras, or at the Hotel Dieu in Paris, or at the Drummond-street hospital in Edinburgh, while they speak highly for the industry and zeal of the medical men engaged in them, have contributed less to any immediate improvement in practice, than some of the simplest prescriptions which have reached us from the unlettered Hindoos. It is far from my wish to slight or undervalue the labours of zealous men, prosecuted at no common sacrifice; I believe those labours may hereafter bring forth abundant fruit for science, and that in one instance at least they have already led, by chemical examination of the blood, to the application of a remedy as successful as it was daring—the injection of the veins with saline fluids. But in new and obscure diseases, implicating almost every part of the system, it is not to be expected that pathological researches can lead to satisfactory inferences in one age or country, or that we can turn to any successful account the mere conjectures to which they give rise. Our practice in such cases must be pure empiricism—not so to say in the ordinary use of the term, but empiricism founded on

* The Cholera of French writers.

analogy, and applied with skill and discrimination. It is on such grounds, in fact, that half the therapeutic doctrines of the present day, which are looked upon as most conclusive, are based; and even those which are strictly connected with pathological discovery, and appear to have grown out of it, will be found, I apprehend, on inquiry, to have for the most part preceded the connexion. Empirical inferences are always of immediate practical application, while those deduced from morbid appearances, at least in diseases which either originate in the nervous system, or are protracted in their course, or affect many organs simultaneously, are of the most doubtful and suspicious character.

I have offered these observations from a conviction that if half the enthusiasm evinced by scientific men in studying the pathology and originating theories about the nature and cure of cholera, was directed to determine the treatment, by careful and logical deductions from the empirical method, we should long before now have arrived at some definite system, and not be left as we have been, should the disease again visit us, uncertain, perplexed, and as liable to be "blown about by every wind of doctrine," as when our experience began.

In reviewing or comparing the chief remedies, or most popular means of cure, which have been resorted to, I regret to say I can only give a partial illustration of the method by which we might arrive at some degree of certainty in our inferences. It is a very imperfect adoption of the numerical method of induction before alluded to, which has been so admirably applied to the investigation of the proper treatment of thoracic inflammation by M. Louis. At the time cholera commenced in this country, I had not seen the account of M. Louis' method, and was led even to the very imperfect adoption of it which I shall describe, by the utter discrepancy of medical opinion with respect to the value of every proposed means of cure, and a consequent anxiety to inquire and determine for myself. The results of this inquiry, imperfectly as it was made, were beyond my most sanguine expectations, and left no doubt on my mind, that if it was similarly pursued in determining the merits of other remedies, as it had been in fixing the pretension of calomel, we should attain

a confidence in our treatment not surpassed by any we possess in the management of more familiar diseases.

In considering the effects of the several remedies which have been employed, it will be convenient to group such of them in classes as seem to be allied in their mode of action. The consideration of sedatives or contra-stimulants, stimulants, emetics, purgatives, astringents, salines, simple diluents, &c. necessarily includes an inquiry into the merits of every reputed specific, and presents readier and more advantageous opportunities for correct comparison. It will also be requisite to discuss the value of every remedy, in reference to its influence in the several stages of cholera, which I have distinguished into three, beside the premonitory one. The latter, or attack of bilious diarrhœa, though almost invariably preceding the disease, and apparently essential to its occurrence, forms no real part of it. There is nothing in its slighter visitations to distinguish it from mild diarrhœa occurring from any other cause, or in other seasons, nor, in its worst attacks, to separate it from English cholera; and when present during the prevalence of the Asiatic disease, it is not, in the majority of instances, followed by it. It is well known, that in every city where the latter has been epidemic, bowel complaints have been general; and yet the proportion of any population which has fallen into cholera under such circumstances has rarely exceeded a twentieth of the whole. The commencement of epidemic cholera should be dated from the occurrence of rice-water vomiting and purging, with suppression of bile, and perhaps urine. This is the first or true primary stage of the disease, and it is, sooner or later, unless interfered with by the influence of medicine, succeeded by another — the second, or stage of collapse, equally well marked in *the absence of the pulse at the wrist*. I have fixed on this symptom of *absence of the pulse* as characteristic of *collapse*, to get rid of the indefinite notions attached to that word by some, and to engage medical men in the use of terms in the publication of their reports, about the precise application of which there can be no doubt. The collapse is followed by a third stage, or that of consecutive fever; distinguished as essentially from the second as either is from the first. These are

the distinctions to which I have adhered in the previous reports, when discussing the mortality of the disease; and the reader will readily perceive, not only the advantage of considering the influence of remedies separately, with reference to each, but the total impossibility of forming a comparison of any one medicine, or plan of treatment, with another, without the strictest observance of them. In the distinctions drawn by Dr. Mackintosh and others, the premonitory is regarded as the first stage of cholera, and their second, or stage of collapse, is strangely enough made to include the true primary stage above described, though the latter often exists for many hours without an appearance of those symptoms which characterise collapse. From reports drawn up with such vague distinctions, in which every medical man is at liberty to class cases of premonitory diarrhœa with true cholera cases, and those with firm pulses, but who have a little blueness or sinking about the eyes, with true cases of collapse, what can one learn, or what compare, or infer? The value of the distinctions I have drawn will be more fully appreciated in the sequel.

It would be needless waste of time to make any particular reference to the effect of remedies in the premonitory or simple diarrhœal stage. The usual treatment for the suppression of ordinary diarrhœa, or that recommended in the circulars issued by the Board of Health, or, still more simple, the exhibition of crude opium in grain or two-grain doses, is always capable of controlling it; and, as far as my experience has gone, I believe the lapse of simple premonitory diarrhœa into spasmodic cholera is always attributable to the neglect or mismanagement of either patient or doctor, and not to the inefficiency of medicine. The pure opium treatment may not be strictly applicable to every case, but it is so to the great majority; and the ill consequences that follow its use, when misapplied, do not, at all events, include the occurrence of cholera. The misapplication of purgatives, on the other hand, however mild they may be, is frequently irremediable; the patient, after their operation, running rapidly into collapse before we can arrest the mischief. The use of purgatives when cholera is threatened, requires, indeed, at all times the utmost caution; nor are they often required

even in those instances where the opium treatment has confined the bowels for two or three days, their natural action eventually taking place before any disturbance of the system is occasioned. I am most confident, that if the public at large was made familiar with the use of opium wherever cholera was present, and it was customary for each person to carry a piece in his pocket, so that no time should be lost in suppressing attacks of diarrhœa, more would be effected in arresting the progress of the epidemic than by the most expensive hospitals, or the most rigid sanitary regulations. It is, in fact, a question with me, whether those who are apprehensive, or constitutionally disposed to the complaint, might not take small doses of opium daily, with advantage, as a preventive, in the same manner as belladonna has been used to prevent attacks of scarlatina.

[To be continued.]

SOME OBSERVATIONS ON THE PATHOLOGY OF STAPHYLOMA.

To the Editor of the Medical Gazette.

SIR,

THE term *staphyloma* has been applied, by writers on the diseases of the eye, to various prominences on the surface of that organ, produced by a partial yielding of its coats. But that form of projection to which the name *staphyloma* was originally given, and for which it is still most commonly employed, occupies the place of the cornea, and is, according to the opinion of all authors (I speak under correction), the cornea itself, degenerated and distended, together with the iris, which has become adherent to it throughout its whole extent; the anterior chamber being, of course, annihilated. The following words of Mr. Lawrence * express, I think, very clearly and fairly, the generally received opinion as to the mode of origin of *staphyloma*:—"In order that the state of *staphyloma* should be produced, there must be severe inflammation of the eye, involving the entire cornea, and rendering its texture opaque; that inflammation must also have extended to the iris, and caused it to adhere to the cornea; and there must be increased

* Treatise on the Diseases of the Eye, p. 373.

secretion of the aqueous humour, to push these unnaturally connected structures forwards."

This form of staphyloma is usually called *spherical staphyloma of the cornea*, and it may be either *partial* or *total*. In the following remarks, every other form of staphyloma—such as conical staphyloma, staphyloma scleroticæ, &c. is excluded. What is known by the name of *staphyloma iridis*, will be shown to be merely a stage of the disease under consideration.

The structure of the cornea is changed, the iris adheres to it, surface to surface, and the two together are distended into a staphylomatous projection. Such, in a few words, is the commonly received opinion regarding the formation of staphyloma. I have examined many cases of inflammation of the eye—inflammation of the cornea, inflammation of the iris, inflammation of the membrane of the aqueous humour, &c.; but never observed adhesion between the two membranes take place in the manner described. In penetrating ulcer of the cornea, and prolapsus iridis, of course the prolapsed part of the iris adheres to the cornea at the one point, but though the two membranes have come into contact in their whole extent, in consequence of the escape of the aqueous humour through the opening in the cornea, made by the penetrating ulcer, and though, in consequence of the attending inflammation, the circumstances favourable for adhesion exist, still the iris does not adhere to the cornea surface to surface, as the pleurapneumonalis does to the pleura costalis; on the contrary, as soon as the ulcerated opening in the cornea closes, so that the aqueous humour is allowed to accumulate, the iris recedes from the cornea, and the anterior chamber is re-established. The only adhesion between the iris and cornea is at the one point where the prolapsus iridis took place. Again: cases of penetrating ulcer of the cornea occur in which, the aqueous humour having escaped, and the iris come into contact with the cornea, but no prolapse of the former having taken place, no adhesion at all between the two ensues, but the iris recedes from the cornea as soon as the opening in it from the ulcer closes, and prevents the farther escape of aqueous humour. One should suppose that if adhesion between the

anterior surface of the iris and inner surface of the cornea was a thing prone to take place, that it should have been observed in those cases of inflammation of the membrane of the aqueous humour in which the operation for evacuating the latter has been performed; but I apprehend no one ever saw such an event ensue. In the operation for the extraction of the lens in cataract, the aqueous humour having escaped, the iris comes into contact with the cornea, and continues so for a considerable period; but if there be no prolapsus iridis, adhesion never takes place. If the iris does not adhere to the cornea when these two parts come into contact, from the escape of the aqueous humour, and while in a state of inflammation, much less do they adhere, independently of the removal of that fluid*.

But it may be said, it is not in consequence of such inflammations of the eye that staphyloma occurs, but that "purulent, gonorrhæal, and variolous ophthalmia, are the most frequent causes of staphyloma." True; but under what conditions does staphyloma occur in those diseases? Not by adhesion of the iris to the cornea, surface to surface. Mr. Wardrop says, "as far as I have been able to observe, staphyloma never occurs unless the cornea has been previously ulcerated, and unless the ulcer has penetrated into the cavity of the aqueous humour and destroyed the cornea as deep as the internal tunic." I apprehend it would have been nearer the truth to have said, that staphyloma never occurs without more or less extensive destruction of the cornea. Purulent, gonorrhæal, and variolous ophthalmia, are the diseases of the eye, in consequence of which staphyloma most frequently occurs, and these are the very diseases in which the cornea is more or less extensively destroyed.

The only mode of formation of staphyloma which I could ever trace is the following:—

If, in scrofulous, catarrhal, or catarrho-rheumatic ophthalmia, there be a penetrating ulcer of the cornea, the aqueous humour escapes, the iris falls forward into contact with the cornea, and a small part of it is perhaps prolapsed through the ulcerated opening. The progress of the ulceration being stopped by the yielding of the

* Middlemore on Diseases of the Eye, vol. 1, p. 512.

inflammation, the prolapsed portion of iris, and the ulcerated part of the cornea, are involved in one cicatrice. The opening in the cornea being thus closed, the aqueous humour again collects, and the anterior chamber is restored, though somewhat diminished, in consequence of the partial adhesion between the iris and cornea (*synechia anterior.*) There is no prominent distension on the front of the eye in this case, because, as the inflammation subsides, the small protruded portion of iris shrinks and flattens; but if the destruction of the cornea has gone on farther, either by ulceration or the giving way of an onyx, and considerably more of the iris has protruded, the prolapsed portion of the iris does not shrink when the inflammation begins to abate, as in the former case, but remains, and forms a projection at one part of the cornea, generally the lower or lateral. This projection is at first merely a bag of the iris distended by the aqueous humour; but by and by its exposed surface becomes covered by an opaque firm tissue, of the nature of the *tissue of cicatrices*, and this tissue is incorporated at the base of the tumor with the sound cornea. The projection, the mode of origin I have just described, is a *partial staphyloma*; it is not a distension of the cornea itself, but a protruded portion of the iris covered by a *new tissue*, intended to supply the loss of substance which the cornea has sustained. The mode of origin of a *total staphyloma* is essentially the same, but differs only in degree. The whole or greater part of the cornea being destroyed, as occurs in gonorrhœal, purulent, and very often in variolous ophthalmia, as also that of new-born infants, the whole iris falls forward, the pupil becomes closed, and the aqueous humour being thus allowed to accumulate in the posterior chamber, the iris is kept distended in the form of a tumor on the front of the eye. Its surface gradually gets covered with an opaque cicatrice-like tissue, or pseudo-cornea, which assumes a greater or less degree of thickness, and a total staphyloma is the result. Sometimes the central part of the cornea only is destroyed, a ring of the circumference still remaining; the staphylomatous projection has then the form of a small globe stuck on the front of a larger.

In 1834, when in Glasgow, I directed the attention of Dr. Mackenzie to the subject, and he adopted my views so

far as to admit my description of the origin of staphyloma into the second edition of his work, then in preparation (p. 602). Subsequent and more extensive experience and observation have convinced me more of the correctness of my views, and induce me to speak more decidedly on the subject than I did at that time.

Founding my reasonings on those views, I conceived that the supply of aqueous humour in the still-existing posterior chamber was what kept up the distension of the iris, and the consequent moulding of the pseudo-cornea on its surface in the form of a round prominence on the front of the eye. If, therefore, it was natural to infer the source of the aqueous humour could be destroyed, we should not have the development of the staphylomatous projection, or, if already formed, it would disappear. For this purpose puncturing the tumor is not found to answer well. To break in upon the integrity of the posterior chamber, I conceived the simplest and most effectual plan would be to extract the lens,—an operation which I put into practice in the following case.

A young man about 22 years old came to me labouring under the effects of severe purulent ophthalmia of both eyes. In the right eye I found the cornea destroyed, and the iris protruding and distended with aqueous humour, the pupil being closed. The left eye had also suffered very much; there was penetrating ulcer, prolapsus iridis, and consequently considerable distortion and contraction of the pupil. Both eyes were still affected with the inflammation, and it was very doubtful whether the left eye could be prevented from getting worse, especially as it was evidently kept in a state of additional irritation from the presence of the staphyloma in the right. By an incision with a Beer's cataract knife in the protruding and distended iris, the lens was extracted. Severe re-action followed; less perhaps in consequence of the operation, than in consequence of the patient not having been in a situation to take proper care of himself. The iris did not again become distended; on the contrary the eye shrunk, and irritation being thus removed, the left eye progressively improved, as far as the organic changes it had already undergone allowed, and farther than there had been previously reason to hope for, as vision was pre-

served sufficient to enable him to resume his employment as a porter.

In those cases in which the eye is destroyed by purulent ophthalmia, whether of adults or new-born infants, gonorrhoeal ophthalmia, variolous ophthalmia, &c., and in which staphyloma does not form, but the eye-ball shrinks and becomes atrophic, I suspect (though at present I cannot adduce any direct proof) the lens has escaped on the giving way of the cornea.

The conclusions, then, which I draw from my observations are the following:—

That the iris and cornea do not unite surface to surface, and if they unite at all it is only partially, and that in consequence of penetrating ulceration of the latter, and prolapse of the former.

That the tissue composing a staphyloma is not degenerated and opaque cornea, but a *new tissue*, of the nature of the tissue of cicatrice, developed on the anterior surface of the iris exposed by the destruction of the cornea itself.

I am, sir,

Your obedient servant,

T. WHARTON JONES.

30, Newman-street, Oxford-street,
17th February, 1835.

PLASTER OF PARIS IN FRACTURES.

To the Editor of the Medical Gazette.

SIR,

In my note-book I find the following extract; it must have been written many years since, as I have no recollection from whence I made it. The plan has frequently presented itself to my mind as worthy of trial, and I am happy to find my opinion, that it might be turned to good account, verified by Mr. Sweeting. The extract is headed, "Arabian Mode of Curing Fractured Limbs."

"The Orientals will never consent to have a limb cut off. Their practice is to lay the limb upon an oiled mat, after reducing the bones, and then inclose it in a case of gypsum, or plaster of Paris; an operation which they perform much in the same way as is practised by statuary to take the cast of a limb. They first pour the plaster of Paris under the limb, until it rises to such a height as to touch the whole lower sur-

face, filling up all inequalities, so as to form a sort of bed, placing at the same time a few hollow reeds at proper distances, and in such positions as to serve to conduct away through the plaster any fluid that might collect in the gypsum case, from the wounds, &c. When this becomes firm, which it does in a very short time, the limb is next covered with the same plaster of Paris, so as to inclose it completely, and, on hardening, to form a light case or plaster boat, to keep the parts in as natural a position as possible. They next make a sort of furrow or channel in the soft plaster, on the upper surface, to receive such vulnerary fluids, during the treatment, as they think conducive to the cure, and which filter through the gypsum to humect the leg at pleasure. To render this upper shell more easily removed or changed during the cure, if necessary to examine the state of the parts, &c., they make deep incisions into the soft plaster, both lengthwise and across, though not quite through, by means of which the upper case is removed without disarranging the limb. The firmness of the lower part, or bed, makes the removal of the whole boot practicable, should such a measure at any time be found expedient."

I have not seen Mr. Beaumont's pamphlet, and he may have possibly made mention of the practice being followed among the Arabs and eastern nations: if, however, it has not been noticed, it may probably be interesting to him and Mr. Sweeting to read the above.—I am, sir, with much respect,

Yours obediently,

THOMAS INGLE.

Lyme Regis,
Feb. 7th, 1835.

CASE OF BLACK VOMIT IN AN AFRICAN.

To the Editor of the Medical Gazette.

SIR,

In reference to some valuable remarks on the "Malignant Fever of British Guiana," by Dr. William Fraser, contained in a late number of your journal, I would request your insertion of the following case, one of many which no doubt can be produced in answer to that part of Dr. Fraser's letter where, in

speaking of Africans, he says, "I have heard it stated that the black vomit is unknown among the sable race; but whether this is true or not I cannot, from my own experience, attest or contradict*.

Your obedient servant,
J. MACDIARMID.

February 7, 1838.

Sierra Leone, 1837. — J. Richards, African private, 2d West India Regiment, was admitted into hospital May 11th, with slight febrile symptoms; bowels constipated; tongue furred. On the 13th, reported as "making no complaint;" skin cool; bowels regular; tongue, however, still furred. On the night of the 15th, patient was suddenly seized with vomiting of coffee-ground matter, mixed with blood. Morning visit reported, "Patient in a low typhoid state; pulse weak and thready; extremities cold; smell of the body cadaverous." Lingered till 10 A.M. of the 16th.

Sectio cadaveris, four hours after death. — Conjunctiva deeply tinged with bile. Thorax: Lungs on the right side adhered extensively by pretty strong bands to the parietes; superior portion of the right lung, and numerous patches of both, presented a dark spotted appearance, and, when cut into, sanguineous engorgement in a great degree, fluid blood flowing out profusely without the slightest pressure. A small quantity of serum in the pericardium. Abdomen: Liver about the natural size, pale, and somewhat condensed in structure; tinged yellow. Gall-bladder contained an ounce and a half of a dark fluid resembling liquid tar. Stomach filled with a dark fluid to the amount of a pint, similar to that vomited before death; portions of its serous surface, especially towards its cardiac extremity, dark and congested; the lining membrane in the same situations corresponding in appearance. Spleen slightly enlarged, dark, and semifluid, giving way under the finger. Bladder contained a pint of fluid similar to that found in the stomach, but thinner. Intestines generally presented nothing remarkable. Membranous textures of the body throughout deeply tinged with bile.

ON THE
CEREBRAL EXTREMITY OF THE
OPTIC NERVE.

To the Editor of the Medical Gazette.

SIR,

I AM desirous of putting upon record, through the medium of your valuable journal, a fact regarding the cerebral extremity of the optic nerve, which I do not find noticed in any author on this subject. It may be interesting to many, inasmuch as it appears to afford additional evidence of the important office of the cineritious neurine; especially when taken in connexion with those views of the function of the grey matter which Mr. Grainger has lately so materially elucidated by his minute dissections of the central extremities of the spinal nerves, and the deductions which he makes in confirmation of Dr. Marshall Hall's views of the spinal cord as a true centre of power. I will also take this opportunity of bearing my testimony to the accuracy of Mr. Grainger's statement regarding the roots of the spinal nerves.

I traced fasciculi of both the anterior and posterior roots of the spinal nerves, into the grey matter of the cord, in presence of Dr. Todd, at King's College, on a dog which he had procured for that purpose; since which, I have seen them exposed by Mr. Grainger himself, at his school in Webb-street.

The connexion of the optic nerve with the brain itself has been variously described by different authors; by some it has been described as arising only from the thalami nervorum opticeorum; by others, only from the optic tubercles; and by most, in latter days, from both of these bodies, "by a flat band of white fibres*." These fibres, which come from the surface of the thalamus and the optic tubercles, are well known and easily demonstrated; but they are not the only fibres of communication between this nerve and the brain.

If the optic nerve is carefully traced from its commissure backward, it will be found to be connected to the tuber cinereum, as described elsewhere; after crossing the crus cerebri, to which it is connected with membrane, it divides into a superficial and deep layer: the superficial layer is that which is

* MEDICAL GAZETTE, Jan. 20, p. 641.

* Mayo's Physiology, 4th edit. p. 450.

described in most anatomical works; the deep layer, which is thin and flat, plunges partly into the substance of the thalamus and partly into the corpus geniculatum. Those fibres which go through the corpus geniculatum are separated into delicate threads by cine-ritous neurine, as the motor tract of the spinal cord in the corpus striatum. These fibres which plunge into the thalamus are stronger and more distinct, and after spreading into rays are lost in its substance.

This arrangement may be seen after raising the nerve from the crus cerebri, either by tearing its fibres very carefully in a brain previously hardened in alcohol, or by making a longitudinal perpendicular section of the optic nerve in a recent brain, right through the corpus geniculatum and thalamus; when one layer of white neurine will be seen on the surface of the corpus geniculatum, and another just passing through its anterior part, but principally through the substance of the thalamus, separated from the first by the posterior and superior portion of the grey matter of the corpus geniculatum.

I remain, sir,

Your obedient servant,

SAMUEL SOLLY.

No. 1, St. Helen's Place,
Feb. 11, 1835.

MEDICAL GAZETTE.

Saturday, February 24, 1838.

"Licet omnibus, licet etiam mihi, dignitatem Artis Medicæ tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

DR. ELLIOTSON

ON THE PROPOSED

LECTURES ON PATHOLOGY.

WE suppose that a certain portion of *amour propre* is common to all men; but in some the quality is so superabundant, that it betrays itself when they least intend it, and often when they are quite unconscious of it. The learned Professor of Medicine in University College stands in this predicament. In his edition of Blumenbach, recently published, he lays claim to almost every important improvement

made in physic during his time; and in a printed letter now before us, he modestly expresses his opinion that all teachers of medicine should be made by authority to take his lectures as their model.

The occasion which has given rise to this very curious document is the real or supposed intention of the University of London to insert a course of pathology in the curriculum, for the guidance of aspirants to degrees in physic. Dr. Elliotson disapproves of this, and so do we; but the worthy professor, in adopting the arguments already repeatedly urged by us, contrives to place them in such a light as to make the measure against which he argues appear to be imperatively required. He informs the Senate that he teaches medicine in such a manner as to render a separate course on pathology and morbid anatomy unnecessary; but as he more than implies that this mode of proceeding is peculiar to himself, his argument goes to shew that a change in the method of teaching generally adopted is absolutely requisite. The medical Faculty of the London University have no power to compel lecturers on medicine to teach morbid anatomy; and thus Dr. Elliotson's epistle, so far as it may be supposed to influence them at all, is calculated to confirm them in the propriety of remedying the supposed evil in the only way which is open to them—namely, by decreeing that aspirants for their honours shall have attended an *ex professo* course upon the subject in question; a proceeding which they are well aware would be followed by the announcement of abundance of such lectures.

That there may be no mistake, however, as to Dr. Elliotson's mode of conducting either his course or his argument, we shall give his own account of it:—"I commence," says he, "with an account of what is meant by disease." "I examine the nature and operation

of the various causes of disease in general." "I then proceed to the history, nature, and treatment of general diseases." "I arrive at the diseases of individual parts." "I proceed through the head, chest, and abdomen." "I illustrate by individual diseases all the general principles." "I exhibit all the morbid preparations," &c. &c. &c. All this information, with *I, I, I*, in the van, would naturally lead any one to suppose that the plan described was something absolutely novel, and totally different from the system adopted by others. Now, from the document before us, we have no doubt that Dr. Elliotson is firmly persuaded of this, and implicitly believes that no one in the three kingdoms but himself knows that a course of lectures on the theory and practice of medicine ought to comprehend the pathology and morbid anatomy of the diseases treated of. The same general observations have been made in his introductory lectures, and on a variety of other occasions, by the learned Doctor, but perhaps never so very broadly as now; for all this preliminary setting forth of methods generally adopted, as if they were peculiar to himself, is followed up by the modest and unassuming recommendation to the Senate of the University, as the simplest mode of getting over all their difficulties, "*That it should be declared that the course on the principles and practice of medicine must comprehend the subjects treated of in mine*"!!! Such are the words contained in a printed letter addressed to the Medical Faculty of the London University, bearing the signature—J. ELLIOTSON*.

The simplicity with which this re-

* The sentence, as printed in Dr. Elliotson's letter, contains a parenthesis, which we have omitted, because it separates the first and second clauses too far from each other. It runs thus:—"That it should be declared that the course on the principles and practice of medicine (which is, in truth, a course of general and special pathology in all its branches of pathogeny, etiology, semeiology, and therapeutics), must comprehend the subjects treated of in mine."

commendation is made cannot fail to be appreciated by all; but we take leave, in justice to other lecturers, to add, that it is not necessary for the Medical Faculty to issue the proposed mandate; and for this reason—that the courses of lectures on the theory and practice of physic, delivered in London, and probably every where else, with few if any exceptions, do already contain what Dr. Elliotson so gravely recommends as something new.

As to the general question of the expediency or in expediency of establishing separate lectures on Morbid Anatomy, we take the opportunity of repeating the sentiments we formerly expressed (see Leading Article, August 19th, 1837,) namely, that such a proceeding is wholly uncalled for, and inexpedient; the only example in this country of attendance on such lectures being rendered imperative, occurs in Edinburgh, where the arrangement was notoriously the result of a desire to serve Dr. Thomson; and the medical faculty of the University of London may rest assured that if a similar regulation be adopted by them, it will be regarded as a job to serve the interests of Dr. Carswell. By the way, neither he nor the Professor of Materia Medica is much beholden to their colleague for writing such a letter as that which has called forth these remarks.

PRESENTATION OF SIR CHARLES ALDIS.

It was announced in the Court Circular, a few days ago, that Sir Charles Aldis had been presented, at the Levee on the previous day, by Sir Henry Halford. May we ask, does the learned Baronet know the history of Sir Charles's knighthood? Is the President of the College of Physicians aware that the name of the party in question has been omitted from the list of mem-

bers of the College of *Surgeons*, in consequence of the transaction alluded to? To make the affair still more complete, the book which Sir Charles presented to his youthful Queen on this occasion, was nothing less than an exposition of his method of *curing* cancer without the knife, and on the abuse of mercury in the treatment of—certain diseases. *Proh pudor!*

MILITARY PUNISHMENTS.

IN our articles upon Military Flogging (vol. xx. p. 277 and 596,) while we condemned the inhuman manner in which it has hitherto been practised in our army, we ventured to express a doubt whether there was any thing objectionable in the punishment itself when restrained within the bounds of reason and humanity. In other words, though the old punishments of 800 or 1000 lashes were substantially death with torture, and the modern *mitigation* to 200 or 300 is still utterly indefensible, the philanthropist cannot make any valid objection to punishments of 40 or 50 lashes—punishments which are found in various countries to be perfectly efficient in repressing crime, without the disgraceful necessity of making the surgeon an aid to the torturer, while he stands by and carefully computes how much more suffering the sinking culprit can endure.

We will now proceed to a further consideration of this painful subject, following as our guide the spirited treatise of General Napier, to which we have been already indebted*.

Picketing is a species of torture which has happily grown obsolete. It consisted, we believe, in making the offender stand for a given time upon a pointed stake, where he could relieve himself from the pain endured by the

foot only by a forced position or violent exertion of the arms; and when the continuance of this exertion became impossible, the foot could no longer be relieved. General Napier thinks, that if torture is to remain as a military punishment in time of peace, the picket is preferable to the cat-o'-nine-tails; because he supposes the former to be attended with no danger to life, the culprit being well the moment his punishment is over, and no marks being left. To this it might be answered, that under proper regulation there would be no danger from the lash; that it is a doubtful advantage for torture to leave no mark; and that it is highly improbable that prolonged picketing is unattended with danger. Hence we cannot join in the regret that this species of torture has been disused.

To flogging itself, the General makes eight objections, namely, that it is torture; that it is torture of a very unequal infliction; that the culprit may have more or less obtuseness of feeling; that the infliction may vary, not only according to the will or strength of the drummer, but also according to the temper of the commanding officer and drum-major; that the state of the man's health cannot always be ascertained at the time when he is punished; that the danger to life is greater in a hot climate than in a cold one; that the first punishment is the most cruel, as the back of one who has been frequently flogged gets callous; and, lastly, that the lash brands the sufferer with indelible marks.

These objections are very forcible when urged against the enormous punishments of the old school, and those unjustifiably severe ones which are still allowed to take place; against those which we propose they would be comparatively feeble. The inequality of the punishment would still remain, it is

* Remarks on Military Law and the punishment of Flogging. By Major-General Charles J. Napier, C.B. London, 1837.

true; but this inequality exists in all other punishments. Thus we learn from Mr. Malcolmson's account (*MED. GAZ.* vol. xx. p. 279), that many men, particularly those of indolent habits, endure solitary confinement for four or six weeks, on bread and water, without injury; while in others a shorter period is ruinous to the health. In like manner fines, transportation, working on public roads, &c. &c., are unequal punishments, and are borne with more or less difficulty according to the means, temper, and stamina of the offenders.

Nevertheless some of the inequalities in the infliction of flogging, such as those resulting from the kind of instrument employed, and the duration of the infliction, might be abolished by proper regulations. This would put an end to all refinements of vengeance; such, to give an instance, as we find in the letter of Colonel Arthur to Major Bradley, dated Belize, July 15th, 1818:—

"In carrying the punishment awarded against the prisoner Reeves into execution, which I desire may not be hurried, but most impressively inflicted," &c.

General Napier says—

"I never would admit that drummers were to decide whether a culprit should be flogged with more or less severity; there is a certain degree of severity in the blows given by drummers, any great deviation from which an experienced eye can discover at once; and I always endeavoured to prevent the drummers from being more or less severe than this medium. If the soldier to be punished be not a favourite, if his crime have disgraced the corps, the drummers are oftentimes very savage, and more often they are so much the contrary as to deprive the punishment of its due effect, and almost to provoke the commission of crime."—*Remarks on Military Law*, &c., p. 172.

Hence it is clear that it would be necessary not merely to limit the lashes to a reasonable number, but also to fix most rigorously the manner in which

they are to be inflicted; otherwise your disciplinarians, looking back with a longing eye to the good old days of a thousand lashes, would use some instrument of prodigious power, a cat-o'-ninety-tails, or a scourge fit for the Furies, and make mince-meat of their victims in a few minutes; just as they tell us in books of travels, that a Russian hangman can kill a criminal, when it is thought desirable, by three or four blows of the knout.

It was a shrewd saying of Dr. Cullen's, that there are more false facts current in the world than false theories, and this is singularly exemplified in the present discussion. We showed in our last article on the subject, that authorities differ as to whether a general court-martial is limited to the infliction of 200 lashes, or not limited at all; in India it would seem, at any rate, that there is no such limitation. By a general order, dated Head Quarters, Camp, Moodkee, April 4, 1837, Robert McKin is sentenced to 500 lashes, for having struck Serjeant Winter*. General Napier, again, is very indignant at the practice, which he supposes to exist, of flogging women, in England†, and says, "It is surely high time to abolish this remnant of barbarism;" when the fact is, that it has been abolished nearly eighteen years‡.

Another strange fact relating to this subject is, that Lord Wm. Bentinck, then Governor-General of India, abolished flogging among the native troops on the 24th of February, 1835; thus paying a compliment to the easy *corrigibility* of the Hindoo, at the expense

* *Morning Herald*, Nov. 6, 1837.

† *Remarks on Military Law*, &c. p. 157, note; and p. 201-2.

‡ "The punishment of whipping has been long recognized by the English law, the statute of 22 Hen. VIII., c. 12, directing all vagabonds taken begging to be whipped. But by 1 Geo. IV. c. 57, this punishment is very properly abolished with respect to females, and imprisonment to hard labour, or solitary confinement, is substituted in its stead."—*Drakon's Digest of the Criminal Law of England*, p. 1376.

of the Briton. Lord William, in his evidence before the Commissioners appointed to inquire into military punishments, states his belief that this difference will not create any jealousy on the part of the European troops; he probably thinks that the British soldier will cry out, like the swain in Virgil, *Non equidem invideo, miror magis!*

There is no doubt that one of the chief difficulties in the way of the abolition of flogging is the indifference to it which habit has produced in the minds of military men. On this point we have General Napier with us; for he says, "it is to be observed, that when men are charged with the infliction of any punishment (no matter how revolting it may be in its nature), they generally become desirous of adding to its severity: their minds grow hardened by seeing such punishments inflicted, and they erroneously believe that the bodies of their fellow-creatures grow equally indurated. To correct this horrible disposition to cruelty, which seems inherent to our nature, reason must interfere, or the heart becomes steeled through the eyes*."

And after telling the story of the old woman skinning the eels, he adds—"Those who are accustomed to see and inflict horrible suffering, as the old woman was, lose the power of feeling for their victims, and fancy the infliction not to be sufficiently severe, because it has ceased to excite their own sensibility†." Yet, at p. 140, the General asserts, that it would be utterly false to say that those who are in favour of

military flogging advocate its continuance from want of feeling!

Nil fuit unquam
Sic dispar sibi!

General Napier thinks, on the whole, that it may be safely dispensed with in time of peace, and that it is better to shoot, hang, or transport, a soldier-thief, than to habituate troops to the sight of flogging; but on a campaign, "the army cannot afford to lose the bayonet even of a thief."

ON ANIMAL MAGNETISM.

No. II.

FROM the theories we proceed to the facts, real and false, of this art, and these we shall consider in the order of their importance. We shall endeavour to show that there is not a credible effect said to be produced by animal magnetism, which does not occur under certain other circumstances, from causes of which one or more is in operation during the supposed exercise of magnetic influence. As the causes to which we shall ascribe the different symptoms act alone, and certainly, in many cases, there can be no reason why in others in which magnetism is employed in conjunction with them, their action should be denied, and all the effects ascribed to the other agent, whose presence and nature are altogether hypothetical. A sufficient cause for the production of any phenomenon being proved, it is contrary to all reasoning to ascribe the same phenomenon to any second cause, unless the operation of the first be clearly disproved, or *à fortiori*, while the first is acting.

Of the slighter effects produced by animal magnetism we have but little definite account, authors having generally related only the more extraordinary of the results obtained. On the majority of persons no influence whatever is exhibited; on those least affected a number of anomalous slight symptoms are produced; a sensation of heat or of coolness in the part operated on, which sometimes appears to follow the hand of the magnetizer; a feeling of creeping or trembling over all the body, or wherever the influence is said to be most

* As an instance to show how the bystander becomes hardened as well as the inflicter, we may mention the celebrated proposal of Paley, in his *Moral Philosophy*. He lived in hanging days, when there were two hundred capital offences on our statute-book; and imagining, therefore, that the eels had become used to skinning, he puts forth a suggestion, that some criminals should be thrown alive into dens of wild beasts. Yet Paley was at bottom a mild, good-humoured man.

† Remarks on Military Law, &c. p. 146.

strongly exercised; a slight increase in the frequency and fulness of the pulse; the respiration somewhat hurried; occasional blushing; restlessness, or rather the feeling vulgarly called the *fidgets*, or the *vapours*; slight twitchings of the muscles of the face, or of the part operated on; or yawning and sighing; calmness and quietude, followed by slight drowsiness and desire for repose; heaviness of the eyelids and the limbs; apparent fatigue; dozing; and at last more or less sound sleep, with or without dreams.

Assuredly, none but magnetizers could have thought of ascribing such common things as these to any uncommon cause. The feelings of heat and cold, and those of creeping and trembling, for example, which they would have to result from the entrance or exit of the magnetic fluid, are to us only the usual imaginary feelings which most persons have if their attention be strongly directed to any particular part of the body, more especially if (as is generally the case with magnetic patients) something is expected to occur. Our reader may at once experience this: let him direct his attention to one of his feet, steadily and intently for a few minutes, keeping it all the time quite unmoved; he will very soon perceive sensations of restlessness, or slight itching in it, and if at all nervous he will find the muscles begin to twitch, or cramp come on, so as to oblige him to change its position. Every one must have felt the painful tickling of the palms of the hands, or other parts of the body, during anxiety, or when awaiting something that will at last come suddenly as an electric shock. Long before the knife, or other instrument expected to produce pain, has touched the body, a shivering *retreating* sensation is felt or imagined in the part; and hence many may really be said to cry out before they are hurt, from feelings for which there was no external cause; and so when a blow is suddenly struck at a person, although contact does not take place, he perceives an evident sensation.

The approximation (without contact) of the tips of the fingers to the secretive part of the body, as to the space between the eye-brows, the spot selected by many magnetizers, gives rise to a slight tickling sensation, which produces annoyance and restlessness in some, and

tranquillity in other persons. It may depend in part on the transmission of heat from one to the other, but its more especial cause is shown by this—that if a healthy person who has perceived the sensation when his eyes were open, be made to close them, he can no longer tell with accuracy whether the fingers are approximated to his face or not.

These and the hundred similar instances of subjective sensations which we might quote, are sufficient evidence that the slight sensations experienced during magnetic manipulations *may be* imaginary. M. Bertrand says, that the effects he has produced have always been proportionate to the conviction of the patient. In all these cases, there is attention strongly directed to different parts of the body; an expectation of something occurring; anxiety, or even fear, any one of which is cause sufficient, and in other similar cases actually efficient. Why seek for another in pure hypothesis?

The same explanation is sufficient for the slight disturbance of the circulation and respiration, which indeed are rarely undisturbed by impressions on the mind, however slight, and are sometimes so affected by the more powerful feelings of fear and joy, that death itself is produced.

For the other slight symptoms of *vapours*, drowsiness, and at last natural sleep, no other cause need be sought than the tediousness and *ennui* of passing the hands for more or less than an hour over the most sensitive parts of the body. This is only an instance of the well-known effect of weak monotonous impressions on the senses inducing sleep; analogous examples are found in the soothing influence of a body seen slowly vibrating, or of a distant calm scene, or the motions of the waves, or of quivering leaves; or in impressions on the sense of hearing by the sound of a water-fall, the rippling of billows, the humming of insects, the low howling of the winds, the voice of a dull reader; or on the nerves of common sensation by gentle friction of the temple or eye-brow, or any sensitive part of the body; the rocking of a cradle; any slow and regular motion of the limbs, or trunk;—all these instances show that the effect of monotonous impressions on the

senses is to produce, in most persons, tranquillity, or drowsiness, and ultimately sleep. In some, indeed, they produce another magnetic symptom—restlessness, malaise, or the *vapours*; and this is especially the case with those on whom magnetism is said to act most powerfully—viz. the nervous and sickly, in whom the same monotony that more commonly favours sleep, is sufficient to maintain all night in restless watchfulness.

For the production of some of these symptoms, too, we may suspect the influence of imitation, which possesses a power clearly seen in producing sleepiness; as when the figure of a gaping man is shown to one slightly fatigued, or when one person in a company yawning, all the rest follow; or in the trick of slowly opening the hand before the eyes of an irritable person.

Here, then, for all these slight symptoms we have causes more than sufficient; for many of these will produce them on all persons, of whatever temperament, while they follow magnetic manipulations in only a portion of those submitted to its influence. Indeed, if there be any thing remarkable, it is that any body can sit for an hour unmoved, while another is, with all gravity, looking him intently in the face, and *pawing* over him. Human patience can scarcely endure to *think* of the dulness of such teasing.

Now one or more of the causes we have mentioned—either fixed attention, imagination, anxiety, fear, ennui, fatigue, the influence of monotonous impressions, or imitation—is present in every instance. It is *certain* that the effects *may be* the results of these, and it is therefore quite irrational to suppose them to depend on the superadded influence of animal magnetism, which is entirely hypothetical, and of the existence of which we have no evidence except from those facts which are clearly explicable by the analogy of similar phenomena.

The next effect which we shall notice is the state of *crisis* which Mesmer was in the habit of producing. The symptoms observed in this state were violent convulsions, constrictions of the throat, with sensations of suffocation, as if a ball were lodged there; spasms of the abdominal muscles, pain at the epigastrium, crying, laughing, and hiccup; spitting of a thick, viscid, and sometimes bloody

fluid; flushings of heat and sweating, or sensations of chilliness. As soon as one was seized, another followed; and, before long, nearly the whole of the patients were similarly affected; while the sight or a movement of the principal magnetizer seemed sufficient at once to calm them, or a word from him would give a different appearance to their symptoms.

Now we would refer to the judgment of any man of average ability, who is in the habit of seeing many persons affected with hysteria, whether these symptoms are not daily under his observation—nor these alone, but others far more extraordinary, and to non-professional persons even frightful. It would be difficult indeed to say what symptoms *never* occur in this disease. Sydenham said that, at various times, they “resemble most of the diseases wherewith mankind are affected;” and it is unfortunate that, amidst such a choice as he might have produced, Mesmer should have presented only those which are, as it were, its generic characters, and present in nearly all cases.

The symptoms, then, of the *crisis* are those of hysteria; the causes, too, are alike in both. It is well known that the hysterical paroxysm is very generally produced by fear, or anxiety, or anger, or vexation; that talking about it, or thinking intently on it, will often bring it on; above all, that imitation is an almost irresistible cause of it; while among the most strong predisposing causes is an excited state of the animal passions, the presence of the opposite sex, and an habitual desire of attracting attention. Now of all these Mesmer took advantage; they were his real apparatus, which the tubs and pipes so often described only served to conceal, by giving an appearance of a really active electricity; for few, if any, of his patients were of a class who could know the impossibility of disturbing the electric equilibrium by glass bottles and water. All Mesmer's arrangements were well adapted for producing a powerful impression on the minds of his patients. They were all assembled in one large apartment, and seated around an impressive-looking apparatus, while a dim mysterious twilight only was shed upon them from without, or from a few lamps; notes of soft music were heard from a concealed corner; and young men, selected for their beauty

and robust forms, began to perform the various manipulations of the art, passing their hands over the face, bosom, body, and limbs, of each of those who sat around, in silent expectation of some extraordinary sensation, of which they had heard from Mesmer or their friends. Is it wonderful that ardent and voluptuous French girls should blush, or that their pulses should quicken, or that their breathing become rapid, under such excitement as this? or that a worn-out old *debauché* should perceive unusual sensations? or that the nervous and debilitated should tremble at such treatment? When all that excitement of the animal passions—imagination, anxiety, love and lust—could accomplish, then Mesmer himself entered the apartment, habited in delicately-coloured silk, and wearing an air of deep gravity and importance: walking about as if endowed with magic power, he presented to each in turn a rod, or his hand, which *they were told, and believed*, was charged with the magnetic virtue. Some of them were then at once seized with hysterical fits, and all the others, by imitation, followed them. “As soon as one began,” says the first report, “another succeeded.” “The convulsive state,” says M. Du Potet, “once excited in a patient, never failed to manifest itself in all the others.”

Here, then, we have every circumstance pointing out the true nature of the crisis. The patients were nearly all women, and these the most frivolous of the voluptuous court of Louis XVI., during the most licentious period of modern France; there was therefore the favourable predisposition in ardent animal passions, in a superficially cultivated intellect, in a body weakened by dissipation, and a mind yearning for some new and powerful excitement; there were expectation, anxiety, and perhaps some fear of strange and novel feelings—for Mesmer never failed to let his patients know what might (and *therefore would*) be the effect of the magnetic influence: there were the awe with which ignorance looks up to one supposed to possess supernatural power, the heated passion excited by the meeting and the contact in no decent degree of the opposite sexes, and perhaps some modest fear. With these it would have been more remarkable if the patients had not been affected with hysterical paroxysms; but these once excited, the most powerful of all their exciting causes, imi-

tation, at once came into force. The influence of this is estimated only by those who have seen it. It is but a short time since we saw nearly every woman in a large surgical ward in hysterics, the example for which had been set by one who saw a slight operation performed on another patient. Dr. Bright mentions a similar case. Dr. Darwin speaks of a number of nuns who were successively afflicted with hysteria, and amongst other symptoms, an anxiety to imitate the squalling of cats. Dr. Whytt describes a disease common in the Island of Zetland, which affected a number of young unmarried women, and some few of the opposite sex. There were violent palpitations, convulsions of the limbs, and difficult respiration. If one was seized in the market, or at church, all who had ever been affected before directly followed the leader, and each such general disturbance was sure to increase the number of convulsionnaires. Boerhaave had a similar case in a number of boys and girls in a charity-school at Haarlem, and Dr. Haygarth another in Anglesey, where in two or three months 18 girls were affected with pains in the head and side, convulsions, and other symptoms, which magnetizers would have called magnetic. In short, the acquirement of even severe hysterical affections, if not of epilepsy and other nervous disorders, by imitation, is well known, and its occurrence guarded against by every practical physician, and even by the public.

We lay the more stress on this as a cause of the supposed magnetic symptoms, because it is one that the magnetizers themselves (very few of them being practitioners) cannot know the importance of, and because its influence has been evident in the account given by every one of them, however different the effects they produced. Thus, all Mesmer's patients had the commoner form of hysterical fits, and *all were affected alike*; the same was the case with Deslon's. Puysegur discovered somnambulism, and immediately he had few but were somnambules (a state unknown to Mesmer), and he said that every patient was affected according to the system and wish of the magnetizer: he himself desired somnambulism, and all his patients had that affection. M. Petetin wished for cataleptics, and in less than two years he had in the then small town of Lyons eight cataleptic cases,—a greater number we believe than has occurred in

all England during the same period. And thus it has continued. The series of symptoms which each magnetizer induces may, as Puysegur said, be whatever he pleases: he has but to make one case for a model, and this he may shape and frame by descriptions of desired results, before exciting the mental feelings, and all the rest will at once accurately imitate it. We have no doubt whatever—for from what little has already been seen or published it is even now nearly evident—that the generic character of all cases of magnetic hysteria produced in this country will be the easy sleep of M. Du Potet, and the raving, abusive, and convulsive form of Dr. Elliotson.

If further proof of the hysterical nature of the crisis were wanting, it may be found in the cessation of these symptoms when the causes to which we have referred them ceased to be made use of. It would be difficult else to explain why the same state never occurs now; for the magnetic agent must be the same, and therefore why not the effect? We can easily imagine that it was *advisable* the crisis should be given up; it was too violent, and not marvellous enough; at times too painful to the patient, and alarming to the by-standers; not to add that it had been made the means on no few occasions of gratifying the passions whose exaltation had favoured its production. The twilight chamber, the large assemblies of both sexes, the mystical apparatus, the magic dress, and the awful appearance and gestures of the arch magnetizer, were therefore given up, and the common form of hysteria which they had produced was exchanged for the mild and comparatively pleasing somnambulism which the philanthropy of M. Puysegur engendered.

It may be said by a few that the fact that the *crisis* occasionally occurred in men, disproves its hysterical nature. But this is an error; though more rarely, it is well known that symptoms precisely like those of hysteria in women occur in the opposite sex, especially among those who present the same peculiarities of temperament and external characters as the females subject to it—who are easily excited to grief or joy—who are of irritable or variable temper, and of weak or moderate intellect, of generally delicate health, or debilitated by disease—and who are what is loosely but expressively called *very nervous*. These are the *only* individuals of the

male sex who are obnoxious to hysteria or to the influence of magnetism—a fact which, while it assists in proving the correctness of the view which we have taken of it, disproves completely any theory that has yet been invented.

REPORT

LAID BEFORE

THE GENERAL COURT OF GOVERNORS
OF THE

Small-Pox and Vaccination Hospital,
1st of February, 1838.

BY GEORGE GREGORY, M.D.
Physician to the Hospital.

My Lords and Gentlemen,

I BEG to place before you a statement of the principal occurrences within the Hospital during the past year, and this I do the more willingly, from a conviction that the facts I have to adduce are calculated, by shewing the great utility of the institution, to attract towards it that share of public patronage which it enjoyed in former times, before vaccination shrouded it in a measure from the public notice.

The hospital received within its walls, during the year 1837, 251 patients, of whom 12 laboured under eruptive diseases, not of a variolous character. There remain 239 cases of true small-pox, who here found a refuge, relieving thereby numerous families from extreme inconvenience, and withdrawing from the crowded population of this metropolis so many sources of infection and danger. Of the 251 cases admitted, 94 were females, and 157 males.

There were admitted,

Under seven years of age	29
Between the ages of seven and fourteen ..	27
Of fourteen years of age and upwards ..	195
Total	251

Of these there were received,

With letters of recommendation from individual governors	16
With letters of recommendation from parishes	101
On payment of one guinea (chiefly servants in gentlemen's families ..	41

As objects of charity,

From the several hospitals and public asylums of London	29
On their own application, and without any recommendation but their sickness and poverty	61

Total 251

I would beg to invite the particular attention of the governors to the fact, that nearly two fifths of the patients, during the past year, were admitted without any letters of recommendation, and simply as objects of charity; and to add my belief that this may be assumed as the average practice of the hospital for a long series of years past.

The admissions were very far from being equally distributed through the several seasons of the year. The accompanying Table will shew, that during the first three quarters they were comparatively few. About the middle of October small-pox began to prevail as an epidemic in London, and increased to such an extent, that 70 patients were received in the course of one month (December), a circumstance hitherto unparalleled in the annals of the hospital. This epidemic still continues.

Table of Admissions into the Small-pox Hospital during the year 1837.

January	5	25
February	8	
March	12	
April	8	12
May	11	
June	20	
July	11	44
August	14	
September	19	
October	27	140
November	43	
December	70	
Total	231	

The general character of the disease, during the past year, has been mild. The deaths have amounted only to 46, equivalent to 19 per cent., which is an unusually low rate of mortality.

The number of cases subsequent to vaccination, has been considerable, amounting in all to 95, or 40 per cent. of the total admissions. Of them, nearly one-half (46) had small-pox in that peculiarly mild form, called the varicelloid. These cases are devoid of all danger, and are sent into the hospital chiefly for the security of others. The remainder (49) took the small-pox in various degrees of intensity. Many were for a time in danger; but of the whole number one only died. The records of the hospital, this year, corroborate the important principle, that the susceptibility of small-pox increases with the interval from the date of vaccination. Eight children only (of ages varying from seven to fourteen years) were received having small-pox after vaccination; while of adults (between the ages of fourteen and thirty-five) the number admitted having small-pox after vaccination, was 87.

It further appears, that not only the fre-

quency but the severity of the disease increases with the distance of time from the period of vaccination. The eight children, just mentioned, had each the disorder in the mild or varicelloid form. The severe cases of small-pox after vaccination occurred exclusively among the adults.

In these facts there are still abundant sources of consolation. While we acknowledge, on the one hand, that the security which vaccination affords against the inroads of small-pox is neither so uniform nor so permanent as had been anticipated, we see, on the other, that vaccination can, in a measure, strip small-pox of its malignity, and reduce it to the character of a mild, and comparatively harmless disorder.

The bills of mortality announce only 217 deaths by small-pox, in 1837. Owing to the recent law of registrations, these documents are avowedly much less to be depended on than formerly. Making all due allowance, however, the past year appears to have given the fewest victims to small-pox of any year which has elapsed since the bills of mortality were established in London. The number of cases may have been as great as in former years, but the difference is in the rate of mortality.

I shall next advert to the vaccination department of the hospital, which this year presents a novel and interesting feature. The number vaccinated during the year amounts to 3,078. Besides which, lymph has been supplied to 1,013 medical practitioners and others.

The lymph in use at this hospital had been preserved, in uninterrupted descent, for a very long period of time; but for three or four years past, I had noticed that its intensity was diminished, and that eight or ten incisions produced not more irritation than the three to which I was accustomed fifteen years ago. In March last, Mr. Marson, the resident surgeon, employed lymph obtained from a different source. This new lymph was found to be far more intense and active than the old lymph. Three or four incisions were now found amply sufficient; and so satisfied was I of the superior quality of the new lymph, that, after a careful trial of about two months, the old lymph was suffered to die out, and for the last six months we have vaccinated exclusively from the new stock. These facts have convinced me, that vaccine lymph, by passing through the bodies of many persons, loses, in process of time, some essential portion of its activity. It follows from this, that an occasional resort to primary lymph from the cow becomes a matter of great importance, perhaps even of indispensable necessity.

Through the kindness of Mr. Sewell, of the Royal Veterinary College, I have re-

cently obtained fresh matter from the cow, with which trials have been made. Although these have hitherto been unsuccessful, I shall pursue them, as opportunity offers, and avail myself of all favourable occasions for trying new varieties of lymph. The existence of two forms of vaccine virus, possessing different grades of intensity, is now clearly established. Others may possibly exist. At any rate, the selection of an active virus is a matter of such obvious importance to the successful practice of vaccination, that I shall continue to direct my earnest attention towards it.

GEORGE GREGORY, M.D.
Physician to the Small-Pox and
Vaccination Hospital.

31, Weymouth Street, London,
February 1, 1838.

PHYSICAL SOCIETY, GUY'S HOSPITAL.

Saturday, Feb. 10, 1838.

EDWARD COCK, ESQ. IN THE CHAIR.

MR. DENDY read a paper—

*On Effusions into the Membranes of the Brain
and Spinal Marrow.*

He referred briefly to the discrepancy of pathologists regarding the origin of these effusions, Cheyne, and Goelis, and Abernethy, having asserted that hydrocephalus may usually be traced to ramollissement, or hyperæmia, or effusion of lymph in the alimentary canal; while Spurzheim and some of his disciples, in reference to cause and effect, affirm that the cerebral is always the primary, the intestinal the secondary disorder.

He then alluded to the varied situations of the effused fluid, beneath or within the envelopes of medullary matter, and to the extensive intercommunication, which often reduces the ventricle and spinal sheath to one cavity.

"In the description of acute and chronic effusions into medullary membranes, authors have employed a latitude of expression, as if they consisted but of one serous tissue, like the pleura or peritoneum, and as if the fluids were all alike, and poured into one cyst.

"The minute anatomists have, on the contrary, differed as to this locality. Bichat, and indeed the majority of pathologists, fixing on the serous bag of the arachnoid, while Andral, Magendie, and Dr. Green, have decided on the subarachnoid tissue, or pia mater."

"The effusions may be referred to two principal divisions:—

"1. As the result of abnormal or dis-

eased action—the acute, or inflammatory, or febrile forms—the deposit of fluids and films morbid in their nature.

"2. An excess of normal secretion—the conate or chronic forms—resulting from intra-uterine derangement.

"The termination of the acute forms, hydrocephalus and inflammation of the spinal membranes, in limpid effusion, were most strongly marked in the puerile stage, when the balance of circulation between the sanguineous and absorbent systems is in constant danger of disturbance.

"In most cases there is an essential proneness or predisposition, closely allied to struma."

Mesenteric disease, conjoined with hydrocephalus, and the acute disorder attacking one child after another, had been observed by the author. In one family six were snatched away by rapid effusion—the water stroke.

"It is in these cases that our plans of treatment are so constantly inefficient."

"When meningitis occurs in children of a different temperament, the fatal termination is marked by other characters.

"We have in these membranes two species of inflammatory action—the phlegmonous and the erythematic or tubercular.

"In the first there is more combination of arachnitis with the diseased action of the pia mater, and it is marked by a rapid course; while in the second, it is either the sequela of a subacute form, or comes on by stealth.

"The morbid products of the two forms are usually different. In the first, lymph, false membrane, or purulent deposit; in the latter, generally serous effusion.

"Probably these results may be also influenced by the nature of the membranes on or by which the fluids are poured out. Cases have occurred where aqueous fluid has been found on the pia mater, and a purulent deposit within the cavity of the arachnoid."

The author considered it a question to be decided whether the usual termination of subarachnoid meningitis and arachnitis were essentially different in their character, but thought it probable that medullary ramollissement and granular deposits depended essentially on the former, while thickening and films of lymph were chiefly confined to the latter.

The second part of the paper was devoted to the excess of the normal secretion or cephalo-rachidean fluid, cases widely different from the sequela of encephalitis, and to which alone the term hydrocephalus should be applied.

The observations of the author related to two points interesting both in an anatomical and pathological sense.

The situation of the cerebro spinal fluid, and the origin of conate hydrocephalus and spina bifida.

The first were adduced with a view of testing the opinions of Magendie regarding the seat of the fluid.

It was found that the analysis of the fluid did not yield the constituents of serum, which we might expect if it were contained in a serous bag. Then, regarding the distribution of the arachnoid, it had been a subject of dispute, whether the arachnoid passed into the ventricles by the veins of Galen, as Bichat states, or not.

"Magendie asserts that it does not line the ventricles, but that their cavities communicate with the subarachnoid tissue, and in this is the cerebro-spinal fluid seated. Were this so decided, it would refer the pathology of the conate hydrocephalus, as well as the effusion of granular meningitis, to the pia mater instead of the arachnoid, and might explain the comparative results of our treatment in these cases. In illustration of Magendie's views, it might be difficult to explain the intricate insinuation of the fluid among the convolutions, and also the facility with which sanguineous effusion from the vessels of the pia mater communicates with the surface of the brain and the ventricles, and even descends to the sacrum, the arachnoid remaining *entire*."

Regarding the conate effusions, Mr. Dendy argued, that they might not always depend on osseous deficiency, but that the bifid spine, as well as the enclosed sutures and enlarged crania, might be the result of the pressure of the cerebro spinal fluid from within outwards. "We know that excess of this fluid will stunt by compression the growth of medullary matter, keeping down the cerebral mass to a mere tubercle, consisting of olfactory nerves and corpora striata, and checking the development of the spinal medulla. Thus, I believe, will the centrifugal tendency, or determination outwards, often induce the non-coalescence of osseous sutures. In one case, in which I operated repeatedly, the base of the tumor was a shut lateral ventricle, the fluid having pressed upwards, carrying with it the ceiling of the ventricle, and bulging through a fissure of the parietal bone. May we not, indeed, sometimes regard these openings as the modes of nature to avert the dangers of compression from fluid excess? In the acute disease, where we may have, indeed, absorption, and a renewal of ossific deposit, we have seen re-excitement produce fresh effusion, the sutures have again expanded, and the disease has been re-established for a time.

If ossification had been in these cases complete, the subjects would have probably died with symptoms of compression: this,

indeed, is very speedily the result of hydrocephalus and hydrorachitis, when the cranium and spinal canal are shut.

In the course of the discussion which followed the paper, a difference of opinion arose as to whether the cerebro-spinal fluid was contained within the arachnoid or in the sub arachnoid tissue. Mr. Hilton, on being requested to give his opinion, said, he had no doubt that the cavity of the arachnoid contained it, and disagreed with Magendie, who had asserted, that the ventricles were not lined by that membrane. The chairman, on being particularly called upon by Mr. Dendy, expressed his full concurrence with Mr. Hilton; he quite believed that the cerebro-spinal fluid was contained within the cavity of the arachnoid.

At the next meeting of the society, February 24th, Dr. Guy in the chair, Mr. Birkett will read some cases of syphilis, with observations.

ACCIDENTS ADMITTED AT THE LONDON HOSPITAL.

Surgeon, MR. ANDREWS.

Assistant-Surgeon, MR. ADAMS.

Jan.	Sex.	Age.	Case.
23.	M.	31	Injured hand.
	M.	33	Injured arm.
	M.	70	Injured head.
	F.	6	Burn.
24.	F.	21	Lacerated scalp.
	M.	31	Lacerated leg.
	M.	50	Fractured ribs.
25.	M.	54	Lacerated scalp.
26.	M.	32	Injured shoulder.
	F.	42	Fractured leg.
27.	M.	59	Cut leg.
	M.	21	Injured hip.
29.	M.	54	Fractured neck of femur.
	F.	43	Scalded hand and forearm.

In-Patients 16

Out-Patients 41

Total 57

Two cases of strangulated hernia were operated on: the subject of the first was a female, aged 45, who had been labouring under the severest symptoms of strangulation for a week. There was a very small femoral hernia on the left side. The operation was immediately performed, and the intestine, which had completely lost its natural appearance, was returned. She recovered without a bad symptom.

The second case was that of a young man, who had had an inguinal hernia for some time, for which he had worn a truss; this, however, he had ceased to do for the last year, in consequence of a portion of the hernial tumor remaining constantly in the sac. Symptoms of strangulation had

existed for thirty hours. After unsuccessful attempts at the taxis, whilst in the warm bath, and the application of ice, the operation was performed. The stricture was formed by the neck of the sac, which had become much thickened; the intestine, dark in colour, but not in the least disorganized, was returned, and a portion of adherent omentum was excised.

The man died on the ninth day after the operation, without any relief from the bowels.

It was found, on examination, that the portion of intestine, about five inches in length, had never recovered its proper appearance, but although not sphacelated, yet was dark and contracted, and seemed incapable of transmitting onwards the contents of the bowels above. There were two lines of ulceration of the mucous membrane in the situation of the stricture, as if nature had commenced a separation of the useless part. There were traces of peritonitis, but by no means intense.

CHOROID GLAND OF THE FISH'S EYE.

To the Editor of the Medical Gazette.

SIR,

IN my recent communication on the so-called choroid gland of the fish's eye, I mentioned that its real nature was still very much misconceived by anatomists. I was not then aware of Mr. Owen's observations on the subject, which show that the vascular structure of the body had not escaped him, otherwise it would have given me great pleasure to have adduced such an authority in confirmation of my description.

Mr. Owen's remarks contained in the 3d volume of the Physiological Catalogue of the Hunterian Museum, 1836, are the following:—

"1656, Parts of the sclerotic coat, membrana argentea and choroid gland of the Moon-fish, to show the structure of the latter substance, which is peculiar to osseous fishes. It is always situated between the silvery membrane and the true choroid, and is extended more or less, and in different forms, about the termination of the optic nerve, which is here preserved. In the present species it almost completely encircles the nerve in the form of an oval ridge, and its structure appears fibrous, whence it has been considered muscular; but the fibres consist in reality of minute parallel and closely disposed vessels, which in the recent state are of a bright red colour. The use of this part has not been determined."—Your obedient servant,

T. WHARTON JONES.

30, Newman Street, Oxford Street,
Feb. 14, 1838.

TRIBUTE TO MR. EARLE.

To the Editor of the Medical Gazette.

SIR,

PERCEIVING by the MEDICAL GAZETTE of last week that a number of the pupils of the late Mr. Earle, residing in London, have expressed a wish to mark, in some appropriate manner, their sense of his worth and talents, I beg to suggest that the definite object of such meeting be made generally known by circular or advertisement, and that the subscription list be kept open for some little time, in order that the numerous though scattered pupils of that excellent surgeon and most estimable man may have an opportunity of assisting to render as emphatic and suitable as possible the deserved tribute to his memory.

Your obedient servant,

RICHARD MIDDLEMORE.

Temple-row, Birmingham, Feb. 20, 1838.

COLLEGE OF SURGEONS.

MR. TYRRELL has been elected a member of the Council, in the room of the late Mr. Earle.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, Feb. 20, 1838.

Age and Debility	49	Whooping Cough	11
Apoplexy	6	Inflammation	23
Asthma	13	Bowels & Stomach	1
Consumption	42	Brain	7
Convulsions	28	Lungs and Pleura	16
Croup	2	Influenza	2
Dentition or Teething	5	Insanity	2
Diabetes	1	Liver, diseased	1
Diarrhoea	1	Measles	4
Dropsy	15	Mortification	2
Dropsy in the Brain	2	Paralysis	2
Dropsy in the Chest	3	Small-pox	6
Erysipelas	2	Thrush	1
Fever	11	Unknown Causes	47
Fever, Scarlet	2		
Fever, Typhus	7	Casualties	8
Heart, diseased	4		

Decrease of Burials, as compared with }
the preceding week } 55

METEOROLOGICAL JOURNAL.

Feb.	THERMOMETER.		BAROMETER.	
	from 22	to 31	29.62 to 29.52	
Thursday	15	22 34	29.50	29.58
Friday	16	22 36	29.63	29.82
Saturday	17	22 39	29.94	30.09
Sunday	18	27 39	30.09	29.87
Monday	19	19 39	29.69	29.63
Tuesday	20	28 37	29.63	29.66
Wednesday 21				

Winds E. by N., N., and N.E.

Except the mornings of the 16th and 20th, cloudy; snow and hail, accompanied with rain, on the 17th; and rain on the morning and evening of the 21st.

Rain fallen, 1.375 of an inch.

CHARLES HENRY ADAMS.

WILSON & SON, Printers, 57, Skinner-st., London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MARCH 3, 1838.

LECTURES

ON

FORENSIC MEDICINE;

Delivered at the Aldersgate School of Medicine,

By SOUTHWOOD SMITH, M.D.

Physician to the London Fever Hospital.

LECTURE IX.

Change of Colour in the Cineritious Substance of the Brain. — Change in Consistence. — Laceration. — Ulceration. — Scrofulous Tubercles. — Change of Colour in the Medullary Substance. — Ecchymoses. — Cloudiness. — The Medullary Substance of a Grey Colour. — Ramollissement. — Diffuse Suppuration. — Encysted Abscess. — Induration. — The preceding Changes sometimes unattended with any appreciable Symptoms during Life. — Effusion of Serous Fluid, Coagulable Lymph, Purulent Matter, and Blood, into the Ventricles. — Apoplexy. — Premonitory Symptoms.

Change of colour in the cineritious substance of the brain.—The cineritious substance of the hemispheres of the brain presents various morbid appearances, sometimes the result of natural disease, and at other times produced by external violence, with which it is highly important, in a judicial point of view, that you should be familiar, and able, in any given case, promptly and accurately to refer to their proper cause.

The cineritious portion of the cerebral substance is often the seat of inflammation, acute or chronic, and of congestion, active or passive. These affections are indicated by a change in the colour of the cineritious substance, which, from its natural grey colour, may vary from a rose red to a bright crimson, or from a dusky brown to a deep violet, or purple. The

brighter and the more intensely red the colour, the greater the evidence that it is the result of recent acute arterial action. The more dusky or purple the colour, the greater is the probability that the change is connected with interrupted circulation of the venous capillaries, constituting the state of venous congestion. When the arterial capillaries give to this portion of the cerebral substance a crimson colour, and when this colour extends over a large portion of the hemispheres, you may in general consider it as evidentiary that inflammation has been induced by some natural cause; but in proportion as this red or crimson colour is limited in its extent, and is distinctly circumscribed, it should beget suspicion that it has been produced by some cause acting specially on this particular portion of the brain—as a blow, a fall, or some other form of external violence. When the dusky or purple colour indicates congestion of the cerebral vessels, you should examine whether this state of congestion be confined to the blood-vessels of the brain, or whether it be connected with obstructed circulation in some distant organ, or have been produced by some external agent. You may find reason to conclude that the congestion is confined strictly to the cerebral blood-vessels; or you may discover in the lungs, in the heart, in that peculiar condition of the different viscera which accompanies fever, the true source of the congestion in the brain; or, on the contrary, this condition of the cerebral blood-vessels may afford one evidence of poison, or of death, from some form of external violence—as suspension, or drowning.

Change in consistence.—When the change of colour in the cineritious substance is the result of natural disease, its consistence is generally equally altered. It becomes more soft and pulpy than natural, and is either torn away from the convolutions, with the pia mater, on the slightest

touch, or, when that membrane is removed, is capable of being scraped away with the greatest ease.

Laceration.—But instead of a change in the colour and consistence of the cineritious substance, you may find it actually lacerated. This, of course, can result only from the action of some violent agent. You will, in general, have little difficulty in determining what that agent is. The effusion of blood may have broken down the cineritious substance. Then you will find the clot probably at the surface of the convolutions, and, on searching farther, you will discover laceration in the medullary fibres also, or some other evidence of an apoplectic seizure. But if you cannot succeed in tracing this event to any natural cause, you must bear in mind that it may have been occasioned by external violence (as concussion), for the evidence of which you will then seek.

Ulceration.—The cineritious portion of the brain, as well as the foliated surface of the cerebellum, is occasionally the seat of ulceration. This is not a common occurrence, but you may meet with it. On different parts of the hemispheres appear spots of various extent, with ragged edges and sensible loss of substance. On the ulcerated spots the pia mater and arachnoid are commonly destroyed: sometimes the spot presents the appearance as if a clean excavation of the cerebral matter had been made, and the excavated surface is covered either with bloody or with purulent matter. From this condition of the brain, taken alone, you can determine nothing as to the cause of death. It is sometimes the result of natural disease, but it is often the consequence of mechanical injury; and when you meet with it, you must search for other indications of disease, or of violence, before you can determine the true nature of the case.

Serofulous tubercles.—The cineritious substance is the common seat of those depositions of adventitious matter which are called serofulous tubercles. These bodies are often wholly confined to the cineritious substance, until by their increased growth they pass, on the one hand, into the white matter, and on the other, become adherent to the pia mater and the other membranes.

They vary greatly in size, from that of a millet-seed to that of a pea, and sometimes they attain to the magnitude of an egg. Like the tubercles of the lungs, they appear sometimes to remain perfectly quiescent for a long time after their formation, but at length they begin to soften in the centre, and then constitute a kind of abscess surrounded by a firm cyst, very analogous to that of the softened tubercle of the lungs. While quiescent and of small size, they may occasion no appre-

ciable inconvenience; but when at length they become active, grow, soften, and form cysts of purulent matter, they produce violent irritation. This irritation often manifests itself quite suddenly in some severe form of cerebral disease which proves rapidly mortal. A child may be in apparent health. Stopping suddenly while at play, or waking up in the middle of the night from sound sleep, with a scream, it may complain of violent headache; or without any pain, it may suddenly become pallid, faint, and perfectly prostrate, and from that moment with appalling rapidity may supervene the most urgent symptoms of acute hydrocephalus. On examining the brain, you may find different portions of the cineritious substance studded with tubercles, in a state of softening, with or without serous effusion.

Change of colour in the medullary substance.—Notwithstanding the countless number of blood-vessels with which the medullary substance of the brain is pervaded, it is when perfectly sound almost a pure white. On a circumscribed spot, or over an extended surface, you may find this substance changed from a white to a pink or a deep red colour: a change which you may regard as evidence of active inflammation of the brain, even though there be not the slightest appreciable alteration in the consistence or structure of the cerebral matter. This evidence you may consider as greatly strengthened when on making a section of the brain you find the points of blood from the cut surface increased in number; when the points themselves are larger than natural, forming blotches rather than minute points; and when, as the scalpel passes over the cut surface, drops of fluid blood ooze from the vessels. You are called to examine a case of very rapid death. On inquiry into the history of it, you find that the person was attacked suddenly with headache; that the headache was followed quickly by delirium, delirium by coma, and coma by death; or to the headache succeeded violent convulsions, in a paroxysm of which the patient expired. On examining the brain, you find no appearance of disease whatever excepting this red tint, which is restricted to a circumscribed spot, or which pervades in a greater or less degree the whole extent of the medullary matter. Here is an adequate cause of death, and whether the redness be limited or circumscribed the cause may be natural disease. Nevertheless, whenever the redness is distinctly circumscribed, search diligently for evidence of mechanical injury, because inflammation thus limited in its extent is very frequently the immediate consequence of external violence.

Echymoses.—If, instead of red points or

blotches, you find numerous minute ecchymoses, like petechiæ, of the medullary substance, you may consider this appearance as unequivocal evidence that the circulation of the cerebral blood-vessels has been from some cause or other greatly obstructed; obstructed to such a degree that some of the capillary branches have given way, and allowed the escape of minute portions of blood. This can scarcely happen from any natural disease seated in the brain itself; but it may result from great obstruction to the circulation, in consequence of disease in some distant organ, as the lungs or heart. Often, however, it is entirely the result of mechanical violence. One characteristic mark of severe concussion is the appearance of little round spots of ecchymoses, the result of ruptured vessels; the spots varying from the size of a pin's point to that of a pea, and situated in different parts of the brain.

Cloudiness.—But instead of blotches or ecchymoses you may find the clear white substance of the brain clouded, as if from staining; and the cloudiness may be either of a pink or of a purple colour. When it is of a pink colour, it indicates acute inflammatory action; when of a purple colour, retardation of blood through the cerebral vessels. Such retardation of the blood is the constant consequence of disease in some distant organ, as the lungs or heart; but on the other hand it is often the attendant on death from asphyxia, and more especially of death from suspension.

The medullary substance of a grey colour.—Sometimes as the result of natural disease, apoplexy, paralysis, convulsions, dark-coloured blood is accumulated in the minute vessels to such a degree that the medullary substance of the brain is changed from a pure white to a grey colour: an approach to this may also be the result of death from suspension.

Ramollissement.—Such are the changes of colour in the medullary substance to which it is important that you should direct your attention; but you will often find not merely the colour, but the consistence of this portion of the cerebral matter, essentially altered. By far the most frequent morbid change of this kind is that termed softening (*ramollissement*) of the brain. A part of the medullary substance becomes broken down into a soft pulpy mass, which at first retains its natural colour, and is without fœtor; but as the process goes on, the colour changes to a yellow, or grey, or greyish brown, and sometimes it acquires a reddish hue from the admixture of blood. The degree of disorganization varies from a slight diminution in the natural consistence of the medullary substance to a semi-fluid mass, which often presents the appearance of a

curd. The softening may affect any part of the brain; the surface or the substance of the hemispheres, the corpora striata, the optic thalami, the crura cerebri, any portion of the apparatus of union, and the cerebellum. It varies greatly in extent, being sometimes confined to a very minute space, and at another involving the greater portion of one lobe, or even of one hemisphere. It appears to result from two different causes. The evidence is indubitable, that in a great number of cases it is the direct result of inflammation, acute or chronic; but in some cases it would appear to be the consequence of a diminution in the supply of arterial blood to the part affected, from the operation of some cause which obstructs the circulation, and which thus produces a species of gangrene. Dr. Abercrombie suggests, that when any portion of the brain is deprived of its proper nutriment, in consequence of the nutrient arteries being from any cause rendered incapable of supplying it with the requisite quantity of arterial blood, it may lose its vitality or become gangrenous, just as any other part of the body.

Diffuse suppuration.—Sometimes the softened cerebral substance is evidently mixed with purulent matter, affording decisive evidence that the process of inflammation has been going on. Sometimes the inflammatory action being diffuse, and having no tendency to limit its progress by the effusion of lymph, or the formation of a vascular cyst, there is formed a large ragged undefined cavity, filled partly with purulent matter extremely fœtid, and partly with broken down cerebral substance, the surrounding medullary substance being soft and disorganized.

Encysted abscess.—But on the contrary, you will often find a well-defined regular cavity, filled with purulent matter, lined by a soft and vascular cyst, and surrounded by medullary matter in a healthy state. This constitutes the encysted abscess, the *apostema cerebri*. Collections of this kind appear often to have their origin in the development of the serofulous tubercles already described, which slowly grow, soften, and suppurate; but in other cases they seem to consist merely of a portion of cerebral matter itself, indurated by inflammation, and subsequently surrounded by a cyst of coagulated lymph, and deriving its vascularity from the fine vessels of the pia mater. Cysts of this kind appear to be readily formed in the brain during the slow process of inflammation, and they often arise spontaneously from internal causes; at the same time, however, it is important that you should bear in mind, that it is often possible distinctly to trace their origin to external violence.

Induration.—But inflammation, instead of softening, may indurate the brain. It is by no means uncommon to find the cerebral substance in general preternaturally firm and unyielding; at other times the hardening is partial, extending only to a small circumscribed portion; and this hardened part is often surrounded by a portion in a state of softening. When the induration is limited in its extent, you may succeed in tracing it to a blow or fall, or some other form of mechanical violence; but it may have its origin purely in internal causes, and a careful examination of all the circumstances of the case will often enable you to ascertain the true cause.

These changes sometimes unattended with any appreciable symptoms during life.—In general, inflammation of the brain, and the several morbid changes in the cerebral substance produced by the inflammatory action, are attended with such manifest disturbance of the functions of the organ, as to leave no doubt that it is the seat of serious and alarming disease. But this is not invariably the case, and it is of the last importance, in a judicial point of view, in all your examinations of the cause of sudden death connected with cerebral disease, that you should bear this fact in mind. Instances without number occur, in which decided inflammation of the brain, extensive softening, diffuse suppuration, encephalitis, abscess, and preternatural induration of the cerebral substance, exist during life without being discovered, or even so much as suspected. Persons in apparent health, but really labouring under one or other of these forms of disease, are seized so suddenly with violent symptoms, and die with such extreme rapidity, that the suspicion is actually excited that they have been destroyed by poison. An elderly sailor for months before had done duty, eaten his rations, and drank his grog, as usual. One day, while in the act of pulling his oar, he suddenly dropped down, and died at once. On examining the body after death, there was found in the middle lobes of the brain an extensive abscess, which had made its way to the surface. You can readily conceive of circumstances under which such a case of sudden death might give rise to the suspicion of the administration of one of the most powerful poisons; but the inspection of the brain would at once decide the question. Dr. Christison records a remarkable case, which actually did give rise to a strong suspicion of poisoning.

A man who had previously enjoyed excellent health was found one morning in a low lodging house, in the Lawnmarket, at Edinburgh, comatose and convulsed; he died several hours afterwards.

The neighbours spread a report that the woman of the house had poisoned him, with the view of selling the body for dissection. By an odd coincidence, it happened that the police, when they went to apprehend the woman, found an anatomist in the house concealed in a closet. The body was examined judiciously. An ulcer was found on the fore part of the left hemisphere of the brain, and a small patch of softening on each middle lobe. These appearances declared the true nature of the case, and were quite sufficient to decide that the death arose from a natural cause.

But you must bear in mind that softening of the brain, when not far advanced, is very apt to escape notice, because, as has been already stated, it is not necessarily attended with a change in the colour of the diseased part. There is reason to believe that it has been often overlooked, and that the death which it has occasioned has been erroneously attributed to simple apoplexy. Whenever you meet with a case apparently of simple apoplexy, search carefully through the substance of the brain, to see if you can find no point of softening in it.

Effusion of serous fluid into the ventricles.—The circumstances connected with the effusion of a preternatural quantity of fluid into the ventricles of the brain, occasionally present questions of considerable interest in a judicial point of view. The moisture which lubricates these cavities during life would appear to condense after death, so that in the healthiest condition of the brain they always contain some portion of fluid; but the quantity seldom exceeds a drachm in each lateral ventricle, and that contained in the smaller cavities is scarcely appreciable. In certain states of the system this quantity may increase somewhat during the process of death, or shortly afterwards; but when it exceeds two or three drachms, you may generally regard it as the result of some morbid action, the true nature of which it may be of great importance that you should clearly ascertain.

That a preternatural quantity of fluid may accumulate in the ventricles of the brain, as the result of acute inflammation, the evidence is indubitable. The inflammatory action may come on quite suddenly from a great variety of natural causes, and terminate in effusion with great rapidity. In a case of this kind, with febrile and inflammatory symptoms highly acute, death may take place in a few days or even hours. On examining the brain, you may be able to obtain from all its cavities not more than half an ounce, or six drachms, of fluid; yet this may have been a case of inflammation ter-

minating in effusion—a case of hydrocephalus in its most acute form.

The more intense the inflammation, and the more rapid its progress, in other words, the more acute the hydrocephalus, the less the effusion. The intensity of the inflammation extinguishes life before the fluid has time to accumulate in any considerable quantity; and the small quantity which is poured out, from the suddenness with which the effusion takes place, produces more violent irritation than a much larger quantity slowly deposited. Suspicion arises that a child has been put to death by violent means. Two or three days before its death it was in perfect health. It was attacked suddenly with the most violent symptoms, for which no probable cause can be assigned. You find in the brain manifest traces of inflammation; you find a quantity of fluid effused into its cavities, the amount of which, when all of it is carefully collected, scarcely exceeds half an ounce. You inspect the chest and abdomen; you find all these organs free from disease. You are now in possession of evidence sufficient to warrant you in deciding that the death in question has arisen from a natural cause, acute hydrocephalus, arising as an idiopathic disease of the brain.

All the other forms of hydrocephalus will occasion you less difficulty. When you find an accumulation of three or four ounces of fluid, with the indications of recent arterial action in the brain, you cannot for a moment doubt the nature of the disease. Still less can you be in doubt when you find the accumulation to be, not in ounces, but in pints; when, instead of two or three drachms, the quantity condensed when the brain is perfectly sound; or half an ounce, which, as we have seen, may be all that is effused when the disease exists in its intensest form; or from three to four ounces, the quantity accumulated when the disease is moderately acute, you find from eight to ten pints; and when, as the necessary consequence of so large an effusion, the whole shape of the ventricles is lost, and the convolutions form little more than a lining to the skull. Such an accumulation of fluid can have taken place only by the gradual and very slow deposition of it, a deposition often congenital, and, when it begins in infancy or childhood, not infrequently the immediate consequence of a high degree of congestion, induced by an enfeebled state of the system.

Coagulable lymph, or purulent matter, poured into the ventricles.—When, instead of serous fluid, you find coagulable lymph or purulent matter poured into the ventricles, in the absence of all evidence of violence, you may consider these undoubted results

of inflammation as induced by some natural cause; but you should always bear in mind that these results are very commonly the immediate consequence of inflammation induced by external violence.

Effusion of blood into the ventricles.—And so also is the effusion of blood into the ventricles, which may happen from a violent blow, and which is the frequent result of concussion. But the effusion of blood into the ventricles is intimately connected with the consideration of apoplexy, which, of all the diseases of the nervous system, is, in relation to forensic medicine, the most important.

Apoplexy.—Apoplexy consists in the abolition of sensation, and the loss of motion. The extinction of these faculties may be complete or incomplete, and may be with or without convulsions.

Premontory symptoms.—Should you be called to ascertain the cause of death in a person who has died suddenly of apoplexy, alone and in the midst of strangers, and of whom no one can give any account, of course you can obtain no light from the existence of any symptoms that may have preceded the fatal event. All the knowledge you can gain in this case you must derive from the inspection of the body. But if the deceased have been struck with this disease in his usual abode, and in the midst of his companions and friends, whatever apprehensions or suspicions may have arisen, you may sometimes obtain so much light on the case, merely from the previous history of the symptoms, as to render any inspection of the brain needless—needless, that is, as the means of affording evidence that the death in question has resulted from a natural cause. But, of course, however complete the evidence of this may be to your own mind, you would never fail to examine the body carefully in all cases, to ascertain with certainty the exact cause of death.

Apoplexy often appears to come on quite suddenly, without giving the slightest indication of its approach, while, in reality, the premonitory symptoms are abundantly obvious to a careful observer. Its voice is not heard or not regarded; but it not the less sounds the alarm of the coming evil. By giddiness, headache, a sense of weight in the head, dulness of mind, a momentary loss of consciousness, peculiar noises, as ringing in the ears; depraved vision, slight defect of sensibility in particular parts of the body, frequent irregular or convulsive actions, stupor; palsy, partial perhaps, and slight, but not the less significant, it announces that it is at hand. It is true that severe, and even fatal, attacks do occur without any previous symptoms which have excited alarm. It is even stated by Rochoux, a

writer of authority on this disease, that out of sixty-three cases which came under his notice, only nine had distinct premonitory symptoms. This is a vastly greater proportion of perfectly sudden attacks of apoplexy than occurs to the observant practitioner in this country, who finds that, in the great majority of cases, some one or more of the premonitory symptoms which have been mentioned existed before the attack. You must not, then, attach too much importance to the absence of these premonitory symptoms; but if you find that they have been present, you have obtained one fact evidentiary that the death has arisen from a natural cause.

No age nor sex is secure from this disease, but it is more frequent in the male than the female; and it chiefly attacks persons advanced in years. You may meet with it in the infant, but this you would note as an extraordinary occurrence, whereas you would expect it in the old. A case recently occurred in this city in which it attacked a child two years and a half old, and on examining the head there was found a large extravasation of blood at the surface of the brain. I have myself recently met with three cases of it in my own practice in persons all of whom were under thirty, and one scarcely twenty. Dr. Craigie observes, that of eighteen cases described by Bonetus, five occurred in persons above sixty, and six in persons below forty. Morgagni relates the cases of thirty apoplectic persons, seventeen of whom were above the age of sixty, and five below that of forty. Of thirty-one cases of bloody extravasation in the ventricles or the substance of the brain recorded by Lieutaud, one was at the age of twenty-five; eight between the ages of thirty and forty-one; eleven between forty-one and fifty-one; six between fifty-one and sixty-one; two between sixty-one and seventy-one; two between seventy-one and eighty-one; and one only above one hundred. Of twenty-nine cases seen and dissected by Portal, two were between nineteen and twenty-three; four between thirty-one and forty-one; seven between forty-one and fifty-one; eight between fifty-one and sixty-one; four between sixty-one and seventy-one; and the same number between seventy-one and eighty-one. Of six cases of cerebral hæmorrhage given by Cheyne, three were between thirty and thirty-five; two at fifty; and one at sixty-three. Of the sixty-three cases given by Rochoux, two occurred between the ages of twenty and thirty; eight between that of thirty and forty; seven between that of forty and fifty; ten between that of fifty and sixty; twenty-three between that of sixty and seventy; twelve between that of seventy and eighty; and only one between

that of eighty and ninety. According to this view apoplexy is extremely rare before the thirtieth year; from that period to the fiftieth it is not common, but may occur; after fifty it becomes more common; between sixty and seventy is more frequent, becomes of the same rate of frequency after seventy as before sixty, and is very rare after the eightieth year. Of the cases given by Lermier and Serres, though two are between thirty and thirty-five, the greater part are between sixty and seventy-five. From the whole, then, it is plain that apoplexy is rare in the young, and common in the old. If, therefore, the person be of advanced age, you have obtained another fact evidentiary that the death has arisen from a natural cause.

It is also certain that there is a close connexion between corpulence and apoplexy; for the same circumstances predispose to both conditions of the system—a vigorous constitution and good digestive powers. The stout and close-built frame, the broad chest, the short neck, the large head, the full and florid cheek, the brawny limbs, have been so often found in connexion with this disease, that they have been conceived to form the apoplectic constitution. Apoplexy may, indeed, attack the tall and thin, but the pletoric frame is much more predisposed to that irregular action of the arterial system on which this disease essentially depends. While, then, you must not attach too much importance to the absence of the apoplectic conformation, yet, if it be clearly and strikingly present, you have obtained a third fact evidentiary that the death has arisen from a natural cause.

The immediate cause of apoplexy is often a full meal; consisting, perhaps, of indigestible food and stimulating drink. Articles of food and drink, which may be taken with impunity, and even advantage, by those in sound health, may prove highly dangerous, nay, even instantaneously fatal, to those strongly predisposed to apoplexy. If the death in question has occurred after a full and stimulating meal, and more especially immediately after, or even during such a meal, in a person accustomed to habitual indulgence amounting to intemperance, you will have obtained a fourth fact evidentiary that death has arisen from a natural cause.

Great anxiety of mind, violent emotion, whether depressing or exciting, and severe long-continued and uninterrupted mental exertion, to the exclusion of physical exercise, lead to such a derangement in the circulation of the blood-vessels of the brain, as to prepare the way for apoplexy. If, then, you find that circumstances of this kind have existed in a powerful degree for a considerable time, and more

especially that they have concurred in a person in other respects predisposed to apoplexy, you will have obtained a fifth fact evidentiary that the death has arisen from a natural cause.

Severe blows inflicted on the head, the immediate effect of which may have ceased for a long time, and have been forgotten, sometimes lay the foundation of apoplexy, and must be regarded as an occasional very powerful predisposing cause of the disease.

Much importance cannot be attached to one of these circumstances alone; but the presumption will strengthen as the number of individual facts concur in the same subject, according to that cumulative power which I have already shewn to be characteristic of evidence.

But whatever force there may be in these predisposing causes, and however they may assist the diagnosis in certain difficult and doubtful cases, still the only absolutely certain signs of the existence of this disease, and the operation of it as the cause of death, are to be found in the morbid conditions of the brain; and with these it is of the last importance, in a judicial point of view, that your knowledge should be exact and complete.

LECTURES ON TUMORS,

Delivered at St. George's Hospital,

BY CESAR HAWKINS, ESQ.

I. ENCYSTED TUMORS, *continued.*

2. *Serous or Aqueous Encysted Tumors.*
3. *Hydatid Encysted Tumors.*
4. *Congenital Encysted Tumors.*
5. *Bursal Encysted Tumors.*

I.—2. *Serous or Aqueous Encysted Tumors.*

THE second genus of encysted tumors may be called the aqueous or serous encysted tumors, and they consist originally of a thin delicate secreting cyst, containing a transparent fluid, like water in appearance, and composed of pure water, with a small quantity of animal matter, which Dr. Marcet has termed muco-extractive matter. This fluid is not coagulable by heat, since it contains no albumen, as serum does, and therefore the term *serous* has been given, you will observe, not to the secretion, which is aqueous, but to the cyst, which is like the serous membranes in appearance, especially the arachnoid membrane, which, you are aware, secretes scarcely any albumen naturally. You will find a minute account of these tumors, especially as to the liver and kidney, in a paper of mine, published in the 18th vol. of the *Medico-Chirurgical Transactions*. These cysts have also been called *hydatids*.

but very erroneously, since they are fixed, and attached to, and nourished by, the vessels of the surrounding parts, and are not parasitic animals, which will be hereafter described to you. They are termed *spurious hydatids* by Mr. Hunter, but I think either of the names I have mentioned is better.

It is conjectured by Dr. Hodgkin, that they originate in the obstruction of excretory ducts, and Sir A. Cooper assigns the same origin to an analogous disease of the testis. Now there is no doubt that cysts of fluid are often formed in some glandular structures, such as the breast and kidney, in this manner, and in the same way also in the ducts of the salivary glands, and I believe the mucous follicles are obstructed so as to form similar tumors about the vagina and elsewhere. It may be objected, perhaps, that tumors thus formed should be termed *mucous encysted tumors*; but, in fact, their secretion, when thus confined and changed, is no longer mucus, but the cysts become exactly like serous membranes, and the secretion from them is often transparent water. All this is no more than we should expect *a priori*, as analogous to the origin of the sebaceous encysted tumors. But we must not forget that this will only account for some such tumors, and that serous encysted tumors are met with in the cellular tissue of organs that have no secreting property, and no ducts to be obstructed. Look, for instance, at this plate of Dr. Hooper's, of cysts of this kind in the brain, or at a preparation from a patient of Dr. Macleod's recently, in which you may see two large serous encysted tumors in the same organ. Here, then, they must arise in a different way; possibly by enlargement of the cells of the cellular tissue: whether the fluid secreted in them is first prevented by some peculiarity from being absorbed, and the cyst subsequently condensed, or whether the secreting cyst is first organized in a peculiar way, may admit, indeed, of dispute.

After a time, changes often take place in the cyst and in its secretion, altering the appearance of the disease. The fluid becomes more mucous and tenacious, but still transparent, or brownish and opaque; or some albumen is deposited, generally sinking to the bottom, or lining the cyst; or the fluid is mixed with blood, though still transparent, or dark and thick, like currant jelly; or green, or black, like ink; or sometimes mixed with pus. So also the cyst sometimes loses its delicacy and transparency, and becomes thick and opaque, like the reflected pericardium, or tough and fibrous, like the capsules of some of the joints; and sometimes, also, the cysts become partly ossified.

Both the names given to this genus be-

come, therefore, inapplicable to every stage of the disease, and a new one would be necessary for every change in the fluid, or in the cyst; we may, however, without much impropriety, retain the name for every encysted tumor, which probably consists originally of a simple serous cyst, secreting an aqueous fluid.

You see the disease before you in various organs, in these preparations from the brain, ovary, liver, lungs, testis, breast, spermatic cord, neck, thigh, and so on; in fact, scarcely any part seems exempt from their formation.

Examine these tumors in the living person, and you find a smooth, firm, elastic, rounded tumor, in which fluctuation is very evident when you tap it, unless the sac is very small and much distended, in which case they sometimes feel very solid and hard. You find them of every size; from that of a pea to a sac containing several gallons of fluid, and yet still retaining its original appearance. If, however, the sac has become very thick and fibrous, the diagnosis between a tumor of this kind and a solid tumor becomes more difficult, though with care you will seldom fail to distinguish the fluctuation of fluid even when they are deeply situated, as in the abdomen, in which situation they acquire the greatest size; probably from the inferior degree of pressure to which they are subjected. Their deeper situation will almost always distinguish them from the last genus (the sebaceous encysted tumors), which can, of course, only be formed beneath the skin. Their general freedom from inflammation, and its attendant symptoms, will usually distinguish them from abscesses, although, if they grow very rapidly, they sometimes occasion a good deal of pain, and even considerable constitutional irritation; and they are most like the two next genera of tumors, from which, in some instances, there can be no means of distinction till they are dissected.

The treatment of serous encysted tumors must depend partly on their size and situation, partly on the condition of the sac and the nature of its contents.

1. If they are superficial, and the sac thin, and the fluid watery, or nearly so, you will sometimes succeed in producing absorption of the fluid and obliteration of the cyst, by external stimulation. You may try this lotion:—

R. Muriat. Ammoniacæ, ʒij.; Aceti, ʒix.;
Sp. Vini Rectif. ʒiiij. M. ft. Lotion.

If this excoriates the skin, the following may succeed:—

R. Liqueur. Plumbi Subacet. ʒj.; Sp.
Camphoræ, ʒviij. M. ft. Lotion.

By something of this kind, assiduously

applied on linen, even large tumors will sometimes be cured, when near the surface.

2dly. They may be sometimes cured by puncturing the cyst, and evacuating its contents by means of a grooved needle, or a trocar. On the exterior of the body this plan will generally be a palliative only. A girl was under my care for a long time, with a tumor at the side of the neck, who was unwilling to have measures adopted for its perfect cure, but was relieved from the inconveniences of its pressure upon the larynx and œsophagus, by evacuating about eight ounces of aqueous fluid, from time to time, by a small trocar. In the interior of the body a single puncture will very often obliterate the sac, probably because very effectual pressure can be made, so as to keep the sides in contact. You have very lately seen me evacuate twelve pints of pure water from the abdomen of a young woman in whom the tumor had existed four years, and did not appear to be connected with the ovary, though I cannot say to what part the sac, that no doubt contained the fluid, was attached, unless it was the liver, as Dr. Chambers believes. A few years ago I saw Sir Benjamin Brodie puncture a cyst of this kind, in the liver of a boy of 12 years of age, a patient of Dr. Chambers, and let out a pint and a half of pure water; and the boy seemed to be cured when he left the hospital. In another case, Sir B. Brodie had an opportunity of seeing a young lady five years after the same operation for a serous encysted tumor of the liver, and no return of the disease had taken place.

3dly. Although you cannot at once procure the absorption of the fluid by external irritants, nor by simple evacuation of the contents of the cyst, yet you may succeed by using both means; the secretion of the cyst, when it fills again, being sometimes different from what it had been before the puncture (just as you will find to be the case with regard to enlarged bursæ), and yielding to the lotions you had employed without success before the puncture. A woman had a good deal of inconvenience from a tumor, the size of an orange, in front of the trachea; and having punctured it with a needle, and let out four ounces of dark-red fluid, I applied an irritating plaster of ammoniacum, with a little powdered cantharides sprinkled on it. The cyst partly filled again, but then became smaller, and entirely disappeared in five or six weeks. These simple means may then always be tried before having recourse to the more severe; and if they fail, they do not at all interfere with what I have next to recommend.

4thly; Then if adhesive inflammation is insufficient to cause the obliteration

of the sac, suppurative inflammation will probably succeed, which may be excited by making a moderate incision into the tumor, and dressing it lightly with lint to the bottom; or by means of a seton through the longest diameter, as recommended by Maunoir, of Geneva, and by Dr. O'Beirne, of Dublin, for what the former terms hydrocele of the neck, *i. e.* an aqueous encysted tumor in that situation, where it is not uncommon, as you will already have seen.

Both these methods are often employed for encysted hydrocele of the spermatic cord, which is this disease of that part. The unfortunate Delpach, whose tragical end by the hand of one of his own patients excited so much sympathy a few years ago, cured some very large ones in the orbit and neck by dressing with lint, one containing as much two pints of fluid; but I should tell you that one patient died from the sac passing through the orbit into the cranium. For a circumscribed and accessible tumor I like free incision, and dressing with lint; for a large and irregularly-shaped tumor among muscles, the seton is to be preferred, as the irritation sometimes requires to be continued for several months before the sac contracts enough to be finally obliterated; and you must be prepared in important situations, such as that in the neck, for a good deal of local irritation and severe constitutional disturbance, from either plan of treatment; and if small and not inconvenient, and not growing, you should not unnecessarily expose your patient to risk. If you use a seton, you may make an incision at one part, and through this introduce a long straight or curved trocar, and pass it out in the opposite direction; and through the canula some silk may be passed.

But 5thly, you can scarcely hope that a tough fibrous cyst, or one mixed with bone, will be removed by absorption, or filled up by granulation; and therefore it must either be destroyed by caustic, or removed. Having, therefore, opened the cyst freely, you may rub the interior with strong nitric acid, applied by means of some linen tied on the end of a small piece of wood. A man, 55 years of age, had a tumor in the neck, divided by the sterno-mastoid muscle into two portions, one of which was situated in the triangular space between the mastoid and the trapezius muscles; the other projected on the inside of the sterno-mastoid muscle, pushing the larynx to the right side, and making him disturb every one by his snoring at night, and interfering much with the functions of the parts. It was dark-coloured, and seemed disposed to burst. I punctured it, and let out ten or twelve ounces of transparent fluid, and a

second time a thicker fluid, mixed with blood. As it filled again rapidly, I made an incision into it, and rubbed the inner surface with nitric acid, the inside being irregular from bands of ligamentous structure in several directions. I felt the scaleni muscles behind, on the outside of the sterno-mastoid, and other muscles from the sternum in front; the larynx and trachea were attached to the cyst at its inner extremity, and I could feel the vessels at the back part in contact also with it. By the fourth day the cavity had contracted to a small size, and the sloughs had almost separated without much inflammation, when he was unfortunately attacked with fatal erysipelas. This circumstance might, of course, follow any mode of treatment; still you should reserve this plan to the last, as attended with greater risk than the preceding methods of treatment.

Lastly, you will see here some cysts which have been entirely dissected out; and it accessible, and the sac be fibrous or osseous, this operation is to be preferred to the caustics; but it is often quite inadmissible. A young child was a patient of Mr. Keate's in this place, with a cyst, which I saw him puncture, in the nates, which evidently came out of the sciatic notch, beyond reach of the knife; a child, two years of age, was a patient of Mr. Babington's, with one or several cysts, reaching from the axilla to the neck, through all the vessels and nerves of that cavity; and the case I have just now mentioned shews you what you would meet with in the neck. Here is a preparation of a cyst in the neck, mixed with the solid matter of fungus hæmatodes, and which I therefore only punctured to let out the fluid; but observe what I should have encountered if I had tried to remove it: here is the jugular vein in front, the nerve at the side, and behind it; three inches from the vein, is the carotid artery, and all closely attached to the cyst. Even if not mixed with malignant substance, be very cautious how you remove these serous encysted tumors in the neck. Dr. Warren, in the work I have before alluded to, well remarks, how tempting they sometimes look, as if quite detached, while in reality processes of the tumor may dip into very important situations; and he relates two cases, in one of which he tied the internal jugular vein, held aside the par vagum, and dissected off the common carotid artery; and in the other, the internal carotid artery and jugular vein were both left insulated, after a difficult dissection, in the upper part of the neck. He very properly thought the operations, when he had completed them, scarcely justifiable with his knowledge of the connexions of the cysts; and very likely the cases might

have been cured by the other means which I have described. In all operations on the neck for tumors, you will do well also to recollect, that air may get into the veins, and cause an instant death.

All these operations, however, are occasionally liable to be attended with dangers. Having punctured the cyst, it may inflame, when you do not wish it to do so, as when situated in the interior of the body, especially if growing rapidly before the puncture. A little boy had a tumor on the right side of the abdomen, occasioned by a blow six weeks before his admission into the hospital, under the care of Dr. Seymour and myself. As he was suffering very much, I punctured it, and evacuated eighteen ounces of water, shewing, therefore, that it was one of these cysts, the nature of the case having been previously obscure. When he died from the rapid growth of the tumor afterwards, I found it nearly filling the abdomen, and even projecting below Poupart's ligament. I gave an account of this case, along with the description of these tumors in the liver, in the *Medico-Chirurgical Transactions*; but you may see in this preparation the thinness of the cyst when containing five pints of water mixed with pus, and the curious circumstance of there being a separate kidney removed from the right, and both attached to the cyst.

Here is another of these serous encysted tumors from the neck, which was punctured, but bled so largely, from an unhealthy state of vessels, that it was obliged to be removed by Sir Benjamin Brodie; but the patient died of sloughing. Occasionally when opened a peculiar action takes place, and a fungus arises, sometimes called malignant, though erroneously. In a patient of Mr. Walker's in the hospital, with a cyst of this kind in the breast, a large unhealthy fungus, three inches in diameter, with great pain and excessive factor took place, requiring the amputation of the breast; the case did well, however; nor, as far as I know, is there any reason in such cases to expect a return of the disease from any thing malignant in its nature. Perhaps this fungous growth is more likely to ensue when only a small puncture is made and left open, or when the cyst is thick and fibrous, than when it is freely opened, and the sac still thin.

A man was admitted into the hospital, with a tumor of the liver of some standing, lately attended with increase of suffering, which I punctured, and let out a good deal of fluid, consisting of pus mixed with watery mucus, and tinged with bile. He was relieved for some time, and the cavity was nearly filled up, when it began to bleed a good deal, the edges ulcerated, and an unhealthy bleeding and sloughing fungus took

place, very much like the appearance of a malignant disease, and attended with so much pain, that his sufferings were hardly alleviated by six grains of acetate of morphia daily. He became gradually emaciated, and died five months after the puncture, at which time the sloughing ulcer was fourteen inches broad by ten in its shortest diameter. Here is a preparation and a cast taken from a patient of Mr. Babington's, who died exactly in the same way, though the disease was of smaller extent. The ulceration here was so very like that of fungus hæmatodes that Mr. Babington could not help suspecting it was of that nature, though he was acquainted with my previous case. Now here there was no disease in the substance of the liver, and no hardness like that of an abscess of this organ, but only a little body of the size of a nut, that I am inclined to believe was an encysted tumor obliterated after the puncture, but assuming an unhealthy disposition analogous to that previously mentioned in the breast.

There is a variety of tumor described by some persons, in which a cyst at first appearing to be of the kind we are discussing, becomes filled with solid matter growing from one side of the cyst, and ulcerating and projecting through the opposite side. Mr. Abernethy describes one such case, where the cyst was cut across in amputating the arm, where the fungus grew again, and destroyed the patient. I cannot say I have seen any thing of this kind in an encysted tumor of the kind I have placed before you, and I suspect the cases in question are instances of cysts with fungus hæmatodes, and not of serous encysted tumors. Here is a little preparation said to be of this sort, however, but I do not know its history.

Such is a brief account of this kind of tumor, but when situated in important situations other mischiefs may be produced. In the chest they induce oppression of breathing and suffocation;—in the brain fatal irritation of that organ;—in the liver they may give rise to anasarca and ascites, and occasion the death of the patient from their immense size, as much as twenty pints of liquid having been found in them, besides the fluid in the peritoneum. In one case I recollect a man being brought into the hospital in consequence of a fall from a waggon in which a basket struck him and ruptured one of these cysts of large size in the liver, and occasioned his death in a few hours, much of the substance of the liver having been absorbed by pressure.

1. 3. *Hydatid Encysted Tumors.*

The third genus of encysted tumors is composed of hydatids, and they may be

found in many of the situations in which the serous encysted tumors occur. We have seen that the serous cysts are sometimes called hydatids, but erroneously, because the cysts are nourished by the vessels of the surrounding part; the hydatid-cysts, on the contrary, contain parasitic animals, zoophytes, which are nourished by imbibition only, by means of their own coats, from the fluids of the part in which they are situated. They are for the most part mere globular bags, situated in some cavity of a living animal, and are the *trachia hydatidea* of Linnaeus. In the lower animals they generally have a distinct head, sometimes elevated on a kind of neck, with an opening through it into the great bag; they are found in immense numbers in sheep, sometimes in the liver, in what is called the rot, or in the brain in the disease termed the staggers, and they are often accompanied by the flat worms, the flukes; placed in warm water they evidently appear to have a kind of voluntary motion, and power of changing their position. The human hydatid has less evidence of distinct vitality, being seldom possessed of more than a contractile property by which it rolls up in a curious way, as you may here see; it is the *hydatid accephalocystis* of Rudolphi, scarcely ever being seen with a mouth or head; but occasionally the more perfect animal, the *hydatid cysticercus*, is found in the human subject mixed with the accephalocysts. The parent hydatid appears to have a power of generating others, which are attached at first to the interior of the original one, covered by a fine membrane, whence after a time they drop off, and float in a kind of aqueous fluid which fills each bag, of which there are sometimes an immense number, as you may see in these preparations. The smallest of them, when formed in this manner, are less than the globules of the blood, while others, as you see, are an inch or two in diameter.

Around them there is generally formed a cyst of cellular structure, like the serous cysts in appearance, which, like them too, becomes thick and fibrous, or even osseous, as in this example; but occasionally there is scarcely any thing that deserves the name of cyst around the hydatids. The cellular cyst secretes a fluid that sometimes becomes thick and adhesive, and even purulent, in which case the fluid within the hydatids still remains thin and transparent, being changed by the independent vital properties of the coats which compose them, and undergoing a kind of assimilation. Sometimes, where the parent hydatid is large, the cyst is lined by a thick unctuous secretion instead of fluid, and occasionally it becomes quite filled

with hydatids, broken down, and pressed together into a mass of the consistence of isinglass-jelly.

Even the largest hydatids have no attachment to the cyst in which they are contained. Here is a part of one, for instance, removed by Mr. Keate from the breast, where three pints of fluid were evacuated by a puncture, and this single large bag was easily pulled out from the cavity in which it lay quite unattached. The cellular cyst in which these globular bodies are contained is often mistaken for the parent hydatid; but you will recollect, from the name I have given you of hydatid encysted tumor, that there is the *attached cyst*, containing loose hydatids and fluid.

How they become developed in the living body in such enormous quantities is a curious subject of inquiry—whether by spontaneous generation, as some persons imagine, or whether (as is more probable) the germs are conveyed by the mucous membranes or blood-vessels, through the textures of the body, till they meet with some appropriate nidus, in a frame fitted by ill health, or disease of some kind or other, to favour their retention and growth.

The hydatid tumor is most common in the liver, in which situation most of these preparations were found, but they may be met with elsewhere, and I have already mentioned their occurrence in the breast and the brain; in fact, there is scarcely any part of the body in which they have not been seen by some person or other. The diagnosis of these tumors will be much influenced by the thinness or solidity of the cyst, and by the quantity and consistence of the fluid in which the hydatids float, and by the number and size of the hydatids themselves. The cyst will sometimes fluctuate distinctly, if the number of hydatids is small compared with the fluid, or if the hydatids are themselves large, so as contain much fluid; at another time it vibrates rather than fluctuates, when the hydatids are very numerous and small, or the fluid of the cyst is scanty, or thick and gelatinous; sometimes, again, the cyst may be so firm, or so filled with hydatids, as to appear almost solid, especially when deeply situated. Sometimes they produce little inconvenience, even when of large size, so as to be distinguished by this circumstance from a collection of fluid pus; but on the whole they occasion more pain and impediment to the functions of the parts in which they reside than serous encysted tumors, which also generally fluctuate more distinctly than the hydatids. Sometimes they inflame and suppurate, but even then the

existence of the semifluid tumor, for a definite time, before symptoms of suppuration shewed themselves, will serve as a guide to the probable nature of the disease.

In parts of the body which have an external communication, they occasion little disturbance of the general health. I have seen them discharged by the bladder from the kidney for a great length of time, with only occasional inconvenience from their obstructing the urethra. Not a very uncommon situation for their growth is, however, behind the bladder, where, having no exit, they have produced fatal pressure upon this organ, with consequent disease of the kidneys. So, also, in the brain or heart, although they have been found, when unsuspected during life, they can scarcely increase much without serious consequence. Sometimes it would seem that the containing cyst in the liver or spleen bursts, and some hydatids escape into the peritoneum; sometimes a vast number of separate cysts of hydatids are found in various parts of the abdomen, amounting to many pounds weight, in which case an irregular surface of vibrating tumors would be a diagnostic sign. What are called hydatids are often found in the uterus, as in these preparations; but I doubt their nature, since you may see an attached surface from which they grow, and they are fixed to one another like bunches of grapes, so that they must have a different mode of generation, and are, in fact, all attached intermediately or directly to the uterus. These tumors grow to a large size, and are the result, as it appears, of conception with disease of the membranes of the ovum; and although discharged from the uterus by means of its external opening, the disease is frequently fatal, by the severe symptoms occasioned by their propulsion.

Occasionally it would seem as if the hydatids cease to increase, and the cyst remains stationary, or contracts upon the dead animals. More commonly their death induces inflammation and suppuration in the containing cyst; and I have seen several pints of broken down hydatids, mixed with semi-purulent fluid, evacuated by the trocar. Sometimes, even after pus has been formed, a natural cure has been effected by the tumor ulcerating towards one of the surfaces of the body. Mr. Keate mentioned to me the case of a gentleman who was for a long time ill with some obscure disease, who suddenly had a kind of diarrhoea, in which one potful after another of hydatids was evacuated, from some encysted tumor of this kind giving way. A lady was under Dr. Seymour's care with a tumor of the liver,

which disappeared, and the patient recovered, after the hydatids were discharged by stool. Sometimes the tumor in the liver ulcerates apparently into the duodenum, instead of the colon, and these bodies have been vomited as well as discharged downwards. At other times a communication has been established through the diaphragm, and the hydatids from a cyst in the liver have been coughed up for years through the lungs. In all these cases the patient may get well, and has, perhaps, quite as good a chance of surviving as when an opening is formed either spontaneously or artificially, on the surface of the body. Whether opened or not, however, they may occasion death by rupture, or irritation, in the same way as the serous encysted tumors. For a detailed account of them in the liver I may refer you to the same papers I have alluded to in the *Medico-Chirurgical Transactions*.

Next as to the treatment of these cases of hydatid encysted tumors; which, however, must necessarily vary according to their situation or connexion as well as their actual state.

1. Can you check their increase and get rid of the disease without operation? One of these preparations, I believe, is from a patient of Dr. Chambers's, a young woman, in this hospital, whose tumor in the liver very much subsided under the use of iodine in friction. I apprehend, however, we cannot reasonably expect, from any treatment of this sort, more than a temporary diminution of the size of the tumor from absorption of its fluid part; at least I conceive it can scarcely kill the hydatids, or if it did so, leave them quiet afterwards. I should expect to see what took place in this case, namely, that having some relief for a time, the patient was obliged to return to the hospital, and died of the disease about a year afterwards, with suppuration in the sac. It can do little harm, however, to try for a time the stimulant lotions I before spoke of, as sometimes curing the serous encysted tumors. In some internal organs we can do little more than administer those medicines, such as tonics and alteratives, which are called for by the state of the system, and which may possibly affect the general health so as to retard their growth, or prevent their development in some other situation, or their return, if one such cyst has been cured. Or else we may remedy those symptoms of irritation from pressure which attend their rapid growth, and relieve the further evils which result from their inflammation and suppuration.

2. The tumors may be opened, the hydatids evacuated, and pressure applied, by which means the sides of the containing

cyst will often be made to adhere, so as to become obliterated. This was the case with this large one from the breast, in which Mr. Keate made a puncture with a lancet and drew out the hydatids, and these bodies are so soft and brittle, that a large one, or great numbers of small ones, will come through a small aperture. In some situations, however, as in the liver, or other internal or deep situations, a trocar is preferable to the lancet, from the canula preventing the alteration of position of the textures through which the puncture has been made, from closing the opening. It has been recommended by Recamier, and some other persons, to make an opening by means of an eschar, successively destroying the skin and the subjacent parts, till the sac is reached, both for the aqueous and hydatid encysted tumors, the object of which mode is to procure adhesion of the peritoneal surfaces, and thus prevent extravasation into the cavity of this membrane. I presume, however, the operation will not lightly be performed in the liver or other important viscus, so that adhesion will generally have taken place before the operation; and extravasation is not very probable, even if it has not done so, since the cyst must at least be in contact with the parietes, or we could not make the puncture, and in that case pressure, by means of bandages, and long straps of adhesive plaster, would almost certainly keep them in contact after the puncture. I cannot approve of this caustic mode, if no inflammation has taken place, since its adoption under such circumstances wholly precludes any chance of the sac being obliterated by adhesive inflammation after the puncture, and the chance of mischief from the open suppurating cavity, which is, of course, invariably produced by the caustic, is, in my opinion, more dangerous than the occasional probability of inflammation from extravasation of the fluid of the cyst, which is not an irritating fluid, unless it happens to contain bile, when situated in the liver.

As I have only once or twice seen hydatids in the limbs, let me mention a case in this situation, to show you how reasonably we may hope to procure union by the first intention, and obliteration of the sac, when an hydatid encysted tumor requires a puncture, from its size. It is published in the *MEDICAL GAZETTE*, by an intelligent surgeon (Mr. Attenburrow, of Nottingham), whose son was formerly a pupil of mine. A tumor began about the groin, a month after a fall, and in eleven months increased so as to reach from the pubes two-thirds down the thigh, and to measure twelve inches long, nineteen broad, and thirty-three in circum-

ference. So large was it, therefore, that I think much credit is due to the surgeon for distinguishing the tumor from one of fungus hematodes. Mr. Attenburrow punctured it with a trocar, and evacuated seven pints of fluid, with quantities of hydatids; a bandage was then applied, and the cavity became obliterated, and the patient got well.

Such, then, is the simplest mode of proceeding with hydatid tumors, taking care not to produce injurious pressure; and with this view using a cupping-glass sometimes, rather than the hands, to break down and draw out the hydatids, and watching carefully the appearance of the tumor and the condition of the system after the operation; so that if it inflames and suppurates, which it does more frequently than after the puncture of the aqueous encysted tumor, we may directly open it freely, and give exit to the secretions of the inflamed sac.

3. If, then, the sac has suppurated previously to any operation being practised, the same operation becomes necessary, but the orifice should now be left open after the puncture, instead of being allowed to close. If the tumor is easily accessible it should be freely opened, so that no pressure may be necessary; and a small and insufficient opening is more likely to be followed by irritation, and that unhealthy inflammation and fungous growth, which I before mentioned as occasionally following the puncture of the aqueous encysted tumor. In important situations, however, as in the liver, the trocar of some size should be used, through the canula of which a catheter may be passed after the sac has been emptied, and in a short time the orifice may be enlarged, if necessary, when time has been afforded for more extensive adhesion of the sac to the peritoneum; or if you please you may open the suppurating cyst by caustic (if the symptoms are not so urgent as to require a more immediate evacuation), to which method there is not now the objection I formerly made to it in the uninfamed sac.

Can we facilitate the obliteration of the sac by any injections? I have not seen any used myself, for the patients have either died after the puncture, before the period of quiet suppuration has arrived, when the puncture has been made in an important situation, or else, when not dangerous, the cyst has filled up, and contracted readily, without them. Injections have, however, been recommended and practised by Recamier with success as it would appear, nor is there any objection, that I am aware of, to their use, if the sac is quiet, and does not easily contract or adhere, beginning with warm water, and afterwards employing a small quantity of

spirit and water, or a weak solution of chloride of lime, or other mild stimulant. When the diseases of the bones are described to you by Mr. Babington, he will very probably mention a very interesting case of Mr. Keate's, in which he evacuated some hydatids from the diploe of the cranium, by the trephine, with success; and I believe, in sheep, they have sometimes been removed by this operation from within the cranium.

4. Sometimes, lastly, the entire removal of the sac with its contents may be practised, in situations where it is easily accessible, and not of large size, and the sac is thick and fibrous. Here is a sac containing an hydatid removed by Mr. Babington from the cellular texture of the thigh, about three inches in diameter, and the sac something like fascia in appearance. It is only in cases such as I have just mentioned that I should be inclined to remove the whole tumor by the knife, rather than lay it open, and in many cases, even in the extremities, the excision must be quite impracticable, as in Mr. Attenburrow's case for instance.

I. 4. *Congenital Encysted Tumors.*

The fourth genus of encysted tumors I will call *congenital encysted tumors*, by which I do not mean all which are found at the time of birth; for instance, a young infant has recently been under my care for some encysted tumors about the neck and face and tongue, making the side on which they occurred double the size of the other. Several of these I punctured, and let out some fluid of different kinds in each, but for the most part like currant-jelly, and most of these cysts did not fill again, so that comparatively little swelling is left; but these were of the second genus, the serous encysted tumors, developed soon after birth. Neither do I mean that all tumors of the kind I am going to describe are discovered at the time of birth, but only that if formed and developed later in life they are of the same kind as those which are most usually congenital.

There are thin cysts met with, generally in young infants, containing a great variety of substances of a peculiar kind. Sometimes there is a quantity of substance like adipocire. I assisted Sir Benjamin Brodie in removing a large tumor of this kind from the back of the neck, of the most offensive odour, which was situated too deeply to have been a large sebaceous encysted tumor. If I recollect right, a tumor which some of you have recently seen removed from the parotid gland by Mr. Walker contained some substance of this sort. Sometimes there is a quantity of fatty substance of a peculiar kind, which has been termed stearine by Dr. Bostock, which often con-

tains mixed up with it a mass of long hair. There is here a portion of a large ovarian tumor, which I had tapped in this hospital, and evacuated about sixteen pints of dark liquid, and in which, after death, a mass of fat, half a pound in weight, was found mixed with another mass several inches in length, of long rolled-up hair. This is a drawing of it from Dr. Seymour's beautiful work on the ovaries, whose patient she was, while in the hospital. Sometimes the cyst contains some shining micaceous particles, like fat kept long in spirit, which has been called margaritic acid by Dr. Prout and Cherreul. At another time, the contents are very like a ripe medlar, or rotten apple, in appearance.

These tumors occur in various situations. Here is a small tumor of fat and hair from the orbit, which is not an unfrequent locality for this genus of congenital encysted tumors, as well as for both the two last genera, and where the operation is sometimes difficult, from the intricate manner in which portions of the tumor sometimes dip down between the muscles and nerves, and where also the cyst is sometimes found attached to the bone. Another very common situation is a cavity in the ovary or broad ligaments of the uterus. I have had two patients with tumors apparently of this kind by the side of the vagina, occasioning a good deal of trouble and pain. One of these I laid open in a pregnant woman, who was to have returned to me, if the tumor formed again, after her confinement; the other suppurred suddenly, after having been five years at least in forming. The most singular kind of tumor, however, of this genus, is when the cyst contains not only hair and fat, where none is naturally found to determine the peculiar growth by its analogy, but has also in it teeth and portions of bone, especially the jaw-bones, or other parts, of a *fœtus*. They are most frequently found in the ovary, whence this sac with teeth and hair was taken in a patient in this hospital, and of which another instance occurred here very recently, though I know not what became of the preparation, and from this locality they have sometimes been regarded as instances of extra-uterine conception, but on very insufficient grounds, for they are found in other situations also. Mr. Barnes, of Exeter, met with teeth in the orbit, and such tumors have been found in the stomach and bladder; Sir Benjamin Brodie, I think, saw one in this last situation. They are met with in virgins, and in the male sex, and in animals of no sex at all, since Mr. Coleman met with a tumor of this kind in the thigh of a gelding.

Such portions of fetuses in congenital encysted tumors are not so common, however, as the other substances I have alluded to, and I should have little doubt that they are examples of double conception, *i. e.* of an imperfect fetus united with or included in another, of which many instances are on record, from the nearly perfect fetus found in the abdomen of a boy by Mr. Highmore, to the cysts containing one or two teeth, or a little hair and fat. There is here a plate from Cruveilhier of a tumor with hair and fat; the hair is unravelled, and you may see that it has been attached to what seems to be a portion of scalp at the side of the cyst. In other cases you do not find any substance from which the hair grows; and with regard to the other peculiar contents of the cysts which I have placed in this genus, it is possible that I am wrong in classifying them thus, and that they should only be looked upon as some peculiar change in the secretion of the serous encysted tumors. If this be so, of course the genus of congenital encysted tumors must then be confined to those which evidently contain such substances as appear to be part of a fetus. Even in the latter case, however, you will understand that although the rudiments are thus commenced and attached in the fetal state, they need not be large at the time of birth, nor become much developed till long afterwards. Being connected with vessels to the part of the body in which they reside, some accidental circumstances may occasion an increased supply of blood to the part, and cause them to lose their stationary condition, and become rapidly increased in size; such, for instance, as the altered condition of the ovary at puberty or during pregnancy.

Whether I am right or wrong in thus bringing into one group all the kinds of cysts and their contents, which I have now enumerated, is not of much consequence, however, since I know not how you could always distinguish them from the two former genera till after the contents have been evacuated, or the cyst dissected out. You can perhaps ascertain the existence of an encysted tumor; but even this must depend upon the circumstance I have before spoken of with regard to the former genera; the quantity and consistence of the secreted materials—the mixture of solid matter—the thickness and distension of the sac, and so on. Nor is the perfect diagnosis of the tumor of much consequence in practice, since nearly the same circumstances which I formerly dwelt upon should guide us, I think, in our treatment of these. In some few, where the nature of the contents, the thinness of

the cyst, and the connexions of the tumor with the surrounding parts, render it favourable or desirable to do so, we may try the puncture of the sac, with a view to its obliteration by adhesive or suppurative inflammation; but the greater number, where accessible, are best treated by excision of the whole tumor, either entire, or by dissection of the cyst after the previous evacuation of its contents.

In some cases, where excision is unadvisable from the connexions of the cyst, we may, as with the serous encysted tumors, where simpler means fail, attempt to destroy the cyst by nitric acid or potassa fusca. In other cases, again, all operations are out of the question, and we are obliged to content ourselves with warding off as much as possible the consequences of the growth of the tumor, without trying measures which would be impossible, or too hazardous to be thought of with prudence and justice to our patient.

1. 5. *Bursal Encysted Tumors.*

The next genus of encysted tumors on our table are the bursal. These take place sometimes in the bursæ and their secretions, either as the consequence of pressure and inflammation, or else without our being able to trace the cause of certain changes, by which tumors are formed, not curable by the usual means suited to inflammation, but requiring the knife of the surgeon. Instead of synovia and the secretions of inflammation in such membranes, you have semi-fluid matter, like currant-jelly in colour and consistence, or otherwise altered; or masses of solid matter like melon-seeds, as in this preparation; and the cyst, instead of its usual structure, becomes, as you may see here, thick and fibrous, and the cavity is sometimes obliterated, so that a large solid tumor is formed.

In such cases the changes are so considerable, and the treatment required so different from what is necessary for inflammation of these textures, that they may not be inappropriately designated bursal encysted tumors. I shall not at present describe them, however, but leave them to be spoken of with the other diseases of the joints and bursæ, and shall content myself with observing, that when you meet with tumors containing more or less fluid, in the known or probable situation of the bursæ mucosæ, you should generally attribute their formation to these textures; and the recollection will assist your diagnosis in many otherwise obscure affections.

RECOLLECTIONS OF CHOLERA;

ITS NATURE AND TREATMENT.

BY WILLIAM GRIFFIN, M.D.

Limerick.

No. VIII.

*Of Sedatives, or Contra-Stimulants—
Calomel—Opium—Brandy—Emetic
Tartar—Ipecacuanha, &c.*

THE first practice almost universally resorted to in this country was that which came so highly recommended from India—the exhibition of calomel in large and repeated doses. It was adopted at St. Manehin's hospital, in connexion with those other means suggested in the Board of Health circulars—emetics, sinapisms, injections, dry heat, &c.—and the results proving very unsatisfactory, opium stimulants, salines, turpentine frictions, turpentine and tobacco injections, &c., were successively resorted to. These remedies appeared as unsatisfactory in their results as the calomel had been; and after having witnessed or conducted the treatment of 44 cases, of whom 21 died, instead of deriving any advantage from the experience, I only became perplexed and distrustful of every proposed means of cure. In this frame of mind I was transferred to St. Michael's hospital, where the admissions were very numerous, and the mortality alarming. I here found the calomel practice pursued with much more boldness and resolution than any results I had witnessed could have induced me to adopt*. As it was little complicated with the use of opium or any other medicine which could materially interfere with its operation, and I had myself lost all faith in treatment, I determined to become a mere observer for one month, that I might have some numerical evidence, at least, in the cases submitted to the action of one powerful remedy, of the good or evil it was capable of effecting. To arrive at more accurate conclusions, I noted in each case, on admission, the presence or absence of the pulse at the wrist, and

took care that the registrar or apothecary did the same whenever I was absent. I also, in conjunction with the other physicians, endeavoured to prevent, as much as possible, the admission of patients in the mere premonitory stage, which, however, was not always possible. At the termination of the month, I found the gross amount of deaths was 47 out of 165 cases, or less than one-third, which was not far away from the general mortality in most countries. But on casting up the tables, and ascertaining the amount of mortality in the separate stages, I was perfectly astonished to find that while in the primary stage, or that of rice-water vomiting and purging, there were only 5 deaths in 119 cases, there were in those admitted in collapse 42 deaths in 46 cases. I had here at once evidence sufficient to lead me to the most certain and confident conclusions. The calomel practice was clearly capable of effecting a greater number of cures than any known remedy in any other dangerous disease, so long as the patient was placed under treatment before the pulse ceased at the wrist; but after that occurred, it did nothing, or it did mischief, the recoveries being perhaps less than might be effected if the cases were wholly abandoned to nature until the period of reaction. These inferences were afterwards amply confirmed by the reports from other hospitals in which similar notes were made of the stage in which patients were admitted, and the comparative mortality in each; the same treatment, with little deviation, having been at the time pursued in all. The correspondence of these reports is remarkable, and may be looked upon as in itself a proof of the general correctness with which they were kept. I am at the same time ready to admit that they only make an approach in the whole to the accuracy which is desirable for medical induction. The deaths in cases treated before the pulse had ceased at the wrist are reported as 5 in 119 at the most successful period, and at 16 in the 100 at the least successful. I believe, in reports very accurately kept, and from which mere premonitory cases were strictly excluded, the mortality would seldom, under any treatment, be less than the latter amount, and would probably, at the onset of the disease, when cases are

* After the exhibition of an ipecacuanha or mustard emetic, it was given in scruple or half drachm doses every hour until the patient was salivated or died. Cordial mixture, saline draughts, and turpentine and astringent injections, were also employed.

generally of the rapid type, exceed 20. The recoveries reported in cases of collapse varied from 1 in 11 to 3 in 10. I do not believe in any hospital in Limerick they ever exceeded the former amount, and I am disposed to attribute the greater number which took place in the second instance partly to our having tampered less with the patients while insensible to medicine, and partly perhaps to the decreased severity of the disease in the latter months of its existence amongst us. Dr. Mackintosh states the estimate of recoveries in Edinburgh to be only 1 out of 12 in cases in which the pulse was either so weak as scarcely to be felt, or was imperceptible at the wrist; but believes this too high, and that not more than 1 in 20 recovered out of that state. In Paris, at least during the first incursion of the disease, I believe the recoveries from collapse were equally rare. Such reports from cities in which the patients had the advantage of the highest medical talent in the world, joined to the well-ascertained inefficiency of all remedies in those cadaverized cases, leads necessarily to the conclusion, that the greater or lesser success in the results indicated simply a greater or lesser severity of the disease.

To return to the treatment. I had now satisfactorily ascertained that calomel was a most successful medicine if administered while the pulse was perceptible at the wrist, but, on the contrary, detrimental when given in collapse; not that it can then do any immediate mischief, the stomach being apparently insensible to its action, but because it is accumulated to an amount that becomes destructive or injurious as soon as reaction takes place. Another object of investigation arose naturally out of these facts. On first visiting the convalescent wards, after I became attached to St. Michael's Hospital, I was excessively shocked at the appearance of the patients, all of whom were in a state of profuse salivation. Their heads seemed, for the most part, of an enormous size, their faces and jaws puffed out, and shining as if they would burst, their edges sunk in, and their lips disgustingly swollen and ulcerated at the angles; some were sitting up in the bed endeavouring to wash the sloughs from their mouths with the sharp gargles, and complaining

bitterly, others were lying moaning, with their faces on the pillows, which were drenched with saliva, while some hung over the bed-side, dribbling the same glairy secretion, mixed with blood and matter, into tin vessels. The slow recoveries of these—the tortures they went through—the shattered constitutions they often went away with, seemed such a drawback from the benefit which could attend any treatment, that I felt, even after I had convincing evidence of the curative powers of calomel, remedies of less efficacy might be preferable, if not succeeded in the cases of recovery by such protracted suffering and ill health. It became, therefore, at once an exceedingly interesting question, whether the cure of cholera by calomel was to be attributed to the usual influence of mercurials on the system, or to some unknown property which it possessed when given in large doses. The universally received opinion, among the advocates for the calomel treatment, at the time, both in England and in this country, was, that if once its mercurial influence could be established—if once the gums became sore, the patient was beyond danger; and whether they were right or wrong in the presumed connexion between the recovery and the salivation, the facts could hardly be disputed. As a consequence of this opinion no limit was placed to the amount of mercury exhibited until symptoms of salivation set in, and of mercurial liniment rubbed into the skin through every stage of the disease. To the results already described, a protracted convalescence, sloughing ulcerations of the mouth, and torture unimaginable by those who have not felt it, almost invariably succeeded. In reflecting on the rapid manner in which the influence of calomel is evinced in cholera, on the suddenness with which it sometimes arrests both the vomiting and purging days before its mercurial action on the system is perceptible, I strongly suspected the correctness of the prevailing doctrine. Even if it was a fact that persons who were salivated invariably recovered, it could be said to prove nothing, since it was clear, when calomel was administered so profusely to all, no one who recovered from cholera could escape salivation. It

might indeed be said, with great probability, *that they were salivated because they recovered, rather than that they recovered because they were salivated.* To satisfy myself on this point, I immediately sent a set of queries to medical friends in Liverpool, Dublin, and other places, and I found that in several instances, not only had patients in salivation for other complaints fallen into cholera, but some who had been salivated for cholera had, during the salivation, sunk back into collapse and died. I was indeed furnished, soon after, by a medical friend in Limerick, with the details of three cases which had occurred in his own practice, in each of which profuse salivation had occurred, with apparent relief of the choleric symptoms, and subsequently a relapse, followed by the usual cessation of pulse, and sinking, and death. It was obvious, then, that the salivating influence of calomel was not that which tended to effect the cure, but some other of which we are less observant, and consequently that the remedy should be discontinued as soon as there was an arrest of the symptoms. It also followed that the external use of mercury by ointments and liniments should be altogether abandoned, since they could not be supposed calculated to produce that sedative or counter-stimulant effect sometimes almost instantaneously observed to follow the exhibition of large doses of calomel. A third and not unimportant inference finally suggested itself—that if the benefit derived from calomel was to be attributed to its sedative influence alone, as it had been by Annesley and other Indian practitioners, it should always be given in large doses only, and those rapidly repeated. It should be given, in fact, altogether (on Rasori's counter-stimulant plan) to produce an effect on the system and the disease, *entirely independent of any sensible evacuation.*

Having fully convinced myself of the correctness of these inductions at the termination of the first incursion of cholera, I determined, should it return, to test them on as extensive a scale as possible; and this I had soon a fair opportunity of doing. The disease made its second visit in the latter end of August, and being appointed to the care of St. John's Hospital with three

other medical gentlemen, we agreed upon a plan of treatment which altogether excluded the use of calomel during collapse, and the administration of large doses after re-action had taken place. No limit was placed on the amount to be given in the early stage of the disease. There was more than usual care observed in preventing the admission of persons with mere premonitory symptoms, and in noting with accuracy the cases in which the pulse had ceased previous to their arrival at the hospital. The result is already given among the tables of the mortality of cholera. There were admitted within six weeks, 120 cases; 59 in the primary stage, and 61 in collapse. Out of the 59 several ran into collapse in despite of treatment, of whom 8 only died. Out of the 61 in collapse on admission, 43 died and 18 recovered,—a greater number than had previously recovered from that state under the most sedulous medical treatment.

But the most gratifying circumstance of all was, we had few sore mouths, and no very bad ones, such as did occur being slight, and attributable to our occasional perseverance in the calomel from mere apprehension, after the symptoms of the disease were arrested. I had, indeed, numerous instances among the recoveries to convince me that the resistance to the salivating influence of calomel on the system was coexistent with the presence of active cholera, and that the resistance gave way with the disease. This was so strictly true, that one dose of calomel, even a small one, given after the symptoms appeared to be arrested, was found to occasion sore mouth more certainly than an ounce administered while they were violent, or on the increase. The probability of salivation depended not on the quantity of the medicine administered, but upon the non-existence of active cholera at the moment of its administration. As long as cholera existed, none of the usual effects of calomel appeared; it acted neither on the salivary glands, nor liver, nor bowels; I suppose on the same principle that tartar-emetic does not excite vomiting, nor opium narcotize, during the presence of inflammation. I have in several cases given it hourly, in scruple doses, to the amount of two or three drachms, or upwards, without eventual salivation; and I recollect one instance, in particular, in which I gave nearly

two drachms within an hour and a half, with perfect success, and without affecting the system. A tall stout young man was brought into the ward one day, who had been for some hours in the disease. He had most violent vomiting and purging of colourless fluid, with suppression of urine; his look was somewhat sunk and alarmed, and cramps were commencing. He was got to bed, and took an emetic of ipecacuanha; of the advantage of which on all occasions I have much doubt. Sinapisms were applied to the calves of his legs, to allay the cramps, and dry frictions were made use of for the same purpose. As soon as the emetic was thrown off, a scruple dose of calomel was given; after which no drink was allowed for a little while, although the use of cold water, saline draughts, or occasionally of mild cordial mixture, usually formed part of our plan of treatment. In half an hour afterwards, I found all the symptoms continuing unabated; the skin, which was warm on his admission, becoming cool, the pulse more compressible, and the voice weaker. Half a drachm of calomel was instantly given, avoiding the administration of drink after it for a time, as before. On passing the bed when another half hour had elapsed, appearances were still more unfavourable; the pulse manifestly failing, and the countenance indicating approaching collapse. I now gave a drachm of calomel at a dose, and anxiously watched the effect. At the end of fifteen minutes I was glad to find there was no advance towards collapse, and within half an hour there was a perceptible improvement in the pulse and increase in the temperature of the skin. There was from that time forward no troublesome recurrence of the vomiting, or purging; no cramps, no coldness, no sinking. The thirst gradually abated, the natural secretions returned, and, without another grain of calomel, the patient recovered. He was discharged in three or four days, without the slightest soreness of mouth, and I met him in the street, in a week afterwards, returning from his work, stout and well.

It is of course not to be imagined that in using any remedy, however influential, we could feel ourselves warranted in rejecting accessory treatment. Nothing, however, on the whole could be less complicated than the curative

means adopted in conjunction with calomel, consisting generally of dry frictions, frictions with turpentine, or sinapisms to relieve the cramps, or the violent action of the stomach or bowels — of cold or saline drinks to allay the thirst, or of anodyne or astringent injections to suppress the discharges by the rectum. This last became a most important object with us, when we grew familiar with the disease, as we often found that where, from the excessive irritability of the stomach, or the smallness and inefficiency of our doses, no early effect was produced on the choleric symptoms, the patient was usually carried into collapse by the exhausting serous diarrhoea. I have, indeed, not infrequently seen a patient, apparently maintaining his ground for some hours, sink suddenly into collapse after one profuse evacuation, and have been so deeply impressed, by such instances, with the necessity of invariably suppressing the diarrhoea, that I latterly made it a rule to give an astringent injection after each evacuation, however numerous, until my object was accomplished. The injection generally consisted of half a drachm of the acetate of lead mixed with thin starch, to which a tea-spoonful of Linctum was added, either when the injections were repeatedly returned immediately after administration, or the cramps were distressing and resisted other remedies. I always, however, considered the administration of calomel more essential to the permanent suppression of the diarrhoea than any injections. The effect of the latter, even when successful, was often of short duration, and not to be depended on; the effect of the former, when perceptible on the discharges, was in itself a sign that the disease was giving way, and was more steady and disposed to continue. The astringent injections seemed calculated to divert and gain time; the calomel to contest the ground with, and conquer the malady.

Opium was seldom given with the calomel, except perhaps in the earliest periods of the attack, its beneficial effects appearing always proportioned to the earliness of its use; but of this it will be necessary to speak hereafter. The cordial mixture (aromatic spirit, with sweet spirits of nitre and camphor) was latterly given very sparingly, or not at all, in the primary stage; and even in collapse it was employed rather with a

view of giving moderate support, than of hastening the period of re-action. The application of external heat in any shape was latterly almost wholly abandoned, and I believe without any disadvantage.

I have now given an account of the calomel treatment and its results; and if ever any remedy got a fair and full trial—if ever all the good or evil it was capable of effecting was displayed—if ever materials were afforded for absolute induction in medical science, it was in the application of this medicine to the cure of cholera at Limerick. It was tested in 1448 cases treated in different hospitals, at different periods, and with a very close correspondence in the results in all. The quantity of calomel used was enormous, having seldom or never been given in any hospital in less than scruple doses, and these generally repeated every hour or half-hour; half a drachm, and even two scruples, was indeed not an infrequent dose.

I cannot conceive that any one who considers the facts and reasonings here brought forward, can again assert we have attained no valuable knowledge, no successful treatment, from our experience of cholera. It is now at least evident that, by the judicious application of one remedy, we can control the disease, or arrest its progress, in 84 cases out of 100, if the patient be placed under our care before the pulse has ceased at the wrist; and if, after that, no more than two or three can be saved out of ten, it is only to be considered that the stage of collapse in cholera is like the stage of muttering delirium and floccitation in fever, the almost fatal conclusion of the disease. It is our object to prevent the occurrence of these states in either; and surprising though it be, it is much more easy of attainment in the former than the latter. I am far from thinking the tables I have given as evidence of facts were kept as accurately as they ought to be, but I believe that they closely approach the truth, and that they will be borne out by subsequent experience, except, perhaps, their accuracy be tested in incursions of the epidemic of a more rapid and virulent type than afflicted this country. They happily furnish us with evidence of the capabilities of a remedy more popular on the whole than any which has been tried, and enable us to form some estimate of the value of others

by a comparison which has been hitherto impracticable. I regret to say I do not consider the use of calomel free from objection, employed even in the qualified manner I have suggested; but no other medicine has yet been proved to us capable of effecting so large a proportion of cures. I believe, however, there are some which approach it closely in power, but from the want of all accurate classification in the statements respecting them, I can only offer well-considered opinions as to their probable value.

Opium.

No medicine employed in the cure of cholera has been so much praised or abused as opium. It came to us, like calomel, highly recommended from India, where it was extensively used both among the English and native practitioners, but was not long employed in this country when it fell into very general disrepute. Some asserted that it narcotised the patient without relieving the disease; others, that it obstructed the respiratory functions, and hastened the fatal event; a third party, that it insured the supervention of destructive congestion in the consecutive fever; while a few only maintained that its merits had been underrated, and that no remedy was calculated so safely, so readily, and so certainly, to arrest the course of the malady. It need scarcely be mentioned that this extraordinary discrepancy of opinion arose, as we have already pointed out, from the application of the remedy to all stages of the complaint: some, when they found it effecting cures in one stage, extending their faith in its influence to all; and others, when it was manifestly unsuccessful in the state of collapse, in which so many were brought under treatment, assuming it to be at all periods worthless or mischievous. This was very much the case at the hospitals in Paris, where at first opium was employed very extensively. The great majority of persons admitted were unfortunately in deep collapse, to which the warmest advocates for its use consider it to be inapplicable; and the natural result was a total loss of faith, and abandonment of the remedy. At La Pitié, where it was given both by the mouth and by injection, narcotism quickly appeared in the majority of cases, without arresting the unfavoura-

ble symptoms, and so it was laid aside. The physicians of the Hotel-Dieu also, for the most part, discontinued its employment, diminishing the doses considerably even when they did use it, or limiting its exhibition to injections or liniments. M. Bally and M. Recamier asserted that it was absolutely injurious in the blue period, and useless in others. Those practitioners, however, who commenced with its use in the early stage of cholera, do not seem to have been so disappointed with its effects; as I believe it was continued, more or less, throughout the epidemic, by Dupuytren, Chomel, Gendrin, Rayer, Lermurier, and M. Mailly.

I have not had much experience myself of opium in confirmed cholera, but I have now before me the opinions and statements of the principal British practitioners, and, setting aside such as appear to be founded on inaccurate or very partial observations, the conclusion appears to be, 1st, that, like calomel, it is wholly inapplicable to the stage of collapse, hastening the death even where it allays the sufferings of the patient; 2dly, that it is inapplicable to several cases previous to collapse, occasioning narcotism without abating the symptoms or arresting the course of the disease, such cases being usually those of persons with florid complexions and full habits; 3dly, that in the vast majority of cases with pulse distinct at the wrist, it is eminently successful.

It would be exceedingly gratifying to me if I could draw an absolute comparison between the effects of opium and calomel, as, if the former could be proved at all as successful as the latter, I should unhesitatingly prefer it. If, for instance, we could determine the exact average of persons in the hundred who were liable to narcotism, or other evil consequences of the remedy, and that it did not exceed 16 in the 100, who is there that would not consider it a "*heroic*" medicine in so dreadful a disease? If we can venture on a confident inference from the experience of two very observant practitioners, we have their evidence that the fatal cases under the opiate treatment do not amount even to so many. Dr. Brown, of Musselburgh, says, when once a patient is in confirmed cholera, he thinks of no medicine but opium, and no cordial but brandy. He allows no drink after the opiate, merely wetting

the tongue with water*. Then, if the vomiting has not returned, he gives brandy and water. Should the case nevertheless be running on into collapse, the vomiting and purging continuing, he resorts to opiates and stimulant enemata, assisted by pressure. By this plan he asserts that he saves 9 out of 10, or 90 in the 100. Mr. Tweedie speaks, if possible, still more confidently; declaring that, previous to collapse taking place, the disease may be stopped quickly, safely, and with human certainty, by full and energetic doses of opium, proportioned to the age, idiosyncrasy, and condition of the individual. Whether these gentlemen consider any case *in collapse* in which the pulse is distinct at the wrist, I know not; yet much of the value of their statements depends upon the strictness of their definition of the word.

Opium seems to operate in the cure of cholera as calomel does, by some contra-stimulant power, and not by inducing any narcotic or other sensible effect. This power is manifested in certain medicines only, and always in the exhibition of large or extraordinary doses; for in small or ordinary doses their common characteristic effect is more likely to appear. Thus in ordinary doses, in inflammatory disease, calomel will purge or sdivate, opium will occasion sleep, or emetic tartar vomiting, if it be possible to produce these effects by such medicines in the morbid condition of the patient. But, given in large doses for the same disease, neither purging, nor salivation, nor sleep, nor sickness of stomach, follow the exhibition of any of these remedies: all of them in such instances, however different their ordinary influence, operating on one common principle, and producing one common result—an arrest of the malady without sensible evacuation of any kind. It is, then, of some importance to recollect, when giving any medicine with a view to its contra-stimulant influence, that if any of its ordinary sensible effects appear, there exists some idiosyncrasy of habit incompatible with the safe administration of an ade-

* This should be a general rule with regard to every medicine given in cholera, except such as are intended to excite vomiting. Even where drink is allowed *ad libitum*, as it was lately in Limerick, the patient should not be allowed to take any for a quarter of an hour or twenty minutes, at least, after taking medicine.

quate dose, or the disease is on the decline, and does not require so large a one, or was never present at all, and therefore the system is free to the ordinary operation of the medicine.

I believe the failure of opium in the hands of many practitioners has arisen from their not having exhibited it with sufficient boldness or judgment. With some it was administered timidly and faithlessly; with others it was given in cases where it ought not to have been employed at all. It is very possible that those cases of cholera in which, even in its primary stage, a tendency to narcotism after opium is observable, are precisely such as would be benefited by abstraction of blood, and thus probably made tolerant of a remedy otherwise inadmissible.

There is one great and very mischievous error into which the vast body of the medical profession has fallen, in forming their estimate of the value or the danger of remedies proposed for the cure of this disease, and that is, attributing the fatal congestion in the consecutive fever to some medicine injudiciously made use of in the early stage. So long as this congestion was referred to the fatal practice, as it was called, of administering opium and stimulants, and was advanced with sufficient dogmatism, I was myself led away like others; but when I witnessed the same congestion following the cold water treatment, and yet more, when I heard its frequent occurrence attributed to it, I began naturally to consider whether it was not rather a necessary, or almost necessary, result of the mischief or injury sustained by the system during the collapse; and to this conclusion I eventually arrived. I make the observation here, because not only opium, but other valuable remedies, lost reputation, owing to the dreadful fatality of the consecutive fever, with which they had no absolute or direct relation. In the report of the Hôpital St. Antoine, at Paris, M. Kepler is stated to have abandoned opium, because, though effecting speedy cures, it was sometimes followed by violent attacks of cerebral congestion, which rapidly destroyed convalescents. Similar inferences were drawn by practitioners in this country, on as questionable grounds; and it was only after the cessation of the disease that the truth began to dawn upon the profession.

In concluding the subject of opium,

I must acknowledge myself favourable to a further and more full trial of it. Should the opportunity occur, its use should be as little complicated as possible with other remedies, should be limited to the primary stage, and in that stage to those cases in which narcotism is not readily induced. In all new trials, too, of either this or other remedies, it would be interesting to note accurately the amount of cases in which fatal congestion took place in the consecutive fever, so that, after some experience, a comparison (impossible at present) might be instituted between their results in this way. It is just possible such congestions may take place in greater proportion after one mode of treatment than another, but not at all deducible from any facts at present before us.

Stimulants.

The employment of stimulants, especially brandy, in the cure of cholera, appears to have been as general in the East as the use of calomel or opium. They probably operate exactly in the same way, and are influential or useless at the same periods of the disease. In these countries, however, when brandy came to be tried, it experienced the fate of all other remedies for cholera, and lost its reputation with the mass of the public, which is not apt to be very philosophic in its inductions. The testimonies in its favour which have survived its loss of character are, however, far too decided and too numerous to permit us to assent to the indiscriminate censure which has been directed against it. We cannot understand the very strong belief evinced by those who have used this and other stimulants extensively, that it is generally successful in the primary stage of cholera, and especially in those cases in which the employment of opium is admissible or useful, without supposing it to be founded on some very imposing evidence. I do not indeed know of any reports which would enable me to compare its results with those of calomel or opium, but I hold it to be very inferior as a contrastulant to either. There are individual cases to which, perhaps, it might better apply, but these we shall never have it in our power to distinguish until we are furnished with more accurate estimates of its general effects.

I have been here speaking of stimu-

lants given in large doses, with a view to the arrest of the disease. There is, however, a much more general agreement as to the benefit derived from them when given more moderately, and merely with the object of sustaining the circulation. Most practitioners have employed them in the stage of collapse, and if any thing can really be said to assist the occurrence of re-action, it is probable that very mild cordials may, given patiently, and more as a slight stimulus to sustain the sinking powers of life, than to excite them. Even when they were abandoned as a remedy in several of the hospitals in Paris, their use was continued in this way, and apparently with advantage. The same change of opinion with respect to stimulants was observable among the majority of practitioners in this country, but some still continued to employ them confidently as a means of exciting or ensuring re-action; and if we could give implicit faith to the correctness with which their reports were kept, not without good reason. Mr. Stable, of Aldersgate-street, stated that out of 107 cases with the characteristic dejections and collapse, only 23 recovered in 1832, or about 2 out of 10, when treated in various ways; while in 1833, 17 recovered out of 41 treated by stimulants, or upwards of 4 in 10. Mr. Anderson published three cases (two in bad collapse) which were cured by continued doses of brandy with a little laudanum, until warmth was restored—and many others might be quoted. But referring to Mr. Stable's statement, to which, from the number of cases compared, some value must attach, we can only say, the weight of medical opinion is immensely against it. It is, indeed, a very prevalent belief, that excessive stimulants given in collapse have, in general, tended to oppress what little power remained, which, I am convinced, to say the least, has been occasionally the case with that very popular stimulant, the mustard emetic.

There are other popular stimulants, and amongst them, camphor and turpentine, of which I have not spoken. The latter, recommended by Mr. Sawyer, would appear to deserve a fairer trial than it has yet had in this complaint,—I mean in the primary stage. He gave it, he asserts, with considerable success, in doses of from two to four drachms, combined with a little

laudanum, and found it to remain on the stomach when other fluids were rejected.

Camphor was used extensively in India, and in very large doses. I do not think it has been tried uncombined with other equally powerful medicines, in England or Ireland, except in those places in the latter country where it was administered as a popular specific in the form of a spirituous solution, called the Pousonby Drops*. These drops acquired a high reputation among the non-medical portion of the public in Limerick and the surrounding counties, and I have no doubt they effected numerous cures when given early in the complaint, while the symptoms were yet mild; but I believe their fame was chiefly due to the wonderful recoveries they seemed to effect from the state of deep collapse. They were given in the dose of two drops every five minutes, and no drink whatever was allowed during their exhibition, or until the patient's recovery, except cold or iced water. Under this treatment reaction came on in two, three, or four cases out of every ten; in the usual proportion, in fact, in which it comes on when nature is not interfered with in her efforts; and all the credit of their having lived to pass through consecutive fever, and die of it, was attributed to the camphor. The impression created in the public mind by the absolute cures these drops effected in the early stage, and the extraordinary power they seemed to possess of reviving persons out of the deepest collapse, was so favourable, that on a slight return of the disease to Limerick, I believe in September 1832, eight cases were placed (with the sanction of the Board of Health) under the care of a medical man who was a strong advocate for their employment, to test their efficacy. They were all in collapse, or nearly so, when the treatment commenced; and the success, as may be imagined, was not very flattering; six out of the eight died, after having struggled into consecutive fever, when the Board of Health put a stop to the trial. Other remedies were subsequently made use of for the remaining two, but without avail. As well as I can recollect, not one out of the eight finally recovered.

It was the opinion of some physicians

* One ounce of camphor to six ounces of spirits of wine.

here, that camphor certainly evinced considerable power in exciting reaction, but that fatal congestion always followed. As this congestion, however, so frequently follows the cold water practice, as well as the stimulant or sedative, we cannot look upon such opinions as at all conclusive.

REMARKS ON LEPROA VULGARIS.

ILLUSTRATED BY CASES*.

By JOHN BISHOP, Esq.

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THE great diversity of opinion existing amongst authors respecting the *Lepros vulgaris* of Willan, or the *Dartre furfuracée arrondie* of Alibert, has induced me to offer a few observations on the subject, with cases by way of illustration. The causes which tend to the development of this disease—the immediate seat of the organic change—the nature of that change—and the best mode of treatment, are all more or less involved in doubt and perplexity. The following brief remarks are rather intended to shew in what respect my experience concurs with the theories of this disease already before the profession, than with any intention of proposing new remedies; yet as concurrent testimony has naturally much influence in recommending particular modes of treatment, these details may not be without their use in that respect also.

Lepros vulgaris is by no means an uncommon disease in this country, particularly in the low marshy districts of the West of England, where it appears to be endemic.

With regard to the causes of leprosy, our knowledge appears to be chiefly negative and conjectural. It is stated by Rayer† (and, I believe, by all authors), that this disease is not contagious; “that patients are not kept isolated in our hospitals, and that husband and wife continue to cohabit without one infecting the other.” Bateman‡ likewise observes, that “it is certainly not

communicable by contagion.” He also remarks, “I have met with one gentleman in whom spices or alcohol speedily produce it.” Larrey* attributes the attacks of leprosy which the French army suffered in Egypt, to the unwholesome quality of the pork in that country.

Bakers, and those who are much exposed to the irritating influence of dry powdered substances, are supposed to be more liable to this disease. Dr. Willan considers leprosy to be principally produced by exposure to cold and moisture, an opinion with which I am strongly inclined to agree, and which is supported by the facts contained in the first case subjoined. The physiology as well as pathology of the skin is not less interesting than important. Since the investigations of Malpighi, modern anatomists have bestowed much pains in endeavouring to unravel the minute and complex structure of the skin, which we may hope will ultimately lead to a better knowledge than we at present possess of the seat, as well as the cause, of its various lesions. If it is capable of being demonstrated that there is in the composition of the skin, as stated by Breschet and Roussel de Vauzeme†, “a mucific apparatus, composed of a secreting and an excreting gland, furnishing a soft material which becomes epidermis by desiccation,” we shall have less difficulty in accounting for some of the phenomena observed in leprosy, and in suggesting a probable cause for the profuse secretion of cuticle which attends this malady, as well as for its altered character.

According, then, to the views of Breschet and Roussel de Vauzeme, the excretory duct, which they describe as furnishing the epidermis, must be lined by a *mucous membrane*, which is the case with all open tubes in similar structures throughout the animal kingdom. Now the influence of cold and moisture in promoting copious secretions from mucous membranes cannot be denied. Supposing, therefore, these tubes and the connected apparatus to be in a state of chronic inflammation, the copious secretion and altered character of the cuticle in leprosy may be satisfactorily explained.

* Read before the London Medical Society, Feb. 19, 1838.

† Treatise on the Diseases of the Skin, by P. Rayer. Translated by R. Willis, M.D. P. 620.

‡ Vide Synopsis, by A. T. Thomson. P. 41.

* Relation Chirurgicale de l'Armée d'Orient.

† Nouvelles Recherches sur la Structure de la Peau, par M. G. Breschet, M.D., et M. Roussel de Vauzeme, M.D. Paris, 1835.

The shades of character which serve to distinguish leproa vulgaris from psoriasis are almost imperceptible; but the experienced eye will immediately distinguish the squame of leproa from those of secondary syphilis. It is well known that the whole of the tissues which enter into the formation of the skin are not alike affected in leproa. Its first stage of development, according to Rayer, Bateman, and others, is the appearance of small solid elevations, about a line in diameter, of a circular form, and firm consistence; a character and result in accordance with the hypothesis here proposed, and which, presuming the accuracy of the anatomical details on which it is based, is supported both by analogy and observation.

The remedial measures recommended for the cure of leproa are as various as they are indefinite in their results. The most important of the external remedies are, 1st, baths, simple and medicated. A common vapour-bath, according to Rayer, sometimes proves sufficient of itself for the cure of leproa, whilst he observes that the sulphur vapour-bath generally fails in procuring relief, though we find that it is recommended by Galis, Clarks, and Willan. 2dly. Mineral waters, applied both externally and internally, are supposed by many to be highly beneficial. The most celebrated of these are the waters of Barèges, Caunterets, Bagnères, Bagnoles, and Enghien, in France; and those of Bath, Harrogate, Leamington, and Crofton, in England. 3dly. Ointments composed either of the phosphate, acetate, nitrate, or the proto-chloride of mercury; the latter may be used in the proportions of one drachm to an ounce of lard. I have, however, preferred the use of the ung. hydrarg. nitrat., with the most beneficial results. 4thly. Counter-irritants, and stimulating liniments, have been found useful.

Now although endermic remedies may be of essential service as auxiliaries, still they cannot be relied upon alone, when the disease is of long standing, or of an aggravated character, inasmuch as they exercise but a very indirect and uncertain control over the morbid action of the internal organs, or the vitiated states of the blood, which accompany this disease; nor do they secure the patient against a relapse. In illustration of which, a case is re-

corded by Alibert*, to this effect:—“Tout le système dermoïde étoit affecté, et se résolvait en squames furfuracées ou en molécules farineuses. Nous traitâmes cet infortuné par le régime le plus doux; des bains émollients qui lui furent assiduellement prodigués, assouplirent l'épiderme, lui donnèrent une apparence plus saine, finirent même par détruire jusqu'aux moindres traces de cette affection herpétique; mais, cet homme étant sorti de l'hôpital, négligea tous les soins qui rendent une guérison solide et permanente.” It seems that this man, after losing every external trace of the disease by the use of the baths, subsequently relapsed, and fell a victim to the disease.

The catalogue of internal remedies, is, perhaps, still more extensive, and sometimes not a little curious. Riemer and Carmenati have recommended viper and lizard broths. Carrère, Crichton, Razon, and Bertrand Lagrèsie, have extolled the efficacy of dulcamara. Bateman† recommends dulcamara to be used in combination with 1-12th of a grain of the oxymuriate of mercury. Pitch, “in doses of from six to twelve grains, and turpentine, in doses of from ten to thirty grains,” administered in the form of pills, are productive of a similar good effect, where the cutaneous circulation is very inert. Gurdlestone‡, Willan, Batty§, Bateman, Duffin, and many others, have more especially recommended the arsenical preparations. It is to the beneficial effects of the latter, which I have used very extensively, that I desire to draw attention in the following details.

CASE I.—Mrs. Galpin, a lady, aged 60, residing in the country, requested my attendance on her case in the month of March 1831. On visiting her, she stated that she had enjoyed excellent health for many years, until the commencement of the year 1827. Having at that period occasion to walk to a neighbouring village, the day being warm, and her pace rather brisk, she found herself in a copious perspiration; whilst in this condition, a heavy shower of rain suddenly fell, which penetrated her clothes and wet her to the skin. A severe cold was the consequence, which was succeeded by an itching sensation,

* Description des Maladies de la Peau, par J. L. Alibert, p. 55.

† Synopsis, p. 51.

‡ 15th vol. Med.-Physical Journal.]

§ Ibid. 17th vol.

extending over nearly the whole surface of the body, and she almost simultaneously observed on her extremities detached bright patches, which, after a short interval, terminated in desquamation of the cuticle, and thence extending over the whole body. The secretion of the epidermis now became profuse, one layer succeeding another, until, after the lapse of a few months, every part of the body was invested; the growth of the hair was destroyed, the eyelids were incrustated, and the nostrils blocked up; so that it was requisite to detach the scales from the eyelids before she could open them, and to clear the nostrils with a netting-needle, in order to permit the passage of air. The matrices of the nails, both of the fingers and toes, became affected, and the destruction of the nails followed; her strength began to decline; the extremities, burdened with multiplied layers of epidermis, no longer obeyed the impulse of the will; and the mind was ultimately enfeebled by the combination of protracted suffering with disordered functions. She thus became permanently confined to her bed, and incapable of moving without assistance, presenting an appearance not only disgusting, but it may be said, horrible.

She had now laboured under this disease about three years, during which she had been variously treated by several intelligent practitioners, without the slightest relief. The itching she experienced was almost intolerable. Plumbe* attributes this sensation "to the raising up of the edge of the scale detaching the centre forcibly from the cutis." In the state just related I first saw her, when the symptoms, in addition to those already mentioned, were these:—

The epidermis, which was of an opaque dirty-white colour, was so profuse, that the friction of a small soft brush was sufficient to detach a large basin full; the itching continued exceedingly painful; there was also great prostration of strength; the tongue was of a morbid redness, with prominent papillæ; the tunica conjunctiva of both eyes was slightly inflamed; the bowels were usually constipated; the urine was scanty and high coloured, and deposited a sediment on cooling; the appetite began to fail, and there was occasionally nausea; the pulse became

low and feeble, and her mind was impressed with the conviction that she could not recover. Having, in the first place, allayed this apprehension by expressing an assurance that she would recover, I prescribed as follows:—

R Liq. Potassæ Arsenitis; Liq. Potassæ, aa. ʒi.; Decoct. Sarsap. Comp. Oj. M. capiat cyathum ter quotidie.—Pil. Hydrarg. Submur. co. gr. v., omni nocte sumend.

R Magnes. Sulphat. ʒij. c. cyatho Aquæ Menthæ viridis quoties alvus minus laxa fuerit.

These remedies were continued without intermission for about six weeks. At the expiration of that period the first symptom of relief observed was a great diminution of the itching, amounting almost to a total absence; the desquamation appeared more transparent. The dose of the liquor potassæ arsenitis in the same form was now increased to eight drops, and repeated as before, three times a day. The parts of the skin which appeared more irritable were touched at night with a little of the ung. hyd. nitrat. apparently with good effect; a further period of six weeks was devoted to this treatment, at the expiration of which, the itching had entirely subsided, the production of scales had ceased in many parts, and the general health and strength were improved. The same remedies were, therefore, continued. After the lapse of six months, the head and face began to assume a natural appearance, and the body presented isolated patches rather than a continuous surface of scales, the interval being occupied with healthy skin. The patient being now desirous of discontinuing the sarsaparilla, the liq. potass. arsenitis was given in doses of ten drops, in a little tea, three times a day. At the expiration of a few weeks, however, the stomach became irritable from the action of arsenic in this form, and the scales had not entirely disappeared. She, therefore, resumed the remedy, in combination with sarsaparilla, which agreed perfectly well. At length, after having used the arsenic without intermission, for twelve months, the skin was restored to its healthy functions, the hair and nails began to grow, the strength returned, so that after recommending her to use the medicine occasionally, with a view to prevent a relapse, I left her in a state of convalescence.

The points of greatest importance in

* On Diseases of the Skin.

this case I consider to be, first, that the existing cause of the disease was easily traceable by the effects immediately produced in consequence of exposure to cold, moisture, and suddenly suppressed perspiration. Secondly, That the liq. potassæ arsenitis, exhibited in small, but long-continued doses, was found capable of curing this most aggravated case. Thirdly, That the copious formation of the epidermis under such circumstances favours the opinion, that it is produced by an apparatus, the vessels of which are lined by a mucous membrane.

The last proposition may be regarded as purely hypothetical; but, independently of the concordant theory of Willan, we find that Dr. Duffin* entertains the same general notion, though in a less specific form, owing, no doubt, to his want of that knowledge of the structure of the skin, which we now possess, as a basis for his observations. He, however, remarks, "I imagine that the primary evil lies in the secretions of the true skin, which becoming vitiated by their local irritation, induce chronic or subacute inflammation of the vessels, that either nourish or produce the cuticle, and that they produce a superabundant supply of morbid cuticle."

The next case was that of a female servant who had been in the employ of the lady whose case has just been detailed. Her history was briefly as follows:—

CASE II.—Ann —, aged thirty, of fair complexion, and robust constitution, stated that she had been in excellent health until the period of her engaging in the service of the above-mentioned lady, during her residence with whom she had at times made use of her comb, and on one occasion she accidentally pricked her head, upon which appeared almost immediately afterwards a complaint of similar character to that of her mistress, commencing at the point where the comb had penetrated, extending itself over the whole head, and accompanied with a sensation of itching over the whole body. This patient was subjected to the plan of treatment just described, with the exception that she used the liq. pot. ars. dropped into a little tea, instead of its being combined with sarsaparilla; the ung. hyd. nitratis was the only local application employed. Without reciting unnecessary details,

it will perhaps be sufficient to say, that after using these remedies for six weeks, the itching began to subside, and when another similar period had elapsed, the dose of arsenic having been increased from six to ten drops, three times a day, it had entirely subsided; the cuticle appeared more transparent; the disease no longer extended its boundaries; and, finally, after persisting in the use of arsenic for nearly nine months, she became convalescent.

Having now for several years pursued the same mode of treating this disease, during which a great many cases have been subjected to this plan of treatment in public and private practice, I can with confidence deduce the following remarks from these details:—The last case established the important fact, that although lepra vulgaris be not perhaps communicable by ordinary contact, it may nevertheless be transmitted from one person to another by inoculation. This notion is opposed to all theories respecting the propagation of the disease. It appears also that six weeks usually elapse before the first indications of relief are observed, and that it requires many months to effect a complete cure, by means of the arsenical preparations administered in small, but long-continued and gradually increased doses. Bateman recommends the liq. pot. arsenitis to be used "in doses of four or five drops, which may be slowly increased to eight, and persevered in for a month, or more," a period which appears insufficient to effect even the first stage of improvement; and it probably is through want of continuing the remedy for a sufficient time, that, in the hands of many practitioners, the arsenical preparations have proved ineffectual. The arsenical solutions, if largely diluted either with the decoct. of sarsaparilla or the decoct. of dulcamara, will rarely disorder the digestive organs: they appear also, independently of their specific effects on the disease under consideration, to give a stimulating impulse to the circulation, to effect a change on the morbid state of the capillaries, to infuse tone and vigour in the vital organs, and to raise patients labouring under extreme constitutional debility to health and strength. Such, at least, has been the effects of the arsenic under my observation.

Bernard-street, Russell-square,
Feb. 20, 1838.

* Edinburgh Med. and Surg. Journal, Jan. 1826.

OBSERVATIONS

ON THE

ANATOMY OF THE ORGANS OF
CIRCULATION IN THE SCOLO-
PENDRA.

To the Editor of the Medical Gazette.

SIR,

BEFORE proceeding to relate the observations that I have made on the circulating system in the Scolopendra, I will give a short statement of what is already known concerning the anatomy of that system in this animal.

In the excellent publication of Straus* on the anatomy of the articulated classes, the circulating system in the Myriapoda, as in the Insecta, is described as consisting of a heart without an auricle, and an artery which distributes the blood throughout the body; after which this fluid returns, and again enters the heart by several orifices. In the scolopendra he describes the heart, or dorsal vessels, as commencing in the last segment of the body, and continuing forwards, in the median line, until it arrives at the second segment; where it terminates by giving origin to a large artery, which, passing forwards into the head, becomes so small near the mouth that it cannot be followed further. At its commencement, this artery gives origin to a pair of branches smaller than the vessel itself, which are directed forwards into the basilar segment. Towards the middle of the head, the trunk gives origin to a second pair of branches, more slender than the former pair, but following the same direction. A third pair goes to the organs about the mouth. After having given this description, the above author states that he has been unable to follow any of these branches to their extremities, in order to ascertain in what manner they terminate.

The above description, as far as regards the dorsal vessel, or heart, and central artery continued from it into the head, is perfectly correct; but the two lateral vessels (fig. 1 and 2, *a*), situated at the commencement of this artery, form a more important part of the circulating system than this author is aware of. Instead of being smaller in size than the cephalic vessel (fig. 2, *b*), I have found them to be, in the specimens of scolopendra that I have examined, considerably larger; they take their

origin, in common with that vessel, immediately from the heart (fig. 2, *c*), and follow a course outwards and downwards on either side of the œsophagus (fig. 2, *d*), to reach its inferior surface, where they unite and form a single trunk (fig. 1 and 2, *e*). Thus the œsophagus in this situation is completely surrounded by a vascular collar, as it is more anteriorly by a nervous one. The lateral communicating vessels descend abruptly, forming short arches. The point of junction of the two vessels is situated almost exactly beneath the anterior termination of the heart, and a little in front of the second infra-œsophageal ganglion: it also nearly corresponds to the posterior border of the second segment of the skeleton. The two recurrent nerves, in their course backwards from the brain (fig. 1 and 2, *f*), pass with the œsophagus through its vascular collar.

The single trunk thus formed runs directly backwards to the posterior extremity of the animal. In this course it rests on the nervous columns and their ganglia (fig. 1 and 2, *g*), but is easily separated from them: it lies beneath the alimentary canal and its peritoneal investment. The vessel appears to terminate posteriorly, after having passed over the last ganglion, by dividing into minute branches; but it here becomes so small and transparent, that it is difficult to ascertain satisfactorily its mode of termination: I have been unable to trace any connexion between it and the posterior extremity of the dorsal vessel. During its course along the abdomen, this vessel gives off several small branches on each side.

The lateral communicating vessels while passing around the œsophagus give off each a small branch (fig. 1, 2, *h*) which proceeds forwards to the muscles, moving the powerful maxillæ of this animal. From the point of junction of the lateral vessels a single small branch (fig. 1, *i*) is continued forwards between the œsophagus and the sub-œsophageal ganglion towards the mouth.

Treviranus (Vermischte Schriften, bd. 2, p. 32) describes the nervous ganglia of the scolopendra as being connected to each other by three instead of by two cords, although in the representation he has given of the nervous system of this insect he has only figured them as being connected by two cords. From the fact of the connecting columns

* *Considérations Générales sur l'Anatomie Comp. des Anim. Articulés.*

in this class of animals being only two in number, it is probable that this author may have mistaken the longitudinal vessel above described for a part of the nervous system.

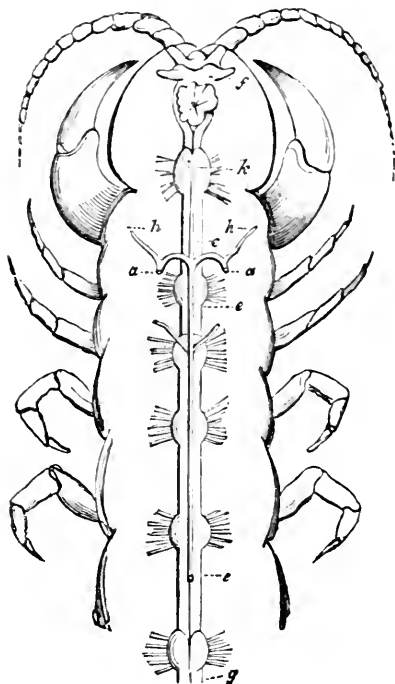
Mr. Newport has likewise fallen into the same error, inasmuch as he has described and represented this vessel as part of the nervous system (*Phil. Trans.*, part 2, 1834): he is correct, however, in figuring it as not connecting the ganglia to each other, but passing over both ganglia and columns, being but slightly connected to them.

Müller has described (*Meckel's Archiv. für Anat. und Phys.* 1828) in the scorpion a cord of a peculiar nature, lying on the abdominal ganglia and columns, giving off no branches, and thicker than both the abdominal cords put together. He states that it is con-

nected to the nervous cords and ganglia loosely, and can be easily separated from them, excepting at the last ganglion. It is distinguished from the nervous columns by its uniform non-ganglionic nature, and duller and more indistinct colour: he adds that it is not hollow, and is, in his opinion, certainly only a ligament (band): it diminishes in size posteriorly, and can be traced as far anteriorly as the vertebral piece of the thoracic segment, where it ends by an abrupt extremity, within the opening through which the nervous cord passes.

From many points in this description of Müller, and from the representation given of this cord by him, and from examination of the parts myself, I am inclined to consider this band as analogous to the vessel above described as occupying the same situation in the scolopendra.

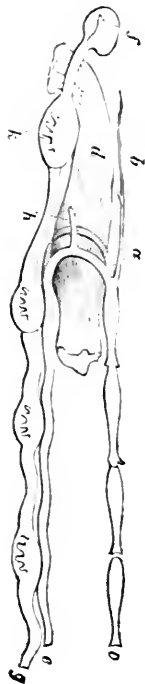
FIG. 1.



Description of the Wood cuts.

Fig. 1 is a view from above of the anterior part of a scolopendra, the heart and alimentary canal having been removed, and the lateral vessels divided: *a*, lateral communicating vessels; *c*, heart or dorsal vessel; *e*, trunk formed by the union of the lateral vessels; *f*, brain; *g*, nervous co-

FIG. 2.



lumns; *h*, branches of lateral vessels going to maxillæ; *k*, sub-oesophageal ganglion.

Fig. 2 gives a lateral view of the parts. The same letters refer to similar organs as in fig. 1, excepting *b*, cephalic vessel; *d*, oesophagus; *i*, the single branch of the point of junction.

pendra. Its close resemblance to that vessel is very obvious on comparing the plate of Müller with the wood-cuts here given, and with the plates of Mr. Newport. No connexion has been traced between it and the brain.

From the small size of the scorpions that I have examined, I have not been enabled to ascertain in a perfectly satisfactory manner the exact relation which exists between this cord and the heart.

Your obedient servant,

W. LORD.

University College, Feb. 24, 1838.

RUPTURE OF THE HEART AND AORTA.

To the Editor of the Medical Gazette.

SIR,

IN the last number of your journal are contained some observations on Rupture of the Heart, by Mr. Thurnam. As instances of this lesion are by no means of common occurrence, I beg to forward you the particulars of a case which has recently come under my notice. I likewise send you the notes of two other cases of interest in connexion with the pathology of the heart. In one of them, hæmorrhage into the pericardium was occasioned by ulceration through one of the sinuses at the root of the aorta, excited by ossification of the semilunar valves,—a termination of the disease which must be exceedingly rare. The other is a case of Rupture of an Aneurism of the Aorta into the right Auricle of the Heart, of which I believe there are only two similar examples on record.

I am, sir,

Your obedient servant,

T. B. CURLING.

1, Mount Place, London Hospital,
February 23d, 1838.

CASE I.—*Spontaneous Rupture of the Left Ventricle of the Heart.*

I was sent for in a great hurry on the 29th of December last, about eight in the evening, to see a Mrs. M—, an inhabitant of the Trinity Alms-Houses, Mile End, who was reported to be dying. I hastened immediately to her residence, but she had expired before I arrived. It appeared that her servant, who was in the room above, hearing a slight noise below, instantly descended, and found her mistress insensible and

gasping for breath, and in another minute she expired.

There was much opposition to the post-mortem examination on the part of the friends; but a medico-legal investigation being considered necessary, the body was opened by a coroner's order. The chest only, however, was examined. There was a tolerably thick layer of adipose tissue over the thorax. The pericardium was distended with dark coagulated blood, which completely invested the heart. On the anterior surface, about an inch from the septum of the ventricles, and two inches from the apex, there was a ruptured opening communicating with the left ventricle, about eight lines in extent, in the direction of the muscular fibres. The muscular tissue of the organ was sound, and not particularly soft; there was no appearance of ulceration in the interior of the ventricle. The coronary arteries were ossified, but the valves were healthy. There was a good deal of fat about the heart.

Mrs. M— was 76 years of age, and had been for many years a complete martyr to the gout. Her toes and fingers were crippled with chalk stones, and she had been confined to her couch from the effects of this disease for several months. I had attended her at various times during the last twelve-months, but she had never complained of any thing which induced me to suspect the existence of any disease of the heart. A few hours previous to her sudden death she had been visited by some of her family, and had appeared in excellent spirits. This would appear to be an example of the *senile* form of rupture*.

CASE II.—*Sudden Death from Hæmorrhage into the Pericardium through a small ulcerated opening in one of the sinuses of Morgagni, caused by ossification of the aortic valves.*

Thomas Ferguson, a stout man, aged 35, was brought into the London hospital, July 2d, 1836, dead. It appeared that he had been taken ill somewhat suddenly, and had expired whilst being conveyed to the hospital; but no further account of him was obtained, except that he had been labouring for

* Some interesting cases of Rupture of the Heart are contained in Farre's *Journal of Morbid Anatomy*.

some time from an affection of the chest.

On examining the body on the following day, the pericardium appeared much distended; and, on laying it open, the heart was found to be surrounded with blood partially coagulated. On removing the blood and making a careful inspection, no rupture of vessel or source of the hæmorrhage could be discovered. The heart was therefore removed for a more minute examination. The cavities on the right side, and the left auricle, were healthy; the left ventricle, however, was greatly hypertrophied. The aortic valves were ossified, and of remarkable thickness, the aperture for the efflux of blood being reduced to fully one-half its natural size. One of the irregular ossified valves was four lines in thickness, and the corresponding sinus was dilated around it. One part of the sinus, surrounding a projecting part of the ossific deposit, was extremely thin, and had a small pin-hole aperture, caused apparently by ulceration. It was evidently through this opening that the blood had escaped into the pericardium. The lungs and abdominal viscera were sound.

CASE III.—Aneurism at the origin of the Aorta, terminating by Rupture into the right Auricle of the Heart.

Nov. 28, 1834, I examined the body of John Osborn, a man aged 35, who had died in the London Hospital the previous day.

The cutaneous vessels, and superficial and large deep-seated veins, especially of the head and neck, were extremely turgid. The vessels on the surface of the brain were likewise much injected; but the organ itself was healthy. There were tolerably firm old adhesions of the pleura, on the right side of the chest, and about sixteen ounces of serum on the left. The lungs were sound, but congested. On opening the pericardium four ounces of serum were observed, and the heart seemed greatly enlarged and displaced, the right auricle being partly concealed by a tumor, which was found to be formed by an aneurism at the root of the aorta, about the size of a hen's egg. The appendix of the right auricle was completely adherent to the exterior of the aneurismal sac, which encroached very much on the right auricle, and diminished considerably the cavity. At the

pars venosa, near the ostium venosum, there was an opening capable of admitting a crow-quill, and having an irregular margin, as if caused by rupture, which communicated with the lower part of the aneurismal sac. There was a good deal of hypertrophy, with dilatation of the left ventricle; also a small ossific deposit in one of the aortic valves, and deposition at various parts of the aneurismal sac and interior of the aorta. The auriculo-ventricular valves, and the valves of the pulmonary artery, were healthy. There were about twenty ounces of serum in the cavity of the abdomen, and there was extreme sanguineous congestion of the liver, and an injected state of the portal system*.

I did not see this patient during life, but I ascertained that he was admitted into the physicians' wards in the preceding August, with general anasarca, cough, and great dyspnoea. A distinct pulsation, and a *bruit de soufflet*, were perceived on the right side of the sternum, and the pulse was weak and resilient. By bleeding, and the exhibition of elaterium and digitalis, the anasarca was removed, but the difficulty of breathing continued to increase. The countenance became livid and anxious, and he was unable to remain in the horizontal position. In the morning of the day before he died, the dyspnoea suddenly became extremely urgent; he could not bear the clothes near his chest, but kept throwing his arms about, calling out for fresh air, and struggling in the greatest possible distress for nearly twenty-four hours before he expired.

The prolonged period of the dying agonies in this case affords a striking contrast to the suddenness of death in the two preceding. In the first case death was almost instantaneous; and in the second, where the opening into the pericardium, at the sinus Morgagni, was very small, a sufficient quantity of blood to arrest the action of the heart escaped in a few moments. In the last case, however, the blood which rushed out of the aneurismal sac passed still into the current of the circulation, but so freely as to oppress the right side of the heart, and interfere with the return of blood to that organ, ending very gradually in fatal congestion of the important viscera.

* The heart is preserved in the Hospital museum.

MEDICAL RELIEF TO THE POOR.

COLCHESTER UNION.

To the Editor of the Medical Gazette.

SIR,

As any thing connected with the medical department of the new Poor Law must be interesting to the profession, I think it right to communicate, through the valuable pages of the GAZETTE, an important alteration that has taken place lately in the medical department of the Colchester Union House. The Guardians have resolved, in future, that all medicines required for the use of the paupers, in the house, should be paid for by them; and the medical officer be remunerated by salary for his services. This I consider to be a very important alteration, and a favourable omen towards a better management in the medical affairs of union houses; and I think the Chairman and Board of Guardians of the Colchester Union deserve the best thanks of the profession, in being the first, so far as my knowledge extends, in doing away with that abominable system of *drugging paupers' stomachs* by contract.

It is to be hoped that, before long, we shall have still more important changes, and that those stately buildings that have been reared up throughout the length and breadth of the land, may yet be formed into valuable medical institutions: at present, so far as regards the profession, they are next to useless. So desirable an end might be effected by intrusting the medical affairs of the several unions in the hands of a medical board in London, who should have the entire management as to the duties required, and the payment of the medical officers. I fear that if any of those who are pursuing particular departments of the profession were at present to look for information from the medical returns of unions, they would be rather disappointed, as the form now used gives but scanty information to a medical inquirer. If a form was issued to the different unions similar to that used in the public services, and the books to be yearly sent for the examination and approval of the medical board, and to be used as they thought proper, what a valuable public medical record would this constitute. It is impossible, under the present pal-

try remuneration, that the medical officers can perform this duty as it ought to be done.

From a few of your editorial remarks, in a late number of the GAZETTE, one unacquainted with the subject might be led to suppose that the present diet is not sufficient for the health of the inmates in union houses; but that such an impression is erroneous, I have only to refer to the Colchester, Lexden, and Wenstree Union Houses (to both of which I have the honour to be attached), where, by reference to the medical returns, it will be seen that the inmates enjoy the best possible health. In the Lexden House, which is altogether an agricultural union, there have been for the last quarter 292 inmates, and our daily sick list has not averaged more than seven. In the Colchester House, in about the same number of paupers, our sick list has been double; this arises in part from being a town union, and also, I may state, it has been doing the duty of a general hospital to the town, from some unhappily misunderstanding betwixt the two establishments. Indeed, in both houses the sick list is nearly made up of people entering already in a state of illness. In either a death has not occurred of any one who has been taken ill while living in the house, with the exception of two children in the Lexden House, who died of measles, out of a number of 35 cases; and these two entered while measles were prevalent amongst the children, and both were of one family, who came into the house in a sickly ill-conditioned state; in fact, with only two other exceptions, all deaths have been in persons upwards of 65 years, and in one case lately at the advanced age of 99.

These facts will, I think, prove, that if the diet table is not over-plentiful, it is sufficient for the good health of the inmates.—I am, sir,

Your obedient servant,

GEO. B. CLARK, M.D.

Surgeon to the Colchester, Lexden, and Wenstree Union Houses.

Trinity-street, Colchester,
Jan. 21, 1838.

[We readily give insertion to the above, although in opposition to the opinions we have ourselves expressed. Much more extended evidence, however, would be requisite to convince us that the diet is sufficient.—Ed. Gaz.]

ON DELIRIUM TREMENS.

To the Editor of the Medical Gazette.

SIR,

I HAVE read with considerable interest Dr. Munk's observations on delirium tremens. My attention was first called to this subject during my attendance on Dr. Hannay's lectures at the Andersonian University of Glasgow. This gentleman has for upwards of twelve years taught his pupils that the disease in question depends on inflammation of the mucous membrane of the alimentary canal, and that it is best treated by local depletion and anodynes. So far as my own observations extend, I have not the slightest hesitation in affirming that these views are correct; consequently to Dr. Munk's remarks, so far as they coincide with them, I cordially assent. There are some of his conclusions in which, however, I cannot acquiesce.

He endeavours to establish four distinct varieties of the disease, each of which he seems to imagine differs from the other in its proximate cause. 1st. He has a form resulting from debility. 2ndly. One in which much cerebral affection exists. 3rdly. Another which is of an asthenic nature, and accompanied with gastritis, *not as a cause, but as a complication*. 4thly. That form of which gastritis is the proximate cause. These distinctions I humbly submit are not well-founded, and therefore have a tendency to confuse our ideas respecting the nature of the disease. Though I freely admit that much evil is to be dreaded by too hasty generalizations, yet I firmly believe that as great danger is to be apprehended from groundless and shadowy distinctions. Dr. M. has entirely overlooked the only data on which he ought to have founded his distinctions. In the first three varieties which he makes, he does not condescend to give the least information regarding the cause of the disease. Now he must surely be aware of the truth of the maxim, "*scire vere est scire causas.*" He neither endeavours to elucidate the difference of their pathological states, nor to point out what is their dissimilarity in this respect from that of the fourth variety. Such a task would, indeed, be futile, as delirium tremens universally depends on gastritis, which, to a greater or less extent, is the real pathology of the affection; the first three

of Dr. Munk's forms being neither more nor less than modifications of the fourth, which he himself states depends on gastritis. This is the true "*origo mali*," however unwilling many practitioners are to admit the fact. But it may be asked, what proof can be adduced in confirmation of this assertion. To this I would answer, 1st. The symptoms. 2ndly. Appearances after death. 3rdly. The effects of remedies. And 4thly. The nature of the exciting cause. 1st. In delirium tremens we have a set of symptoms present in every case, however complicated it may be, or whatever modification it may assume—such as nausea, vomiting, that peculiar state of the tongue (well described by Dr. M.), tenderness and oppression at epigaster, &c. Let me ask, what do these indicate? Do they not clearly point out a disordered state of the stomach? In some cases we have symptoms conjoined to those enumerated, denoting nervous irritability; in others, signs expressive of cerebral affection; and again, we may have a class of symptoms indicating debility. These signs do not, however, occur in every case; but we have always those present indicative of gastric derangement. The others are merely expressive of complications, either lighted up by it, or induced by some peculiarity in the constitution of the individual.

2ndly. Dissections reveal gastritis in all its phases, from its slightest forms to its highest state of disorganization. It may be said that in some cases no traces of inflammation of the mucous membrane can be detected. Such instances, I presume, are indeed rare. It should be borne in mind that the inflammation of the mucous texture may have subsided, and the affections which it lighted up through sympathy in other organs have caused the patient's death.

3rdly. The effects of the treatment afford a proof of no ordinary moment. The benefit resulting from the application of leeches to the epigaster is so marked, and so instantaneous, as to leave no doubt of the presence of gastric inflammation.

4thly. The nature of the exciting cause, the constant and direct application of it to the mucous membrane, are circumstances highly confirmative of the existence of gastritis. Much more might be argued in support of this view. I will not, however, transgress any farther on your space at present.

With respect to the employment of morphia in the disease under consideration, I regard it as a remedy of the highest utility. I would perhaps hesitate to administer it in many acute cases, previous to local depletions; but when this has been premised, morphia is of the utmost value: it allays the irritability of the nervous system, and procures sleep—a circumstance of paramount importance in the treatment of delirium tremens: sleeplessness is one of its most distressing symptoms. In the cases adduced by Dr. Munk, this very symptom holds a prominent place, although he seems to have overlooked it, as he has not had recourse to any means by which it might have been obviated. In the last case which he quotes, morphia was given, and he states with the best effects. Morphia, in subacute inflammatory affections of the mucous textures, seems to possess considerable antiphlogistic powers. But be this as it may, I am confident that it is of infinite service in delirium cum tremore.

“Its balmy influence bestows
On anxious thought a sweet repose.”

The practice which I follow in the affection in question is that recommended by Dr. Hannay in his lectures, namely, the application of leeches to the epigaster, varying the number according to circumstances. After the leeches fall off, fomentations are used to encourage the bleeding; the bowels are kept open by enemata; and morphia given in doses according to the age, the constitution of the patient, and the degree of excitement under which he labours. This treatment, in all the cases which have come under my observation, was followed by immediate relief. In some instances in which leeches could not be obtained I have substituted sinapisms with very good effect. The following case may serve as an example of this:—

Jan. 3d.—A. W., a stout man, aged 26, by trade a bricklayer, complains of severe oppression at epigaster, which is painful on pressure; vomits all matters taken into stomach; eyes suffused; tongue tremulous, red at tip and margins, covered with a dirty white fur in centre. Pulse about 118, but cannot be exactly counted, from tremors, which are almost incessant in extremities. Bowels costive; thirst exceedingly urgent. At some times he imagines himself surrounded by his comrades, who are “mocking” him; at others he sees

a number of “little black fellows,” who are going to fix chains on him. When labouring under this delusion he starts from bed. He sleeps none; became affected three days ago, before which time he had been constantly intoxicated for about six days; and he is generally intemperate in his habits. Leeches could not be obtained.

Admov. Sinapism. epigast. et hab. Sol.
Mur. Morph. ʒss. 4tā qq. h. Injiciatur Enema domest. quamprimum.

4th.—Is more collected; slept a little; bowels moved by enema; tremors much mitigated; pulse 108; vomiting ceased; thirst somewhat abated; oppression at epigaster relieved, but not gone.

Rep. Sinapism, et cont. alia.

This treatment was persevered in (with the exception of sinapism), and on the fifth day from my first visit he was enabled to commence his work.

In conclusion I would remark, that it must certainly be gratifying to Dr. Hannay to think that the experience of others is daily corroborating those views which he has been carefully inculcating to his students for many years, regarding the nature and treatment of this very distressing disease.

If the foregoing observations be deemed worthy of a place in your valuable journal, by inserting them in an early number, you will much oblige

Your obedient servant,
JOSEPH BELL.

Baerhead, Feb. 15, 1883.

FEMALE WITH THREE MAMMÆ.

To the Editor of the Medical Gazette.

SIR,

In the number for Jan. 20 of your valuable journal is “The History of a Female with four Mammæ and Nipples,” communicated by Dr. Robert Lee to the Royal Med. and Chir. Society. Allow me to transmit you the particulars of a case occurring in my practice.

I was called at one A.M. during last spring to a poor woman residing in this town, who was said to be in a fit, and on entering the room found her lying on her back quite dead, with an infant about three months old still sucking at the right mamma; whilst from a small

wart-like nipple, about three inches below the breast, and rather nearer the median line, a few drops of milk were oozing.

On a post-mortem examination, I found this smaller nipple had a mammary gland about the size of a walnut, which was supplied by an artery of the breast above, returning the blood by two veins running parallel to it.

It appeared, upon questioning the bystanders, that she had never given the smaller nipple to any of her children, but had always suffered inconvenience from the escape of milk during the time the child was applied to the breast.

I am, sir,

Your obedient servant,

THOS. THURFIELD, M.R.C.S.

Kidderminster, Feb. 17, 1888.

LACED STOCKINGS.

To the Editor of the Medical Gazette.

SIR,

A PATIENT came to consult me with that peculiar kind of sore leg produced by the rupture of a varicose vein, and from irritation, bad treatment, and inattention, a wound is the consequence, with surrounding tenderness and swelling. He had had this for several years, and as his occupation caused him much walking he was anxious to get it cured, which I effected, by first strapping the leg from the knee downwards with soap plaster, thereby lessening the calibre of the veins, and getting the circulation more equal. The wound in the ankle was dressed with the lotio nig., and soon healed, and a circle of nitrate of silver drawn round to prevent a disposition to erysipelas extending. When he could bear it, and the tenderness of the skin a little subsided, the parts were first protected by the ung. zinci spread on lint, and then strapped over with the soap plaster, and removed weekly; and in order to get the skin firm and healthy, the liq. plumbi subacetatis was put on with a soft brush, and lint over that, and plastered up again: a firm healthy leg was the consequence of this treatment. I then advised an elastic stocking, which, if of a proper description, was to be used for the future.

A week afterwards he brought me a jean stocking, prepared with the caoutchouc, like Mackintosh's patent

cloaks. I did not like to put this on, and told him what I thought would be the result, which has proved to be the case. The stocking thus prepared effectually resists radiation; consequently, with a walk every day, varying from six to sixteen miles, the perspiration is formed, and remains upon the limb.

I had prepared the limb before putting on the stocking, by placing pieces of lint round it to absorb this; but the consequence is, that when I undo the stocking, twice a week, the stench of the perspiration is exceedingly offensive, independently of the skin getting into a very thin, tender, and irritable state.

This prepared stuff, now in general use for stockings of this description, is radically bad, as it takes upon itself the office of the epidermis, without its porosity, the epidermis of the patient becoming the cutis vera in consequence.

It is with the desire to prevent loss of time to patients for the recovery of their ailments, for which a laced stocking is necessary, loss of credit to the surgeon by adopting proper instead of improper means, and finding it out afterwards, for the cure of their patients, and loss to the maker of these bandages or stockings, for they never can keep a good character, that I have now addressed you; and if you find these remarks fit for your journal, you will oblige me by inserting them for a constant reader and subscriber, and

Your obedient servant,

B. RIDGE, M.R.C.S.L.

Terrace, Patney,
6th Feb. 1888

ON THE FORMATION OF PUS.

To the Editor of the Medical Gazette.

SIR,

VARIOUS opinions have been lately put forth concerning the origin and mode of formation of pus. Every one seems to agree that it is a product of inflammation, but how or in what manner the fluid itself is formed, is a matter by no means agreed upon. Equal uncertainty also prevails as to the nature and essence of inflammation,—whether this is in all cases the same affection, or whether there be several varieties of it, and what it is which constitutes its constant and invariable proximate cause, or pathological condition.

An investigation of this kind is beset with many difficulties, although there are those who would cut the matter very short, by affirming that inflammation existed when the vessels of a part took on an inflammatory action; but how this action was produced, or in what it differed from any other action, they are by no means prepared to say; indeed, the word action, as it is frequently employed, seems destitute of any precise meaning. I can easily comprehend that action may vary in its degree of force, or of regularity, but any alteration in the quality of action I do not understand. The terms inflammatory action, gouty action, rheumatic action, scrofulous or venereal action, convey no distinct notion to my mind. When a modern pathologist ascribes the origin of a scrofulous or venereal sore to the vessels of the part taking on respectively a scrofulous or venereal action, I am no better satisfied than if, when I saw a sudden gush of tears consequent to some emotion of the mind, he were to inform me that they were occasioned by the vessels of the eyes taking on a lachrymal action.

If we attend to the symptoms ordinarily present in inflammation, and the manifestation of which all persons agree to be evidence of this state, we shall not find any one to be truly pathognomonic. When inflammation arises in a part to which we have access, we discover increased circulation through that part, redness, swelling, and pain; yet is neither of these symptoms pathognomonic, as each one may take place when no inflammation is present.

By contrasting in a part the state very nearly resembling inflammation with the state of actual inflammation, we may perhaps arrive at some notion of what actually constitutes inflammation—what is its proximate cause and essential nature.

If I immerse my hand some time in hot water, there will be produced increased circulation, redness, swelling, but no pain; if to these symptoms pain was added, we should pronounce the part to be in a state of inflammation. In what consists the difference? Clearly in this; that in the one case the increased flow of blood is freely transmitted through the vessels of the part; in the other case, the increased flow is resisted by the contraction of the vessels through which it has to pass; they

resist the efforts made to distend them, and thus pain is produced, a state of inflammation is brought on. If the vessels readily receive the increased quantity of blood, no actual inflammation arises; it is the resistance offered which gives rise to disease.

It would occupy too much time to go into further consideration of the essential nature of inflammation, concerning which so much discrepancy of opinion exists. I shall content myself with assuming that inflammation is to be explained by considering the relative force of action between the propelling and the recipient vessels: whilst the natural balance is preserved, no inflammation takes place; when the force of the propelling vessels is increased the resistance of the recipient vessels remaining the same, inflammation will occur: the same thing will happen, of course, if the resisting power is increased whilst the propelling power remains the same. If the propelling power is increased, and also the resisting power at the same time, then the inflammation must be of a more intense degree, so that it may be properly said inflammation exists in a compound ratio of force and resistance.

Inflammation thus defined varies considerably in different cases in its progress and terminations—modified, perhaps, by the tissue of the part in which it is situated, or by some other circumstances not very obvious; thus gout and rheumatism, although displaying some of the characters of ordinary inflammation, differ from it in many essential particulars, and in none more than in its indisposition to the formation of pus; a fact which militates against the hypothesis of Dr. Marshall Hall, who says, "I believe that the secreting vessels are variously affected by the various degrees of inflammatory action, an effusion of serum marking the lower, a secretion of albumino-fibrin the higher degrees, and pus the highest of all;" yet in what disease does "inflammatory action run higher than in an acute paroxysm of articular gout wherein no pus is produced? By the by, the Doctor speaks merely of various degrees of action, not difference of quality. I am to confine myself to common phlegmonous inflammation terminating in suppuration, and to say a word or two on the mode in which the pus is produced.

Two different opinions have been entertained, although one is much more modern than the other, concerning the source and manner of production of pus, in inflammation; some attributing its production to a *peculiar* fermentation taking place in the fluid effused during inflammation; others to the peculiar action of the inflamed vessels *secreting* pus, and pouring it out instead of the usual fluids furnished by them in a state of health. An objection to the first opinion, the conversion of the effused fluid into pus, has been raised, on the ground that no fermentation takes place in the fluid effused; but this objection is rather verbal than solid, being founded upon a mistaken apprehension of the term fermentation. Nothing more is meant by the term here applied, than a spontaneous separation and recombination of the primary elements of a compound body.

All the solid parts of the animal body, and all the various fluids, however different in the structure and properties, consist but of a very few primary elements—as nitrogen, carbon, hydrogen, and oxygen, to the peculiar arrangement and proportion of which elements all the difference is owing. The separation of the compound body into its primary elements, and the recombination of them in different proportions, was demonstrated fermentation; the process is universally acknowledged to take place; the propriety of the term when applied to the living animal system is denied; although we have no hesitation in admitting that the process takes place, or in applying the term when the influence of life is withdrawn, viz. the putrefactive fermentation.

A stronger objection to the notion of the production of pus by fermentation, as the word has now been defined, is the fact of this fluid being found on the surfaces of inflamed membranes, where no cavity exists for the collection or accumulation of any fluid to undergo this process or spontaneous decomposition; and it is therefore contended that in this instance at least pus must be secreted. Allowing this fact its due weight, it does not explain the formation of pus out of those extravasated fluids in inflammation, which are certainly not pus when first poured out, but which become so in a short time afterwards if the inflammation con-

tinues. It is said that in phlegmons, the removal of the parts injured, or whose vitality is destroyed, is effected by the action of the absorbents, and a cavity is thus formed capable of receiving the secreted pus. But it is also said, and I believe has been proved, that pus has a solvent power, and it may, therefore, dissolve the solid part in contact with it, and bring them into a state fit for the absorbents to act upon and take up. If such be the case, the old doctrine of pus converting solids into a fluid similar to or identical with itself, may still be true; the absorption of the solid matter without its being converted into pus may be a gratuitous assumption, and would seem to be a superfluous process.

As to the secretion of pus in contradistinction to its formation, we may observe, that on the surface of membranes whereon this is said to be secreted, the fluid, at its first appearance, is not globular, the characteristic of genuine pus, but a transparent fluid, of a consistence in some sort resembling jelly: the globules are produced whilst the fluid lies on the surface of the sore. And in the pustular eruption in small-pox, the fluid in the vesicles is at first thin and transparent, afterwards becoming thick and purulent.

Purulent secretion, as it is termed, is surrounded by no less difficulties than are the various natural secretions themselves; a process not at all understood, but one which has been the subject of much discussion. An attempt has lately been made, I believe, to explain secretion by attributing this function to the electric power with which the nerves are supposed to be endued. But as all the various secreted fluids are obtained from the same identical mass (the blood), and as the action of electricity must always be precisely the same, under given circumstances of temperature and conducting power of the substance acted upon, which in the blood are always equal, how is the variety of results to be accounted for? Were this hypothesis truth, the secreted fluids might be obtained from the blood when drawn out of the body by the application of electric power, which has never yet been done. Life, therefore, is essential to animal secretion; and as electric power alone cannot effect this process, the superadding this power to the influence of life is a mere gratuitous

assumption, and contributes nothing to the explanation.

If it be said that the different secretions are produced from different constituent principles of the blood, each being acted upon by the electric power of its appropriate secreting organ, how is it that the secretion in the same secreting organ ever varies? For instance, if the effect of the electric power in the kidneys is (according to Dr. Wollaston) "to secrete acid urine from blood that is known to be alkaline," whence arises it that other conditions of the urine are produced, as sugar in diabetes? Is there a change in the degree, or in the quality, of the electric power? The degree of power is considerably within our own management; and were this alone requisite to produce secretion, we might succeed in effecting similar results out of the body, as we can obtain different degrees of intensity at our pleasure. If there be a change in the quality of the power, it is no longer electricity; to call it, therefore, electric power, is merely to substitute another name for living principle.

The same arguments will apply to the hypothesis of secretion being effected by galvanic power.

M. Gendrin has published some observations on this subject. He denies pus to be a secretion, and says that inflammation, from its very commencement, produces a state of infiltration of the tissues: at first the infiltration differs little from that in health, except in quantity. As soon as the infiltration has reached a certain stage of its development, it is very easy to recognize a copious deposition of a substance, of the consistence of jelly, interspersed between the fibres of the tissue of the part. This infiltration, interspersed between the fibres and in the interstices of the inflamed tissue, is easily recognized; not only by the presence of a fluid more or less black and gelatinous, but also of a liquid of a yellowish-white colour, which appears slightly incorporated with, or adherent to, the diseased tissue. If we examine with a microscope the fluids thus infiltrated, we recognize the pus by its characteristic globules. At the boundaries of the purulent infiltration, and in those points where the infiltrated fluid *begins* to be purulent, we detect the true globules of pus mixed with smaller globules, which still preserve, in a slight degree, the pale rosy

colour of the globules of the blood, partially deprived of their colouring-matter by rest, *after* their escape from the vessels. It is, then, evident (adds M. Gendrin) that infiltrated pus is only a modification of the spontaneously coagulable fluid, the infiltration of which has *preceded* the appearance of the pus, and which we can recognize in its *transition* into a state of pus.

Can we, then, to a certainty, say whether pus is really and strictly a secretion, or whether only the materials necessary to its formation are at first poured out, and that these are afterwards converted into pus by spontaneous separation and recombination of the primary elements, in the same manner as takes place in the conversion of farina into sugar, or sugar into wine; a process in the animal system depending upon, and effected by, the principle of life, or living principle?

It may be said that the agitation of this question can contribute but little to the improvement of practical medicine; but if (as is confidently asserted) our skill in medical science is in exact proportion to the correctness of our notions of physiology and pathology, it is worth while occasionally to inquire into the solidity of the foundation upon which our science is said to be based, although the result of the inquiry may prove that we know just as much, and no more, on the subject than was known half a century ago. We are too apt erroneously to suppose, that by changing one ill-defined term for another term equally ill-defined, and adopting some novel phraseology, we are making advance in our knowledge of medical science, whilst we are but wandering from the right path; for after all, practical medicine can only be really improved by patient study of the natural history of diseases, and the cautious investigation of the properties of remedial agents.

A plausible hypothesis, maintained with ingenuity, will sometimes wear the semblance of science, and may procure for its inventor a temporary reputation for brilliant discovery; but the best knowledge a physician can possess is that which, being derived from observation and experience, enables him most successfully to grapple with diseases at the bed-side of his patient.

MEDICUS.

Feb. 19, 1838.

MEDICAL GAZETTE.

Saturday, March 3, 1838.

"*Licet omnibus, licet etiam nihil, dignitatem Artis Medicæ tueri; potestas modo venendi in publicum sit, dicendi periculum non recuso.*"
CICERO.

THE RIVAL DISCOVERERS.

A MEDICAL man can scarcely have a greater misfortune in this country, than to love the scientific pursuit of his profession, or to become a discoverer. Sir Charles Bell (than whom no one could more fairly judge) was in the habit of telling his pupils to beware how they let the public suppose that they were any thing but practitioners. "I believe," he says, "the philosophy and the practice of the profession to be necessary to each other, and both to the true respectability of the individual. Is it not strange that the prejudice of society should run against this association of objects so natural and so just? But I have felt that it does; for on each occasion that has joined my name with science, it has been necessary to double my attention to practice*." If we look at the most successful practitioners around us, we find no one who devotes more than a very small portion of his time to anatomy or physiology, or any abstract scientific investigation; and this, not merely because their time is much occupied in actual practice, but because they have learned the danger of a high reputation for any thing but a knowledge of the *art* of medicine or surgery. Many of them, in the earlier part of their life, continued the studies of their pupillage, and first gained the public attention by their works on the sciences ministering to practice; but woe has been to those who, when they had once succeeded thus far, did not timely check

their taste for these investigations. The imputation already cast upon them needed to be wiped off by long abstinence from similar pursuits, if not by the penance of publishing some work of pure practice, in which their *science* should appear only at very distant intervals.

Many of those who read this will see their own course described; we have several in our mind who now congratulate themselves on their opportune change of pursuit, and many who are in vain trying to make scientific studies compatible with the successful pursuit of practice; but we scarcely know one who has succeeded in maintaining the compound reputation of the able practitioner and the accomplished *savant*. The *critical period*, if we may so speak, in the life of a medical man in London, is that at which he leaves off science and commences art.

But the injury which he is likely to sustain in his practice by the reputation of being a good anatomist, or physiologist, or chemist (which, with the public, are expressive of negative qualifications as a physician or surgeon), is not the only danger which the medical student of science has to encounter. If he should make what, as it were in derision, he would call a fortunate discovery, he has to run the gauntlet of numerous competitors for the honour of originality, and, pirate or not, he must fight or give up his claim. And these, too, he finds no childish combats; he has too often to bear up against hard words and ridicule, not to say positive abuse.

We have often made remarks somewhat similar to these, but we never had the ideas so strongly impressed on us as by reading certain letters which Dr. Grant and Dr. Marshall Hall have recently published, as replies to one by Mr. Newport, claiming the discovery of the arrangement of the

* Clinical Lecture, in MED. GAZ. vol. xiii. p. 697.

motor nerves in the articulata. They both, indeed, seem to have been determined to add documents well worthy of the *Bibliotheca Bellica*, and to maintain the bad spirit with which the interminable subject of the spinal cord has been too generally discussed.

Of Mr. Newport personally we know nothing, but we think that his having received very honourable notice from many besides his present persecutors, is a sufficient evidence that his character cannot be quite what they represent it, while his well-known industry, and the untrodden field in which he labours, would, *a priori*, render it very probable that discoveries might be made there. We have carefully examined the case, and to us it appears perfectly clear that his pretensions have been altogether misrepresented in the letters to which we have alluded. In his papers in the *Philosophical Transactions* for 1832 and 1834, he never claims in the most remote manner the discovery of the superior transverse nerves in the articulata; for descriptions of these he expressly refers to Lyonet and Müller, and says that their longitudinal connexions were made out by him after Dr. Grant had suggested the probability of their existence. His discovery is quite separate from this; and though it is the most prominent part of the paper, Dr. Grant *never once alludes* to it, but would imply that all the present question relates to the point above mentioned. Mr. Newport found, and has clearly proved, that the motor tract of the nervous cord in the articulata is not this which is situated above, free, and *widely separated from* the main cord, but that it forms part of the latter, being separated only by a linear depression from the anterior part, from which the sensitive nervous roots on which the ganglia are formed, arise. This principal nervous cord of the articulata, therefore, was first proved by Mr. Newport to be exactly analogous to the

spinal cord of vertebrata, consisting of two columns, from one of which arise sensitive roots bearing ganglia, and from the other motor roots, which have no ganglia, but which join the others just beyond theirs. Whether the superadded transverse nerves, which Dr. Grant had described as the motor nerves, form, as Mr. Newport imagines, from their supplying the parts connected with the tracheæ and respiratory muscles (a separate respiratory system,) is a point of far less importance. It is certain that they *do not form*, as Dr. Grant thought, the true general motor column. Surely, then, Mr. Newport was justified in his complaint, and will be so still more, since his opponents have published their accusations without even alluding, in an intelligible manner, to his real discovery, but heaping upon him the most virulent abuse, for having claimed another to which he never made the smallest pretension.

The original papers will make the truth of our opinions quite evident to any one who will consult them. But we will quote the words of Professor Müller, on whom Dr. Grant would have it believed that Mr. Newport has been guilty of piracy. He says, in his *Retrospect of the Progress of Medical Science* in 1834, "Among the most distinguished investigations in comparative anatomy, is that of Mr. Newport." Then having given him great credit for his accurate descriptions of the development of the nervous cords, of the vagi, and of the transverse superadded nerves, he adds, "An important discovery of the author is, that he has found the abdominal cord of insects and crustacea composed of an anterior and posterior pair of cords. The upper pair have no part in the ganglia, which belong to the lower pair only. In this manner the analogy of the abdominal cord of insects with the spinal system of vertebrata is now much more evident than it has hitherto

been, and the discoveries of Bell on the motor and sensitive systems of spinal nerves find their application in the invertebrata."

We have no doubt that Mr. Newport will be fully justified by all of the profession who read the above statement, or his own, if he should publish one.

But granting, for an instant, that his conduct had been utterly shameless, and that his plagiarism was quite evident, there could be no necessity for such virulence as he has been treated with. A simple statement of the facts of the case ought to have been sufficient, if his opponents do not unjustly hold their present place in public reputation. How can it affect his claims that he should be called by Dr. Grant an "ungrateful parasite," "this cunning and arrogant egotist"—or be accused of "sucking Sir Charles Bell's brain or his purse," of having "arrogant pretensions," and of "directing insolent insinuations against his eminent and honourable patron"—*videlicet* Dr. Marshall Hall? Or what evidence is afforded by the gentleman last mentioned telling us, "I fed, and clothed, and housed, this same individual! yes, I lent him money, I procured him a home in his distress for many months*," or, by way of parenthesis, referring us to the fact of his keeping an account with "Sir Claude Scott, Bart. and Co.?" We are surprised that one who is himself constantly writhing under the vexation of having his pretensions questioned, should thus attack a fellow-sufferer. From no one could accusations of this kind come with so bad a grace as from Dr. M. Hall. He vituperates Mr. Owen, and informs us that he does *not* withdraw the epithet of "*brutal*" from the Council of the Royal

Society, because they disallowed his claim to originality; and heaps the most unmeasured abuse on one who had with comparative mildness asserted his own pretensions in a case in which Dr. M. Hall was not personally interested.

This is not the only occasion on which both these gentlemen have sullied the character of their profession by the coarseness of their language, when they have been excited to attack either public bodies or private individuals; and the silent punishment which they must feel that they have received, might, we should have imagined, have made them rather more gentle. We would point them out to all students who are flattering themselves with a hope of reputation by novelty of investigations, as specimens of a class too common in the scientific world, and distinguished by a grasping, miserly anxiety after renown, for which their love, like the love of money, increases with the having.

*Crevant et opes, et opum furiosa cupido,
Et cum possideant plurima plura petunt.*

Both these gentlemen have done many things well, and have obtained a fair share of credit for them; they ought not thus to try to crush a young aspirant for fame, nor ought they to grudge him the reputation he has earned, and which they do not want; above all, if they would enjoy the good opinion of those whose applause they so much covet, let them desist from private and unproved aspersions, and from that violence and vulgarity of language which is sufficient of itself to obtain for them a denial of the attention they so vociferously demand.

It is much to be regretted that there is no means of remedying the evil that results from the coincident claiming of one discovery by several persons. The establishment of the truth is important, for the honour that redounds from success is the only reward for the labour required to ensure it; and this honour

* As this was passing the press we received Mr. Newport's answer to Drs. Grant and Hall. It places these gentlemen, particularly the latter, in a very awkward position. Mr. Newport positively states, that the *feeding* consisted in occasional invitations to dinner or tea; and that the money affair amounted to the loan of *15*l.**, since repaid!—ED. GAZ.

should be sufficient, not only to pay for the labour, but at the same time to afford compensation for the injury sustained by loss of practice, and the expenses of warfare. When a man's discovery in science is disputed, or pirated, it is, as it were, an invasion of an intellectual patent, and as such deserves punishment as the analogous case, which falls under the penalties of the law. The chastisement inflicted should correspond with the subject and nature of the offence. We should be glad to see such conduct visited, in every instance, by a published exposure by an authorized scientific society; for we are sure that a plan of this kind would insure a fair reward for unpaid labour, and secure those interested in science from the loss of much time, and the annoyance of angry feelings, which they are now so constantly obliged to incur.

The present is not the only case of dispute that we have lately noticed. Dr. M. Hall is, as usual, tilting at every one that does not acknowledge the supremacy of the reflex function, and the correctness of every view of every thing that ever he looked at. Professor Müller's short residence in England was sufficient, we understand, to break the fragile bands of scientific attachment which had existed between him and several of our *martial* literati. Mr. Mayo has published a brochure containing, amongst other matters, a copy of his account-current against the medical public; in which we see he credits himself with some very heavy sums: we are glad to hear, however, that he has consented to accept the amount which Mr. Whewell adjudicated to him, for the spinal nerves. Mr. Owen has been writing, through M. Arago, to the Institute of France, to prove that the claim which M. Coste advanced (in a memoir read before its members,) to the discovery of the allantois in the embryo of the kangaroo, was utterly unfounded, for

that that structure had (as is well known) been described by him in his paper in the *Philosophical Transactions*, long previously. M. Coste seems to have been misled by having examined Mr. Owen's plates without referring to the text; an oversight so glaring, that it is scarcely possible not to believe that it was intentional, especially as there is evidence of his having *guessed* at the structure of the chorion, which was not shown to him.

Many minor wars are also going on. The phrenologists are, of course, embroiled in hot disputes. They now fight both abroad and at home by their own little fire-side at the Phrenological Society, where the disciples of Gall and those of Spurzheim expend a great deal of their remarkable volubility in defence, each of their own master. The magnetic dispute is going on warmly, the magnetizers beating up every where for recruits, though, we believe, with more perseverance than success. In short, war is in all our quarters; there are so many about

"Jealous of honour, sudden and quick in
quarrel,
Seeking the bubble reputation"—

that one can scarcely walk abroad in safety. It is a most inconvenient state of things, and we should like to see it remedied. Many of the present champions will retire from the field as soon as they begin to acquire more lucrative occupation; but more will rise up in their places. Till men of science, generally, will agree that intentional plagiarism, and unfounded claims of discoveries, should be treated much more severely than they now are, the annoyance will of course remain. They should determine on doing this; and a general chronicle of observations, with an accurate record of their dates and publication, should be kept at some scientific society. We might then hope to have peace.

CONTROVERSY REGARDING THE IRISH COLLEGE OF SURGEONS.

A LETTER has been addressed to our publishers, by authority of the College of Surgeons in Dublin, to "require" that we should give up the name of the writer of an article regarding some of their recent proceedings, which appeared in our number of February 10th. We determined not to comply with this demand, and for the following reason:—At the time the paper in question was published, an official member of the Irish College happened to be in London, and immediately wrote an answer to our first correspondent, requesting its immediate insertion. This answer accordingly appeared in the following number of the GAZETTE; while the second part of the paper complained of, although in type and on the eve of publication, was withheld. Now we do not think that a party attacked in a journal has a right first to answer the attack, and then to require the name of the original writer, with a view to ulterior proceedings. This was the view we took of the matter, and we refer to it now as it may be of use to have the question set at rest by the establishment of a precedent. On the present occasion the principle only can be established, because it so happens that the gentleman whose name was sought, and which was always privately attached to his papers, has come forward spontaneously, and declared his readiness to enter the lists against the Irish College. This letter will accordingly be found at page 909*, *extra limites*, of the present number; and if the College or any other party choose to join in the controversy, they will of course see the propriety of complying with the conditions already imposed on Dr. Alcock.

TARTAR EMETIC IN HYDROPS ARTICULI.

THE treatment of effusion into the joints by tartar emetic, in large doses, is an improvement which we owe to the experiments of M. Gimelle.

We shall here content ourselves with giving a few general points in the treatment, in anticipation of the work which M. Gimelle is about to publish on this subject. Since the period when he first became aware of the great advantage to be derived from tartar emetic in this disease, he has treated twenty-two patients in this manner, and all have been cured. The efficacy of the remedy was the same in every case; the differences in the seat of the disease, in the age, sex, constitution, or temperament of the patient, in the cause of the effusion, or in its tendency to be acute or chronic, seemed to have no influence on the cure, which, in every case, was speedy. The tartar emetic was never administered during more than eighteen days in succession, and the dose never exceeded sixteen grains in twenty-four hours. Its first effect was soothing, and it made the pain cease almost immediately. The improvement was obviously more speedy whenever it was borne by the digestive organs, and caused neither vomiting nor purging.

In almost every case these symptoms came on during the first days of the employment of the medicine, which, nevertheless, was ultimately tolerated. When these evacuations had ceased, in some instances there was an increased activity in one or more of the secretions, such as profuse sweats, or pyalism.

The exhibition of the medicine was often followed by a slight feverishness, which sometimes lasted until night. The application of leeches at the commencement was useful, in cases where the inflammatory symptoms were very intense. At the same time poultices were employed, with or without laudanum.

The food was proportioned to the appetite of the patients; on an average, they ate a quarter of the hospital full diet, or its equivalent. Wine was given whenever it was not contra-indicated by the general state of the patient. All kinds of food were given, without selection; raw food, however, must be avoided—such as fruit, or salad, and acids; for instance, vinegar and lemon-juice.

Opium was given for sleeplessness; and dandelion *tisane* sweetened was used in every case.

Such are the general points. We will now give the details of three cases treated

after this method, by M. Gimelle, in the syphilitic wards of the *Hôtel des Invalides*. Two of them (Picard and Demolombe) are still detained in St. Côme's ward, by venereal affections.

CASE I.—Successive Effusion in both Knees.

Claude Picard, a soldier, aged 36, of a lymphatic constitution, injured by syphilis, came into St. Côme's ward on the 12th of July, 1837. His disease was caries of the nasal bones, produced by the venereal virus. His right leg had been crushed by a fall from his horse, so that he could not walk without a wooden leg. After he had been in the ward a fortnight, he was seized with pain in the right knee. In the morning, after putting on his wooden leg, he tried to get out of bed; but he had hardly put it on the ground, when the pain came on so violently that he was obliged to get into bed again. In a short time there was a considerable swelling of the knee, without change of colour in the skin; the part, too, was hotter. The pain was lancinating, and was present only when the patient moved. The swelling increased, and there was evident fluctuation.

For twenty days the patient was treated with antiphlogistics and emollients; thirty leeches were applied, and two blisters, as well as poultices with and without laudanum; but no improvement took place.

The tartar emetic was now administered, and it was given for eleven days running, in the following doses:—

First day.....	4 grains.
Second day.....	6 do.
Third, fourth, fifth, and sixth days	8 do.
Seventh day	10 do.
Eighth, ninth, tenth, and eleventh days	12 do.

For the first three days the medicine caused slight feverishness, very frequent vomiting, and abundant purging. The improvement in the knee was remarkable, for the pain had entirely ceased.

On the following days the medicine was borne by the stomach, and no longer caused purging. The improvement was now more rapid; the serous effusion was re-absorbed, and motion was soon restored.

On the twelfth day the tartar emetic was omitted; on the fifteenth the knee was of its natural size, and the cure was complete.

Ten days after the cure of the right knee the left one was attacked. This time the pain began about two inches below the patella; it was lancinating, and of the

same kind as the former one. It was hardly perceptible when the patient was in bed, but increased in proportion as he moved.

At the end of three days it passed into the knee, still preserving these characteristics. The application of fifteen leeches had but a slight effect on the inflammatory symptoms; the pain remained equally intense, the knee speedily became larger, and there was evident fluctuation. M. Gimelle now began to exhibit the tartar emetic, and continued it for eight days, in the following doses:—

First day.....	4 grains.
Second day.....	6 do.
Third day	8 do.
Fourth day.....	10 do.
Fifth, sixth, seventh, and eighth days	12 do.

As before, there was vomiting and purging for three days, during which period the pain ceased and improvement began.

On the following days the tartar emetic was borne, and the patient merely experienced slight nausea; there were no stools. The effusion was now rapidly absorbed, and the cure was perfect two or three days after the medicine had been left off.

To complete the history of this case we must add—

1st. That during both periods of the treatment with tartar emetic the patient's appetite was middling; he ate quarter-diet, and drank no wine.

2dly.—That sleep was procured by opium.

3dly.—That the patient had profuse perspiration, and an increased flow of saliva: the quantity of urine remained the same.

CASE II.—Effusion into the Elbow Joint.

A man named Clavel, aged 47, of lymphatic temperament, and a medium constitution, who had had several venereal affections, was admitted into St. Côme's ward.

The patient complained of having had a pain in his right elbow for eight or ten days, without any cause that he knew of. The pain had increased gradually, particularly when the joint was moved, and this at last became impossible; there was swelling without redness; increased heat; want of appetite; sleeplessness.

The symptoms were the same when the patient was admitted into the ward; to which we may add, that at the posterior part of the elbow, and upon the sides of the olecranon, there were two round and

fluctuating tumors, which were painful when touched.

M. Gimelle's diagnosis was, that it was a case of effusion into the humero cubital joint.

For ten days successive trial was made of antiphlogistics (such as leeches, and poultices both with and without laudanum) blisters on various spots, and mercurial and hydriodated frictions. These remedies were useless, excepting that the antiphlogistics somewhat soothed the pain. The tartar emetic was then exhibited for eight days in the following doses:—

First day	4 grains.
Second day	6 do.
Third day	8 do.
Fourth day	10 do.
Fifth, sixth, seventh, and } eighth days	12 do.

The medicine was borne perfectly well, and was omitted on the ninth day. There were neither vomitings nor copious stools, in spite of the food which was allowed the patient. The improvement was very rapid, and the cure was complete on the tenth day.

CASE III.—*Effusion into the Knee.*

Joseph Demolombe, a soldier, of strong constitution, and sanguine temperament, who had suffered from the venereal disease, was admitted into St. Côme's ward on the 8th of January, 1838. He was labouring under effusion into the knee, which had appeared for the first time, and without any apparent cause. The swelling began without pain, which did not come on till the fourth day. It was intermittent, and was felt when the patient moved the limbs.

When Demolombe was admitted, the knee had become of enormous size, and there was manifest fluctuation. The general reaction was intense. For the first three days nothing was done but applying poultices to the knee. On the fourth day twenty leeches were put on. On the fifth day the pain was relieved, but the knee was as large as before. On the sixth day two blisters were applied. The size of the knee did not lessen on the two subsequent days.

The employment of the tartar emetic was now begun, and was continued for five days, in the following doses:—

First and second days . .	4 grains.
Third and fourth days . .	6 do.
Fifth day	8 do.

The patient vomited several times on the first day, but not at all on the following ones. On the third day there were frequent and copious stools, which ceased entirely on the fourth.

On the sixth day (which was the 20th of January) the swelling had diminished by four fifths, and the disease was clearly getting well.

The bystanders now requested M. Gimelle to omit the tartar emetic, in order that they might be better able to form an opinion of the efficacy of the remedy. M. Gimelle readily granted their request, as he did not see any danger to the patient in so doing.

The medicine was omitted on the 20th, 21st, and 22nd of January. On the 20th and 21st, the improvement continued; on the 22nd, the condition of the patient was stationary. On the 23rd, M. Gimelle and the bystanders agreed that the malady was aggravated. On the 24th, the medicine was again administered in the dose of four grains, which produced vomiting and abundant stools.

On the 25th and 26th, the medicine was administered in the same dose, but was not followed by any evacuation. At this period there was no longer any fluid in the synovial membrane. The patient walked for a considerable time without feeling pain, or augmenting the symptoms; only, on pressing the joint strongly from above downwards, a slight relaxation could be felt in the parts of the synovial membrane which are extended over the sides of the patella.

From the 27th to the 30th of January, the patient walked about every day; he ate and drank as usual, and felt neither pain nor uneasiness in the knee. Nevertheless, the symptom above mentioned still remained.

On the 31st of January and 1st of February, the tartar emetic was again administered in four grain doses. There were slight vomitings on the first day only. On the 2nd of February the remedy was discontinued. On the 5th, Demolombe was entirely cured, after having taken 42 grains of tartar emetic at three different periods, the dose having never been more than eight grains in any one day.

In this patient the tartar emetic never produced any feverishness; nor were there sweats, nor more abundant spitting than usual. His sleep was always good, so that it was not necessary to give opium. Demolombe's appetite did not fail, and he continued to eat quarter-diet, and to drink wine.—*From the Gazette des Hôpitaux of Feb. 8th, 1838.*

[It strikes us, that had purging been added to the other antiphlogistic remedies employed before the administration of the tartar emetic, the use of so distressing a medicine might perhaps have been dispensed with.—TRANSLATOR.]

ROYAL INSTITUTION.

Friday, Feb. 16, 1833.

DR. AINSWORTH, of the Euphrates expedition, delivered a lecture on the Alluvial Deposits on the banks of the river Euphrates, and entered at some length on the ancient and modern geography of the country about Babylon.

Friday, February 23.

Dr. Faraday delivered a very interesting lecture,

On the Atmosphere of this and of other Planets.

Of our atmosphere the principal ingredients are oxygen and nitrogen, in the proportion of one of the former to four of the latter; but a small quantity of carbonic acid is also found in it. And to this the other arrangements in nature are nicely adapted, so that any material change in the composition of the air would be followed by the most pernicious effects; for if it consisted merely of nitrogen, combustion would be at an end: as substances which burn in common air will not burn in nitrogen, but are immediately extinguished by it. Again, substances which hardly burn in common air, burn fiercely in oxygen. Thus a small quantity of oil, being made so hot as almost to burn in air, if dipped into a vessel containing oxygen, will instantly run into combustion. The consequence of the atmosphere consisting wholly of this gas, would therefore be, that when one thing took fire, the fire would spread with the utmost rapidity to things which at present are not combustible, and every thing would soon be destroyed. And as oxygen is necessary for respiration, it is evident that we could not exist in an atmosphere of nitrogen alone. Hence we can perceive that an atmosphere either of oxygen alone or of nitrogen alone, would not be at all adapted to the present condition of this planet. As to these gases existing together in a somewhat different proportion from that in which we find them at present, although this might not materially affect combustion, it would, however, affect animal life, and superinduce disease. The weight of the atmosphere, too, is wonderfully adapted to the operations which have to be carried on in it. Indeed, of so much importance is its pressure, that if it were taken away, all the changes of season and temperature would disappear.

As regards the other planets, it is only by minute and accurate observation of these bodies, when passed by any other, that we can ascertain any thing respecting their atmosphere. Now in the case of Mars, another body, in passing it, was

observed to be considerably disturbed; whence it is concluded that it is possessed of a somewhat dense atmosphere. The Moon bears no signs of atmosphere, but continually presents one appearance, which has been observed with the utmost accuracy; and mountains, of a mile in extent, may be clearly seen with a good telescope. In observing the Moon pass a star, some have thought they discerned a slight degree of atmosphere: Sir J. South, however, has never perceived any thing of this kind, and we may conclude, that if it has any atmosphere, it is too minute for us to discern.

Sir W. Herschel at certain times described Saturn as having a bright appearance at the poles, while the rest of the planet is colourless; and this he supposed to be snow collected together in immense masses. If this is the case, then there must be water; and if there is water, there must be clouds, or something of that kind, otherwise the temperature would become equalized, and the snow would be at an end. The water, however, need not necessarily be the same as ours, nor do philosophers pretend to say that the snow is the same as ours; for if it were of the same materials as ours, the specific gravity of the planet could not be, as it is, only 1-8th part of the earth.

In Jupiter, the belts which are visible show that there is something moveable (though what we know not) on its surface. Sir W. Herschel one evening observed this planet covered with belts, and on returning, after an absence of scarcely half an hour, these had all disappeared. How this happened we cannot explain. It is, indeed, difficult to account for so sudden a disappearance in a body of so large a diameter as 87,000 miles; yet we must, of course, place the most perfect confidence in Sir W. Herschel, and content ourselves with hoping, that when still further advances shall have been made in this most interesting and important science of astronomy, we shall be able to give full and satisfactory reasons for what in this, as in other cases, appears at present strange and unaccountable. Dr. Faraday then concluded by stating, that though it has not yet been proved that the other planets have any atmosphere, yet they resemble our planet so much in other external circumstances, that we can hardly suppose ours to be the only one in which living creatures have been placed: and that He who made the globe with its atmosphere and its inhabitants, might with as great ease have placed an atmosphere and inhabitants in the others, adapted to the order and arrangement which in them he may have seen fit to establish.

OF

DRUGS ON SALE IN THE ENGLISH MARKET,

With their Prices and several Duties.

(From the Official Returns, Feb. 20, 1838.)

	PRICE.		DUTY.		DUTY PAID	
	£	s. d.	£	s. d.	In 1838 last week	Same time last year
Aloes, Barbadoes, D.P. c	12	0 0	to 30	0 0	} B.P. lb 0 2 F. lb 9 8	7,800 13,233
Hepatic (dry) BD. c	5	0 0	14	0 0		
Cape, BD. c	2	8 0				
Aniseed, Oil of, German, D.P. lb	0	9 6	0	9 6	F. lb 1 4	64 129
E. I. lb	0	7 0	0	7 6	E. I. 1 4	29
Asafoetida, B.D. c	2	10 0	5	0 0	c 6 0	44 25
Balsam, Canada, D.P. lb	0	1 3	0	1 4	lb 0 1	111 142
Copaiba, BD. lb	0	6 6			c 4 0	23 3
Peru, BD. lb	0	4 3			lb 1 0	115 63
Benzoin (best) BD. c	25	0 0	50	0 0	c 4 0	3,039 3,514
Camphor, unrefined, BD. c	9	0 0			c 1 0	291
Cantharides, D.P. lb	0	5 6			lb 1 0	3,240 1,836
Caraway, Oil of, D.P. lb	0	9 0			lb 4 0	851 669
Cascarella or Eleutheria Bark, D.P. c.	1	15 0			lb 0 1	
Cassia, Oil of, BD. lb	0	7 0			lb 1 4	
Castor Oil, East India, BD. lb	0	0 6	0	0 10	c 1 3	820 1,104
West I. (bottle) D.P. 1 1/2 lb	0	2 3				
Castoreum, American lb	1	15 0			lb 0 6	196 147
D.P. Hudson's Bay lb	1	0 0	1	4 0		
Russian lb			none			
Catechu, BD. Pale c	1	8 0			c 1 0	625 8,495
Dark c	3	0 0				
Cinchona Bark, Pale (Crown) lb	0	2 0	0	3 6	lb 0 1	12,580 38,495
BD. Red lb	0	3 0	0	6 0		
Yellow lb	0	2 0				
Colocynth, Turkey lb	0	2 6	0	4 0	lb 0 2	858 1,694
D.P. Mogadore lb	0	3 0			lb 0 2	3,393 4,898
Calumba Root, BD. c	1	4 0	2	5 0	lb 0 6	8,174 6,889
Cubebs, BD. c	5	0 0	15	0 0	c 4 0	23 65
Gamboge, BD. c	1	4 0			c 4 0	36
Gentian, D.P. c	0	1 0	0	1 8	c 6 0	
Guaiaicum, D.P. lb	8	0 0	9	0 0		
Gum Arabic, Turkey, fine, D.P. c	5	0 0	7	0 0	c 6 0	1,612 784
Do. seconds, D.P. c	3	0 0	3	3 0		
Barbary, brown, BD. c	4	15 0			c 6 0	75 437
Do. white, D.P. c	3	0 0	3	10 0	c 6 0	1,497 69
E. I. fine yellow, BD. c	1	15 0	2	5 0	c 6 0	141 42
Do. dark brown, B.D. c	4	15 0	5	0 0	c 6 0	810
Senegal garblings, D.P. c	8	0 0	12	0 0	lb 0 1	3,127 2,508
Tragacanth, D.P. c	0	0 2 1/2	0	0 3	lb 0 6	4,308 6,713
Iceland Moss (Lichen), D.P. lb	0	1 2	0	2 0	lb 0 3	1,424 7,754
Ipecacuanha Root, B.D. lb	0	2 3				
Jalap, BD. lb	0	4 0	0	5 6	lb 0 3	304 336
Manna, flaky, BD. lb	0	1 7			oz 6 0	16 43
Sicilian, BD. lb	1	0 0	1	8 0	lb 2 6	162
Musk, China, BD. oz	5	0 0	15	0 0	lb 1 0	4,558 5,184
Myrrh, East India, BD. c	2	0 0	11	10 0	lb 4 0	20 129
Turkey, BD. c	0	8 0	0	9 0	lb 0 1	30,534 30,829
Nux Vomica, BD. lb	0	13 6			lb 1 0	4,710 5,503
Opium, Turkey, BD. lb	1	1 0			F. lb 1 0	918 1,936
Peppermint, Oil of, F. BD. lb	0	3 6	0	3 3	lb 1 0	919 1,243
Quicksilver, BD. lb	0	2 6	0	3 3	lb 0 6	16,553 13,046
Rhubarb, East India, BD. lb	0	3 6	0	4 0		
Dutch, trimmed, D.P. lb	0	8 3				
Russian, BD. lb	0	18 0			lb 1 0	919 1,243
Saffron, French, BD. lb	0	19 0				
Spanish lb	0	1 0	0	1 9	lb 0 6	16,553 13,046
Sarsaparilla, Honduras, BD. lb	0	2 0				
Lisbon, BD. lb	0	12 0	0	15 0	lb 2 6	1,975 1,791
Scammony, Smyrna, D.P. lb	0	0 3	0	0 4	E. I. lb 0 6	10,975 15,137
Aleppo lb	0	1 6			Other sorts 0 6	15,812 14,754
Senna, East India, BD. lb	0	1 0	0	1 3		
Alexandria, D.P. lb	0	1 0	0	1 3		
Smyrna, D.P. lb	0	1 0	0	1 3		
Tripoli, D.P. lb	0	1 0	0	1 3		

‡‡ BD. In Bond. — c. Cwt. — B. P. British Possessions. — F. Foreign. — D. P. Duty paid.

ACCIDENTS ADMITTED AT THE LONDON HOSPITAL.

*Surgeon, MR. SCOTT.
Assistant-Surgeon, MR. HAMILTON.*

Jan.	Sex.	Age.	Case.
30.	M.	47	Lacerated scalp.
	M.	48	Sprained ankle.
	M.	21	Do. do.
	F.	19	Scalded feet.
	F.	77	Fractured arm.
31.	M.	55	Fractured leg.
	M.	23	Contused wounds of the head and concussion of the spine.
Feb.			
1.	M.	36	Compound fracture of the humerus.
	M.	57	Hernia (reduced).
2.	M.	39	Wounded leg.
	M.	47	Fractured ribs.
	M.	6	Burn (dead).
	F.	43	Injured abdomen.
3.	M.	78	Fractured patella transverse.
	F.	13	Burn (dead).
4.	M.	10	Wounded foot.
5.	M.	39	Retention of urine.
	M.	20	Dislocation of the humerus and fracture of the scapula.
	M.	44	Injured eye.
			In-Patients 19
			Out-Patients 30
			<hr/> Total 49

*Surgeon of the Week, MR. LUKE.
Assistant-Surgeon, MR. CURLING.*

Feb.	Sex.	Age.	Case.
6.	M.	19	Sprained ankle.
	M.	23	Contused foot.
	M.	45	Contused head and face.
	F.	22	Scald on the foot.
	F.	45	Contused back.
	F.	40	Contused side.
	F.	14	Punctured wound of the hand.
7.	F.	12	Contused hip.
	F.	45	Injured eye.
	F.	17	Contused head.
	F.	28	Contused knee.
	F.	48	Contused side.
	M.	17	Concussion and contused head.
	M.	58	Fractured ribs.
	M.	17	Compound fracture of the thumb requiring amputation, and lacerated wound of the hand.
8.	M.	48	Laceration of the scalp.
	M.	22	Sprained ankle.
	F.	65	Strangulated femoral hernia (operated on.)
9.	M.	26	Concussion.
	M.	52	Lacerated wound of the scalp.
	M.	19	Contused knee.
10.	M.	30	Contused side.

F. 26	Gun-shot wound of the face (a.)
F. 26	Cut head.
11.	F. 12 Burn.
	F. 4 Scald.
	M. 49 Lacerated wound of the side.
12.	M. 41 Lacerated wound of the knee.
	M. 45 Sprained ankles.
	M. 37 Contused back.
	M. 58 Fractured ribs.
	M. 56 Fractured tibia and fibula.
	F. 82 Fractured fibula.

In patients 33
Out-patients 35

Total 68

(a.) In this case the wound was inflicted with a bullet from a pocket pistol, which was discharged about a yard from the patient's face, by a young man who did not know that it was loaded. The ball entered on the right side of the nose, traversed obliquely downwards to the left side, avoiding the palate, and appears to be lodged somewhere behind the angle of the lower jaw. Blood continued to ooze from the nose and mouth for some hours. There was much pain, swelling, and constitutional irritation, for several days; but by the repeated application of leeches and other means, this has subsided, and the woman is now in a fair way for recovery; but the bullet has not yet been extracted.

NOTICE.

We have received several letters on the subject of the discussion which is now taking place in Dublin, with reference to some recent regulations of the Irish College of Surgeons. We are unavoidably compelled to postpone their publication. Among them we ought to mention a letter dated Feb. 10, from "A Professor in the School of the Apothecaries' Hall," in answer to one which had appeared in a preceding number.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, Feb. 27, 1838.

Age and Debility 37	Hooping Cough 7
Apoplexy 2	Inflammation 17
Asthma 18	Bowels & Stomach 3
Childbirth 1	Brain 6
Consumption 47	Lungs and Pleura 11
Convulsions 21	Influenza 2
Croup 1	Insanity 1
Dentition or Teething 6	Liver, diseased 1
Diarrhoea 1	Measles 2
Dropsy 9	Mortification 3
Dropsy in the Brain 6	Paralysis 3
Epilepsy 1	Small-pox 7
Erysipelas 1	Stricture 1
Fever 11	Tumor 3
Fever, Typhus 8	Unknown Causes 48
Heart, diseased 3	
Hernia 1	Casualties 10

Decrease of Burials, as compared with the preceding week } 27

CHANGES IN THE
ROYAL COLLEGE OF SURGEONS IN IRELAND.

To the Editor of the Medical Gazette.

SIR,

I SUBSCRIBE my name to this letter. My former signature of "Member of the College of Surgeons," was a real and not a feigned one, and I adopted it not for the purpose of concealment, for voluntarily and of my own accord I transmitted to you my name, but under the impression that the letter of a member of the Corporation would excite more attention than my own name would command.

I was not unprepared for an intemperate reply to my letter of the 10th ult., which I here avow, but I confess that the reply which has been attempted has taken me altogether by surprise. If my letter were, as asserted by your correspondent, "neither more nor less than a collection of direct falsehoods," what more was necessary than to have refuted them? *Magna est veritas et prevalebit!* I am therefore much at a loss to explain the total abandonment of temper and courtesy displayed in that reply. Did the writer imagine that he would thereby deter me from my purpose? If so, he has greatly mistaken me. Vituperative violence never shall deter me, while I feel myself in the right. Did he imagine that he could thereby lure me to descend from my position, and to imitate his example? No, no, sir! He that has truth on his side can afford to keep his temper; and no man loses it until he has lost his cause. Can it be, as his forward authority to you to publish his name would seem to imply, that he intended to impress you, the profession, and me, with the idea that he contemplated maintaining his charges even by the *duello*? If so, I beg to assure you that he must do so by other means, at least so far as I am concerned: or can it be

that this violence was a last resource—a gamester's hazard—to retrieve the falling fortunes of his party? I cannot answer. The result must decide.

He has pronounced my letter to be "a collection of direct falsehoods," and he has "*pledged* himself to support his statements by reference to the minutes of the College and its committees." I accept his challenge, and I call upon him to redeem his pledge. I call also upon you to bear witness to the pledge, and to be umpire in the quarrel. And now for the falsehoods:—

No. 1 states that my assertion "that the publication of an article headed Irish College of Surgeons—Pharmacy Laws, was not sanctioned by the College," is absolutely untrue. My statement, sir, is, that the article has not been published "by the order and with the sanction" of the College. And I repeat it. If it had been, the minutes of the College would contain the order, and a record that its sanction for the publication had been asked and given. I demand the production of them. They exist not. He has told you, and perhaps will tell you again, that it was done by the committee. Sir, I never said that it was not. In truth, until I had read his letter, I did not know by whom it was done, for the statement is anonymous, and could not be recognised as the act of any party. But, sir, the committee is not the College, and I recognise no right in any committee to make use of the name of the College, and above all an anonymous use of it, without its express authority and sanction. And I call again upon the College to see to it.

No. 2.—I refer to No. 6.

No. 3.—"The proposed change has not received due consideration, or sufficient sanction from the College, or the profession." In proof of the falsehood

of this, he informs you that "the proposed change has been under consideration since the month of November last;" *i. e.* from November 9th, when the committee was appointed, to January 8th, when it agreed to its report. An amazing length of time, doubtless, for so unimportant a measure! But, sir, the truth, according to his own statement, (for out of his own mouth I'll judge him,) is that the proposed change had not received any consideration at all. For what does he say? "The appointment of a committee" to consider the change! No; but "to take measures for its accomplishment was at that time made," &c. So that, according to his own admission, the first step taken in the business was to appoint a committee for its accomplishment. In ordinary cases, sir, the first step is to appoint a committee to decide upon the principle, and the propriety of its adoption, but that seems to have been deemed unnecessary in this case, and to have been disregarded. In the second place, he tells you "that the report was unanimously agreed to at one of the *largest* meetings of the College known for years!" Sir, this is a rash—a mad assertion. In reply, I demand to know, 1st, how many members composed the College at which the report was agreed to? 2d, What is the greatest number of members who have attended a meeting of the College within, we shall say, the last five years? On the true answers to these questions I might rest my cause; they alone would suffice. He terms my proportion between the number who voted upon the question and that of the entire body, an equivocation; why I cannot say, since he does not deny the accuracy of my premises. But he says that "there are not more than 90 members resident in Dublin." Well, where were they? Half that number did not vote upon the adoption of the report. Where were the rest? The entire number, more or less by three or four, have been present at the College within the time which I have specified, and why were they not on this occasion? But he says "more than one-third, in consequence of old age, indolence, &c., never attend." There is not, I believe, sir, at this moment, a single member, resident in Dublin, incapacitated from attendance by old age, and I hardly think that you will admit indolence to be a sufficient apology for absence on such an occasion.

No. 4. "The proportion of the communications from the country, favourable to the change, does not exceed a third of the entire." I demand that the falsehood of this statement shall be proved by the production of the letters. And here I have to lay before the profession a flagrant breach of privilege committed against me. I have sought to see these letters. It is my right, as a member of the College, to see them if I please, and when I please, and they are withheld from me. They have been removed from the College, and (as I have been informed by the secretary of the committee) intrusted to a clerk! to make an abstract of them! An abstract of ninety-eight letters from different persons!!! And the secretary declines to inform me who has them, or where the person is to be found, without leave of the committee, who have no authority in the matter.

But leaving your correspondent to redeem his pledge, I shall here submit the published opinions of those practitioners in the country, who have as yet, to my knowledge, expressed them. The profession in Cork have declared themselves averse to interfering with the province of the apothecary. In Limerick they object to the "union of the practice of medicine and pharmacy in the person of the apothecary;" and, on the other hand, they state "that they would by no means sanction the sale of medicines, compounded or otherwise, on the part of the physician or surgeon, whether prescribed by himself or others." (See Limerick Standard, January 26.) And in Sligo, the *licentiates of the College* resident there, and six in number, have declared "that they never have to the College authoritatively offered any opinion whatever on the subject of the bill." And, "though they may possibly anticipate, they would lament, the necessity which should require them to make up medicines for their patients." (See Sligo Journal.) Sir, I ask, are these "unreservedly favourable?"

No. 5. "The licentiates of Dublin have actually protested against the measure." "This is totally false." The following is an extract from the report presented by their committee to the general meeting of the licentiates, adopted thereby, and presented to the pharmacy committee. It has been taken from a copy of the report furnished to me by a member of the licentiates' committee:—

"They must, at the same time, express their deep regret, that, to the discredit of the medical profession, instances are not wanting where a licensed practitioner has combined the profession of the physician with the business of the apothecary, an union which your committee strongly deprecate, as one disreputable to our profession, as at present constituted, unjust to the apothecary, and highly dangerous to the interests of society." What think you now, sir? Do the licentiates, "with very few exceptions, heartily concur in the proposed change?" Have they or have they not protested against it? Your correspondent states that those of them, who are advocates for another plan, are yet willing and *anxious*, in the event of its failure, to recur to the general-practitioner-system as an alternative. Hear what they do say:—"Should they fail to procure from parliament the redress they *with confidence* demand, they feel there will be left for them but one alternative, namely, to look for an enactment by which the physician and surgeon will be entitled to dispense medicines to their own patients; a course, however, to which they acknowledge *it will be repugnant* to their feelings to be obliged to bend." Where is the falsehood now, sir?

Nos. 2—6. No. 2. All "the provisions which it is intended that the Act of Parliament shall contain have not been made public." No. 6. "They appear to me to be only a mask for another as yet not avowed design." To the latter he replies—"I can only on the part of the College, the committee, and myself, flatly deny this statement." You smile, sir, I doubt not, at such a method of proving my assertion false, that "they *appear to me* to be so." I have resolved to refrain from comment; else surely I might make one on the propriety

of gentlemen learning the meaning of language before they venture to use it. But we will suppose his meaning expressed. Now, sir, the published statement contains but six propositions. The petition contains seven; I *demand* the seventh. Is it, or is it not, part of the instruction to the committee that the College shall be empowered to educate and qualify a general practitioner? I *demand* the answer. The published statement contains it not.

Before I conclude, I beg to remind you that you have given insertion to the charges against me on a *pledge* that they should be substantiated. You will therefore not think it too much that I should now expect that you require the fulfilment of that pledge. If it be not redeemed you have been duped as much as I malign'd, and I regard it as an act only of impartial justice that my accuser be required to fulfil his pledge, and that he shall not be permitted to take shelter under any pretext. Let him not imagine that he shall escape under cover of College rules; he was not ignorant of them when he gave the pledge, which was to stamp accuracy upon his statements, and College rules shall not now save him from its redemption. He is a committee-man, and it would seem can do as he pleases. At all events, he can easily obtain from the College, and I will advocate and vote for it, dispensation from any rule which may stand in his way.

I beg to remind the profession that under the bill which is to be sought they are to sell medicine.

I have the honour now, sir, to subscribe myself,

Your very obedient servant,
BENJAMIN ALCOCK,
M.B. M.R.C.S.I.

Dublin, Feb. 24, 1838.

THE
LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MARCH 10, 1838.

LECTURES
ON THE
PHYSIOLOGY AND DISEASES OF
THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL
AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of
1836 and 1837,

At the Anatomical School, Kinnerton-Street,
near St George's Hospital,

By CHARLES J. B. WILLIAMS, M.D. F.R.S.

LECTURE XV.

Diseases of the Air-tubes (concluded).—Changes of Structure: Thickening of the Membranes; Hypertrophy of the Longitudinal Fibres; Rigidity; Pathology and Signs; Treatment.—Dilatations of the Bronchi; Varieties; Pathology and Mode of Production; Symptoms and Effects; Physical Signs, and their distinction from those of Phthisis; Amphoric Sound on Percussion; Treatment and Prevention.—Ulcers of the Bronchi.

WE have now to notice certain changes of structure which the air-tubes occasionally present. These are, I believe, generally the consequences either of inflammation, or of some kindred modification of the vascular function affecting the nutritive process. Inflammation, when often recurring or long continued in the bronchial membranes, as elsewhere, induces a change of structure; and the mechanical forces to which they are subjected in the function of respiration, may modify this change in various manners.

The most simple change of structure is a mere thickening of the mucous and sub-mucous membranes, which you see exhibited in these drawings. This generally, in some degree, accompanies acute inflammation; but it is then only temporary, and subsides as the secretion becomes free

and albuminous, being caused, probably, by only an infiltration of the pores of the tissues with soft lymph, which, as the inflammation subsides, is eliminated and expectorated with the mucus of the membrane. The deposits that are the most readily produced by inflammation in highly vital tissues, are also the most readily removed; and thus it is, that the soft albuminous matter that is effused by acute inflammation in cellular textures, and in parenchymata in general, if it be not so abundant as to interfere with the absorbent functions of the vessels, becomes absorbed as the inflammation subsides.

But it is otherwise when the inflammation recurs frequently, or is of long duration; for it then causes an effusion of a less absorbable nature, involves the less vital structures, and, as the changes induced are slow, so they are more permanent, because they become identified with the nutritive or reparative functions of these tissues. There will then be produced a degree of hypertrophy of some or all of the various tissues composing the tubes. Sometimes there will be an extraordinary growth of the mucous membrane, such as appears to be represented in this plate of Dr. Carswell's, where there is also a great dilatation of the tubes. More commonly, however, it is the harder and less vital textures, that undergo the change, and its effect is to increase the rigidity of the tubes, so that there is a diminution of their expansibility and contractility. Nothing is more common than to see the air-tubes of persons who have long suffered from bronchitis, presenting an undue development of the longitudinal elastic fibres; whilst in other cases the outer cellular coat of the larger bronchi is thick and indurated, and their cartilages are sometimes partially ossified. Any of these changes has the effect of rendering the lungs less easily expandible in respiration; the first in particular is a common cause of the short breath, which persons

frequently affected with bronchitis generally manifest; and although not often serious in itself, yet it may so abridge the sphere of the function of respiration as to make its increased exertion, on bodily exercise, a matter of difficulty and disorder, and to render it ill able to bear any other attacks of disease, to which the lungs can in general adapt themselves by supplementary efforts. Thus, when one portion of a healthy lung is attacked with pneumonia, or compressed by a pleuritic effusion, its function is supplied by the increased and quickened movements of the other portions, which, in their natural state, are equal to this augmented task; but if their pliant elasticity be impaired, and their size more fixed by an increased stiffness, they will be, in proportion, less available for additional exertion, and the body will suffer the more from the crippled state of the function.

The chief sign of hypertrophy of the longitudinal fibres, and of increased rigidity of the tubes generally, is, *difficulty of inspiration*, which is short, quick, and performed with an effort, especially on making any exertion; whilst the expiration is comparatively easy; but both acts are often accompanied by wheezing sounds, from irregularities in the caliber of some of the tubes, and frequently from partial congestions or inflammation, from which tubes thus diseased are rarely free. The vesicular murmur is impaired, and the expansion of the whole chest is perceptibly limited. These signs resemble those of spasmodic asthma, except that they are permanent, and are not removed as the latter may be for an instant, on breathing after holding the breath, as I described to you in the last lecture.

Inasmuch as these lesions seem to arise from continued inflammation, it becomes of the more importance to direct remedies against those forms of bronchitis that are habitual, or frequently recurring. An imperfectly cured cough will often hang on a patient for months, and even for years. Not being much incommoded by it, and the general health not suffering materially, he will not think it worth while to persevere in the use of remedies, or of a proper regimen. In the process of time, however, especially under the influence of fresh colds, to which he is always more liable, the breathing becomes permanently shortened, and an irritation is often fixed in some of the affected tubes, and manifests its effect on their secreting function by an habitual expectoration, generally of a thin mucous, or pituitous character. This affection varies greatly in degree. I have seen several cases of severe habitual dyspnoea, which ultimately proved fatal, present all the characters which I have described; and there was found, after

death, no other lesion than a general redness of the membrane lining the larger tubes, and an extraordinary development of the longitudinal fibres. This appearance is very common in the bodies of old people who have long been subject to cough and shortness of breath; but I have seen it also in the middle aged, and in a few instances in younger subjects. It is not, however, met with in all cases of protracted bronchitis; these sometimes lead to other and opposite results; and it is probably connected especially with some kinds of inflammation, which, as in other situations, show a particular disposition to affect the fibrous tissues; but further observations are wanted on the subject. There is one point with regard to treatment, which is suggested by a knowledge of this change of structure—that not only should we persevere in the use of the means which tend to eradicate the low degrees of inflammation that produce it, especially alkaline expectorants and counter-irritants, but we should also endeavour to countervail, by mechanical means, that mechanical limitation which this change induces in the size of the tubes. If the patient use no exertion, and give his lungs little play, any increase in the rigidity of the tubes will more readily fix them in their present contracted state: but if he take moderate exercise, increased as habit improves his power, the lungs will be kept in that free mobile condition that is least favourable to rigidity or deposition of any kind. Probably even special efforts of inhalation would be useful with the same view; and as this might be combined with some mildly stimulating vapour, such as that of water impregnated with tar, camphor, or the like, it might be made also serviceable in improving the condition of the secreting membrane. You can readily perceive, however, that great discretion is necessary in the employment of these mechanical means; for if they strain the tubes beyond the due limits, they may cause a morbid yielding of their walls, and increased inflammation; and if exertion be used beyond what the function of respiration can support, it will occasion congestions in the lungs, which may aggravate the original disease, and may induce lesions of other kinds. They are more eligible in young than in old subjects, for in the latter the change is more likely to be permanent, under the influence of that general law by which, as age advances, fibrous tissues tend to assume a cartilaginous hardness, and cartilage becomes rigid with osseous matter.

There is another kind of alteration of the air-tubes that has attracted more attention than those which I have hitherto described; I mean *dilatation of the bronchi*.

This affection deserves attention, not only on its own account, but also because it sometimes produces physical signs which closely resemble those of phthisis. The drawings before you exhibit different forms which the dilatations assume. To show them well, the bronchi should be laid open, from their large to their small branches, with a pair of scissors; and in examining lungs in this way, it is not at all uncommon to find the calibers of the branches larger than those of the trunks from which they proceed, the enlargement being generally most manifest in those parts of the tubes where the cartilaginous plates are small and few; but occasionally the larger tubes are dilated also, their rings only here and there limiting the dilatation. Sometimes the dilatations are pretty uniform through some length of a tube (as in fig. 1.) In other cases they form irregular roundish cells, or pouches, freely communicating with each other, and from which tubes of unchanged size here and there arise (fig. 2.) The tissues composing the tubes are generally, at the same time, more or less altered. They are least

so in the tubular form of dilatation (fig. 1), in which the coats are often quite thin, and the longitudinal fibres are distinct, although occasionally enlarged. But in the more globular dilatations, the walls of the tubes are generally much altered. They are irregularly thickened; the thickening being formed in part by hypertrophy of the mucous or submucous tissues lining the cells, which sometimes form folds or wrinkles around the tubes, and partly by a dense tissue on their outside, probably consisting of the parenchyma of the lung compressed by the encroaching tube. There is little or no trace of the longitudinal or circular fibres in this form of dilatation, and the lining membrane is generally in a softened state, and of a red colour, whilst there may be considerable rigidity in some parts of the tubes. This is the worst kind of dilatation, as you may suppose, from its more complicated character; but you will be better able to understand this if we examine a little further into the causes and pathology of dilatation of the air-tubes.

Laennec, who first described these le-



FIG. 1.

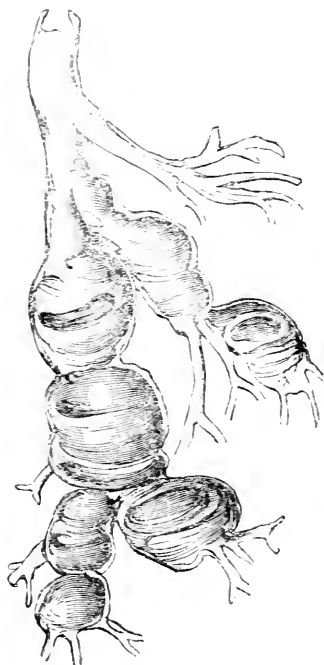


FIG. 2.

sions, attributed them to the frequent accumulation of mucus in the tubes, causing their mechanical distension. He con-

sidered that they were formed especially by long-continued chronic bronchitis, and that the continual recurrence of the same

distension of the tubes led to their permanent dilatation. But this view has always appeared to me to be quite insufficient to account for the remarkable changes which we frequently see in the structures of the dilated tubes; and, according to my observation, these lesions do not by any means constantly occur where the bronchial secretion is copious, and most calculated to cause distension. Neither do we in chronic bronchitis often meet with such a complete suspension of the respiratory murmur as these supposed distensions with mucus ought to produce. The expectoration is generally more diffuent, and less likely to accumulate in the tubes, than that of dry catarrh, and the latter stages of acute bronchitis. M. Andral takes a more rational view of these lesions, in ascribing them to a modification of the nutrition of the textures composing the tubes; but he does not attempt to give any specific explanation of the mode in which their form becomes so remarkably altered.

If you bear in mind all the circumstances of the mechanism of respiration, as we have been considering them in this course, and the various modes in which they may be deranged by the effect of disease on the textures which form parts of it, you will soon find no difficulty in accounting, in several ways, for the dilatations of the air-tubes, as well as for the modifications of nutrition which accompany them, and their effects on the adjoining tissues. I have had occasion to point out to you more than once that a mutual pressure is continually exerted between the interior of the bronchial tree and the air admitted into it by respiration; in inspiration, by the air which enters to distend the tubes; in expiration, by the tubes contracting to expel the air. In forcible acts of respiration, such as coughing, or energetic breathing, this pressure is increased; but in the normal condition of the tubes, when they all convey the air equally and freely to and fro, and meet the pressure with a well-proportioned degree of elasticity and contractility, this pressure is balanced and borne well; and instead of causing disturbance, it serves to keep the air in a constant relation to the blood, and to regulate the circulation through the lungs. But disturb in any way the equality of this pressure, or derange those elastic and contractile properties which are opposed to it, and you may then convert it into a cause of unnatural distension in some parts, whilst it does not reach others with sufficient force. Now there are several circumstances that may cause these disturbances, and they are especially to be met with in those diseases which are known to lead to dilatation of the air-tubes. Bronchitis may act in both of these ways. By

thickening of the membranes or viscid secretions it may cause partial or complete obstructions, which by preventing the free entry of air into some tubes, give increased force to its pressure in others, which become distended in consequence; and it may so alter the condition of the tissues composing the tubes, that, losing their elastic and contractile properties, they yield to the pressure, and become fixed in this dilated condition. Perhaps, as Dr. W. Stokes has suggested, the mere loss of contractility may be sufficient in itself to cause dilatation of the bronchi; but I think that our view will be more complete if we take into account other circumstances which we know to be often present, and the operation of which is perfectly intelligible. Just glance at this sketch (fig. 3), and you will see at once how

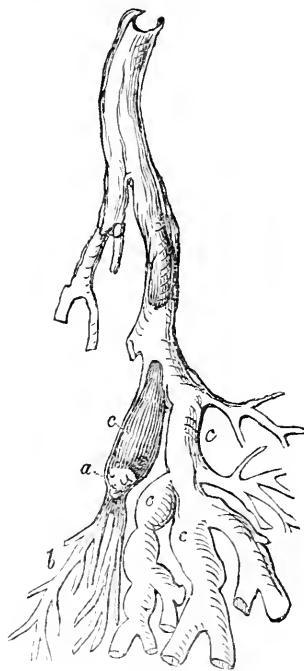


FIG. 3.

an obstruction (*a*) preventing the air in inspiration from entering one set of tubes (*b*), will cause the excessive distension of the adjoining tubes (*c*); and recollect that this is taking place in tubes softened or otherwise modified by inflammation, and you will perceive how the dilatations may become perpetuated, and liable to increase through the altered condition of the constituent textures. Hence the lesions are often not simply dilatations of the tubes, but comprehend also irregular softening

and indurations, absence and thickenings of their several textures; so that when the lung is cut open after death, it may be at first difficult to distinguish that the irregular cavities which it presents are formed by dilated tubes. Then in the production of these dilatations we are not to forget the influence of violence in the acts of respiration. They have been observed especially to succeed to hooping-cough and other bronchial affections in which the cough is particularly violent and long-continued. The effects of these violent acts of breathing may be two-fold; they may cause the dilatations by the prolonged and forcible inspiration in the manner already described; and they may increase them irregularly when so distended, by the sudden pressure of the expiratory forces upon them. But I have met with cases of dilated bronchi in which there had been very little cough, and none of any violence; and here we must suppose that the other conditions, the irregular introduction of air and the yielding of the membranes, were more exclusively concerned in the production of the lesion. I will tell you of another manner in which the bronchi become dilated. In the disease called pleuro-pneumonia the lung is inflamed, and at the same time compressed by an effusion in the sac of the pleura. Now if it remain long in this state, the smaller air-tubes and cells become obliterated by the adhesion of their sides, so that when the liquid is removed from the pleura they will not expand again with the enlargement of the chest; but the large and middle-sized bronchi are not obliterated; they bear the whole force of the inspired air, and become consequently dilated by it. This kind of dilatation is usually conjoined with contraction of the affected side: we shall notice these cases hereafter; they are not very uncommon, although they are not to my knowledge noticed by any writer. Any other circumstance which causes the obliteration or obstruction of a considerable number of the bronchial tubes and cells, must tend to produce a dilatation of the adjoining tubes on which the motions of the chest would act with augmented force.

The symptoms produced by dilatations of the bronchi will be according to the extent of the lesion. Slight degrees of it are met with in the bodies of persons who had not during life manifested any prominent disorder of the respiration; and its simpler forms may exist to a greater extent without producing other effect than a liability to attacks of bronchitis. But where it affects many tubes, has modified their structure, and has enlarged them to such an amount that they press on and obliterate a considerable extent of the

pulmonary parenchyma, it then causes habitual dyspnoea, with more or less cough and mucopurulent expectoration, which is often remarkable for its force. There are, generally, present also the ordinary symptoms of severe chronic bronchitis, from which some parts of the affected tubes are scarcely ever free; and the permanency of these symptoms, together with a degree of lividity, dropsical effusion, and cachectic condition, often induced by the crippled condition of the lungs, forms the usual general character of the aggravated forms of dilated bronchi.

Now you may say that these symptoms look very like those of consumptive disease; and so they well may, for there is injury to the function of respiration, profuse expectoration, hectic fever, and the patient is often slowly wasted away. In their aggravated forms, dilated bronchi are not more tractable than tubercular consumption itself; but their tendencies and constitutional effects are different, and merit as much of a distinction as our means of diagnosis can find for them. Unfortunately, this is not one of a very marked kind; in fact, in many instances it is to be made only by those much experienced in diagnosis; and even they will give it in terms rather of probability than of certainty.

You can readily understand that the air passing in bronchi dilated to a large size, or into cavities, will give a hollower, a more blowing sound, than in those of the natural dimensions; hence, over them the sound of respiration may be bronchial, tracheal, or cavernous, in regions where it is naturally purely vesicular; and if, as it frequently happens, there be liquid in the tubes, the bubbling into which it is thrown will be heard to be coarse and gurgling, instead of the finer mucous rhonchus of common bronchitis. So, also, the voice may be powerfully transmitted through these enlarged tubes; not in a diffused *trémulo*, as usual, but loud and startling, as if issuing from the spot; in some cases cracked and jarring, as in bronchophony; in others more articulate, and with a snuffling and hollow sound, as in pectoriloquy. But these are also the signs of tubercles and excavations in consumption; and we must seek for further distinctions. Besides, in the history of the case, and the character of the constitution, these distinctions are sometimes to be found;—in the situation of these sounds, which, in phthisis, is usually in the superior parts, but in dilated bronchi in the middle regions of the chest; in their character in relation to time, those in phthisis tending to increase and spread as the excavations proceed, whilst those of dilated bronchi remain nearly stationary for weeks

and months; in there being less change in the shape of the chest with dilated bronchi than with phthisis, unless they have arisen from pleuro-pneumonia, in which case the change is different; and, finally, in the nature of the sound on percussion, which, in phthisis, is more extensively dull, especially under the clavicles, whereas, in dilated bronchi, if any dullness exist, it is generally in the mammary, lateral, or scapular regions of the chest, and is often accompanied by a sound of a peculiar kind. This is a hollow tube-like sound, and from its resemblance to that produced by mediate percussion on the trachea, or by tapping with the finger on the mouth of a small phial, I have given it the name of *tracheal* or *amphoric*. I can give you a notion of the kind of sound, by filipping on a finger pressed on the larynx or trachea, or on the cheek when the mouth is opened in the manner of sounding the letter O, thus —. This sound depends, not essentially on the vibration of the walls, as in the case of the ordinary sounds of striking the chest, but on that of the air in the tubes or cavities, which give a note according to their length and size, precisely in the manner of a pan-pipe, or of this India-rubber bottle. In the natural condition of the chest you do not obtain this sound, because the stroke of percussion, and the resonance of the large tubes, are intercepted by the ill-conducting tissue of the lung; but when the bronchi are dilated so as to reach nearly to the surface, or, as we shall hereafter see, more perfectly when the large tubes are brought in contact with the walls of the chest by the pressure of a liquid effusion; or when, by perfect hepatization, the stroke and the resonance can be transmitted to and from these tubes near the root of the lung, you will then get various degrees of this amphoric sound, which it is not difficult to distinguish from the duller and deeper resonance of the healthy chest. The circumstances which favour the production of this sound are the same as those which cause morbid bronchophony and bronchial respiration, and pectoriloquy and cavernous respiration; but it requires a more perfect degree of them. Hence, although somewhat of the amphoric sound is occasionally yielded by empty cavities in phthisis, this does not often occur, it being damped by the irregularity of their form and materials, and the remains of spongy tissue on their surface. The cracked-jar sound (*bruit de pot fêlé* of Laennec) is of the same class, being a slight noise of a sudden motion of air and liquid within resonant tubes or cavities, produced by the impulse of external percussion. It is more common in large phthisical cavities, because their form and size more readily

expose them to the influence of an impulse on the walls of the chest.

Finally, you will be better able to distinguish dilated bronchi from phthisical cavities, when you become fully acquainted with the signs and general symptoms of the latter; and I shall now only add, by way of recapitulation, when you meet with a case in which long continued cough, with purulent expectoration, dyspnoea, loss of flesh and strength, hectic fever, even with some of the physical signs of cavities in the lungs, beware of pronouncing it to be tubercular, if qualified by all or most of the following conditions:—If no proofs of a scrofulous habit can be traced; if the complaint have originated in a long-continued and violent cough, or in an attack of pleuro-pneumonia, and, considering its duration, emaciation have not proceeded very far; if the purulent expectoration have been fetid and sanious rather than flocculent or cascosus; if the bronchial or cavernous respiration, voice, or gurgling, be heard rather in the middle than in the upper portions of the chest, and be there spread over a considerable extent of surface; if these middle portions chiefly sound differently on percussion, being dull when the rest of that side sounds pretty well, or amphoric when the side is generally dull and contracted; and if, although the cough and expectoration continue undiminished, these signs remain stationary for many weeks together.

I have little to say on the treatment of dilatations of the bronchi. You can perceive that when once formed, they can be little under the influence of medicine. The profuseness of the secretion may sometimes be restrained by acid mixtures; and I have known the nitro-muriatic acid in two or three instances succeed in removing its factor. Probably inhalations of chlorine would be useful in such cases. Where the cough is violent or troublesome, it should be allayed as much as possible by sedatives, such as hyoscyamus, belladonna, conium, and particularly opium, or some of the preparations of morphia, due attention being at the same time paid to the state of the excreting functions, and the general condition of the system, which may need various kinds of treatment in different cases. The co-existence of chronic bronchitis often renders external counter-irritation of some service in dilatations of the bronchi; and other antiphlogistic measures are occasionally required on the supervention of intercurrent acute inflammation, which sometimes takes place. It is, however, from preventive measures that we may expect more success; and our knowledge of the causes and tendencies of this lesion suggests the expediency of not abandoning the treatment

of cases of bronchitis, pertussis, and pleuro-pneumonia, until all cough, and the physical signs, have been satisfactorily removed. Most of the severe cases of dilated bronchi that have fallen under my observation, I have traced to imperfect treatment in former inflammatory attacks, and I am fully convinced that many examples of the different structural changes of which I have been speaking in this lecture, may be prevented by an efficient plan of medication, when the complaint is considered merely as a severe cold, a cough, or "the influenza."

I need not detain you on other structural lesions of the air tubes, for they are of too rare occurrence to be of much practical importance. Cases of the bronchi seldom occur but in connexion with some cause which concentrates inflammation in the bronchial membrane in a peculiar manner, such as the habitual inhalation of irritating particles of dust, in the occupations of needle-pointers, stone-masons, and leather-dressers; the continued passage of tubercular matter in phthisis, and occasionally the specific influence of measles, small pox, and syphilis. I do not know of any signs by which the presence of ulcers in the bronchi can be distinguished; they rarely, if ever, exist without a similar affection of the larynx, in which case the voice is impaired or lost; but this happens commonly when the bronchi are not ulcerated, or only so far as to give vent to the matter of vomica in phthisis.

LECTURES ON TUMORS.

Delivered at St. George's Hospital.

By CÆSAR HAWKINS, ESQ.

II. OSSEOUS TUMORS.

1. Tumors of or upon the Bones.
2. Conversion of natural Textures into Bone.
3. New formations of Bone.
 - a. Osteo-cartilaginous Tumors.
 - b. Osteo-stomatous Tumors.
 - c. Osteo-sarcomatous Tumors.

III. SARCOMATOUS TUMORS.

1. Adipose Tumors.
 - a. Common Adipose Tumor.
 - b. Adipose Tumor with retained Cyst.
 - c. Cutaneous Adipose Tumor.
 - d. Hypertrophy of the Adipose texture.
2. Fibrous Tumors.
 - a. In fibrous texture.
 - b. In cellular texture.
 - c. Painful sub-cutaneous Fibrous Tumor.
3. Neuromatous Tumor.

II. Osseous Tumors.

As a second order of tumors, we will take those in whose structure more or less

bone forms an element, and we will call them *osseous tumors*, and the circumstances in which bone is found in unusual situations are chiefly these:—

II. 1. The most frequent situation, as you would naturally anticipate, for a bony tumor to be found, is upon the natural osseous structure; the nature of all tumors, as we have seen, being in some measure determined by the texture in which they arise. But I shall not say any thing at present upon this subject, which we will leave to be considered with the other diseases of the bones.

II. 2. Another mode in which bone is deposited, so as to constitute a tumor or otherwise to be perceived in examination, is by *ossification of the natural textures*, and chiefly of the *fibrous texture*. Here is a mass of bone in the falx cerebri, where it is not unfrequently met with as a cause of epilepsy, or of an insupportable dullness, such was the case, for instance, in the late Dr. Pemberton. Here again you see the conversion of the *fibrous covering of the spleen* below the peritoneum into a thick layer of bone, such as seen occasionally in the pericardium, beneath its serous layer, or on the surface of the liver or lung. This is a specimen of the conversion of ligament into bone in the anterior longitudinal ligament of the spine, constituting a singular kind of anchylosis between the vertebrae. It is not infrequent in our worked horses; and Sir Charles Bell relates a remarkable case, in which a man, subjected to an injury of the spine, broke across this bony splint which he happened to have, and which was, of course, less able to resist injury than the elastic structure whose place it supplied. In many persons tendons and muscular fibres are sometimes converted into bone, as, in this instance, of a portion of what should have been the tendon of the *psoas magnus*. In the late Mr. Heavyside's museum was a preparation where the femur was arched over by a great number of bony spicules, occupying the situation of the vasti muscles. These cases constitute a kind of exostosis not usually productive of much inconvenience, because the morbid growths occupy the places nearly of the muscle or tendon, so as only to shorten their point of attachment, without preventing the action of the muscles, and without interfering with other textures, and because the change is seldom produced to a great extent.

Unless we could discover the elixir vitae, and realise the dream of our ancestors, and make our patients young again, I know not how we are to check this instance, among many others, of the gradual failure of their corporeal frames. There is occasionally, however, a similar disposition to ossific deposit exhibited in early

life, which constitutes a very serious disease, in which ligaments, tendons, and muscles, are all implicated. Mr. Abernethy met with a boy in whom the tendency to ossification was so strong, that besides exostosis, the least blow produced the ossification of numerous soft parts;—in the ligamentum nuchæ, so that his head was fixed; and in the margins of the axillæ, so that his arms were pinioned to his side. In the College of Surgeons is a skeleton in which this disease is seen in a remarkable degree, the most extensive ossification of muscles having taken place, among others of the long muscles of the back down the whole spine, so that the unfortunate subject of this affection must have been a wretched cripple. It would seem that the use of acids, to prevent the deposit—of the nitro-muriatic bath, as it used to be called, and other remedies, have been tried in vain to check this peculiar condition of the system, whatever it may be.

In one of the numbers of the *MEDICAL GAZETTE* is the description of a curious example of the ossification of muscle, or formation of bone in them, said to be common among Prussian recruits, and known familiarly among them by the name of the exercise bone; though I am not aware that it is observed among our own troops. It is the formation of a bony tumor, or ossification of the fibres of the deltoid and pectoralis major muscles of the left side, where the musket rests. There takes place a small red painful spot, which, if neglected, terminates in the formation of a mass of bone, from four to seven inches long, and weighing from two drachms to an ounce; the excision of which becomes necessary, to get rid of this curious result of inflammation.

II. 3. The third genus of ossific deposit consists of those cases in which ossification takes place in *new situations*, not upon the bones, nor occupying the place of the original textures, but as an entirely *new formation* among soft parts; and you may divide them into three species, according to the kind of soft substance usually mixed with the bone.

a. *Osteo-cartilaginous Tumors*.—These are usually met with in the serous membranes, where, after a time, they lie free, like the loose cartilages of the joints, and they are formed in the same manner, no doubt. Here is a small one, which I took from the tunica vaginalis testis, and you can distinguish a solid hard nucleus of bone inclosed in a firm close cartilaginous texture, and covered by a smooth surface of serous membrane, derived from the membrane to which it was originally attached. I had a patient, not long since, with excessively irritable testis, which resisted all the usual remedies, and which I believe arose from a body of this kind, which I wished to re-

move, but could not persuade the patient to submit to it. You may see in this preparation a large irregular mass of bone, which was mixed with very little semicartilaginous matter, and which was formed in an ovarian cyst, from which several other pieces of the same material came away.

b. A second species of osseous tumor consists of a bony cyst, with matter of different kinds in its interior, frequently fatty substance, or a glairy mucilaginous fluid. It is called by some *osteosteoma*, from the former secretion. You may see this large bony cyst, which I took from the dissecting-room, so that I do not know its history, the contents of which were of this kind; it is attached to the testis, which is perfectly sound and natural at its upper end. You will understand by this situation that it is perfectly distinct from the sebaceous encysted tumor, with steatomatous contents; but I believe it is frequently only the sequel of the serous and hydatid encysted tumors, the cysts being, in these cases, changed into fibrous texture, and ultimately into bone, partly or entirely: look, for instance, at this large osseous cyst in the liver. I have not one to show you in the common cellular texture, nor are they often met with. You may see this plate of a large one on the cheek in Alibert's splendid work, which he says was unconnected at first, but became subsequently attached to the upper jaw, and required removal.

c. The third species of osseous tumors may be called *osteosarcomatous* tumors, as the name has been used by surgical writers, though it is not a very good one, and they consist of a mixture of bone with solid substance, not attached to any of the bones of the body. Here is a large mass of fibrous tumors, or fleshy tubercle, looking like a child's head, and completely filling the uterus, all the outer part of which is bone, so thick as to require a saw to divide it. They are occasionally met with in the external parts of the body, though not commonly. I recollect assisting Sir Benjamin Brodie in removing a bony tumor, mixed with soft substance, from the situation of the parotid gland, where such a structure is occasionally met with. I believe the disease returned. Mr. Abernethy saw one of considerable size in the ham. There is somewhere in the museum, but I could not find it just now, a very large tumor with bone in its interior, which was removed from the inside of the thigh, by a surgeon (not in this hospital), in which case, I believe, the patient died of hæmorrhage on the table, or immediately after the operation, from its involving the great vessels of the thigh.

I am inclined to believe that the loose unattached osteo-sarcomatous tumors, like

the tumors of the same name attached to the bones, are of two different kinds; in one of which there is bone formed on the outside of some innocent solid tumor, generally of the fibrous kind, which may be removed with safety when accessible, or a limb removed, if the dissection of the tumor is impracticable, without any fear of the disease returning; while, in the other form of tumor there is a small proportion only of osseous deposition, and this is found chiefly in loose spicular distributed through the substance of the tumor, which is of the nature of fungus hamatodes; consequently, although an operation is equally proper to be performed, it is done with much less confidence, on the part of the surgeon, that the operation will be followed by a successful result. I amputated the thigh for this large malignant disease of the knee joint, originating, as it seemed, in the ligamentum patellæ, which was of the cerebriiform kind, mixed with bone, and which had ulcerated so as to bleed profusely. The patient died a few days afterwards of copious vomiting of blood, which came not from an internal tumor, as I was inclined to suspect, but from some disease of the vessels of the stomach and bowels, distending them with blood, and causing their coats to give way.

In all cases of osseous tumors, except the malignant form, in which the soft substance predominating may obscure the diagnosis, there is no difficulty in recognizing the structure by the feeling conveyed by its solid, firm, unyielding material, which is perceptible even at a considerable depth; and there is, of course, no chance of any cure of an osseous tumor, except by its entire removal by excision or amputation.

III. SARCOMATOUS TUMORS.

The third order of tumors are the *Sarcomatous Tumors*, meaning, as was formerly explained, such new formations as are firm and solid to the touch, not consisting of a single cyst, like the encysted, nor of bone, like the osseous tumors; or, at least, if either cysts or bone are found in them, these appearances bear a very small proportion to the bulk of the solid materials of the tumor. We may adopt a division of them, from the variety they present, into eleven genera, some of which are innocent, others of a malignant character; and the principal circumstance to determine, in their investigation, is whether they are or are not malignant, in order that, in the former case, their removal may be earlier effected by the knife; for unfortunately we shall find, as we proceed, that there is scarcely any one of them which admits of being removed by

remedial means independent of an operation,

III. 1. The first genus is the adipose, or *fatty tumor—adipose sarcoma*. It is a very common kind of formation, and requires, therefore, to be fully understood by you; and let me observe, that it is sometimes called *lipoma*—a bad name, since it is employed also for other formations; and *statoma*, as in the late work of Dr. Warren—a bad name also, since it is more frequently used to designate one of the sebaceous encysted tumors. It is called, too, *cellular tumor*, as by Mr. Burns; and this is as injudicious a term as the others, since it is not formed of the cellular texture at all, but of fat in its appropriate cells. There are three species of adipose tumor; the first of which is,

a. The *common adipose tumor*; which consists of a mass of fat, rather whiter and firmer than ordinary, and of a closer texture, which is inclosed in a thin cellular membrane, separating it from the surrounding fat, and dipping into its interior, so as to divide the tumor into a great number of lobes. The membranous cyst is very loosely connected to the bed in which it lies, except at one point where a vessel or two enter it, or if long, the vessels supply it at two or three places. Its vascularity is very slight, however, so as hardly to bleed if it is cut. You saw yesterday a tumor of several ounces weight removed from the shoulder, irregularly shaped, and lobulated; and I have seen it sometimes almost like a bunch of grapes in shape. Fatty tumors are found chiefly, as you would expect, below the skin in the common adipose texture, and they are common on the back, or thighs, or chest. It is said they do not occur in the eyelids or scrotum, where there is no fat; they have, however, been met with in the scrotum, though of necessity very rarely. They sometimes attain a great size. I have seen one, removed by Sir Everard Home, weighing 26lbs., and one of 10lbs. has been removed. They arise from a blow, or other injury, sometimes; but more frequently without any evident cause. Sometimes, after growing to a certain size, they remain stationary, and undergo no further increase; and whatever size they attain, they generally produce little pain or inconvenience, except from their bulk, dragging thus upon other textures, and being, of course, very unsightly. Here is a small fatty tumor, however, which occasioned very great pain, from its having accidentally a small nerve stretched over it. Occasionally, however, when large, the skin becomes distended over them, and ulcerates, and the tumor projects, with an unhealthy

fungus growing from it, of apparently a bad character, though not really malignant. Here is a small one, ulcerated, and it shows you the yellowish-brown colour which the fat then acquires. Rarely is any inflammation established in a fatty tumor, but Sir Benjamin Brodie met with one case in which the centre suppurated, and matter formed, mixed with oily substance from the tumor.

The diagnosis of a fatty tumor is generally very easy; it is unaccompanied with pain; it is soft and elastic, with a peculiar feeling to the touch that you can seldom mistake after having once felt it, and is different from that of every other swelling, except sometimes the subcutaneous navus, the vascularity and colour of which are usually apparent, and except sometimes a small deep chronic abscess, where the matter is too thick, or too small in quantity, to be rendered sensible by fluctuation, which is very seldom the case; and such an abscess is less elastic and less moveable than the fatty tumor. Medicine has no influence on this tumor, nor have any local remedies power over it, except to do harm. You may often see the tumor after stimulant plasters or blisters have been applied; and the consequence is adhesion to the skin, making dissection necessary in the operation. Pressure also by the clothes, or by other means, induces sometimes the same adhesion, and makes the operation more difficult.

The operation is generally very easy and very safe, and union readily takes place afterwards, in most cases, where no inflammation has preceded it, and the skin has not been rendered thin by pressure and tension.

If the tumor is small and stationary, and not in an inconvenient situation, no operation is absolutely necessary; but it is generally better to do it as soon as the patient consents, as it is so much slighter an operation when small, and, if left, it may grow to a considerable size, or it may inflame, or become of a different character at some future time. The operation is best done by a simple incision through the skin, cutting through the cellular covering of the tumor into its substance; you are then certain of the boundaries of the tumor, and you can readily separate the attachments of the cyst by the fingers, so that you have nothing to cut through except the vessels at its base, and, as we have seen, even a large one will hardly have more than one or two requiring to be tied; you will then bring the skin together, and make pressure over the sides of the cavity from which the tumor has been removed, by plasters and bandages. If you try to dissect round the tumor without cutting into the cyst, you are very liable

to leave a portion behind for the tumor to be re-formed, and you will have much more hemorrhage, especially if the tumor is attached to muscles. If it is torn away, on the contrary, you are sure to remove every portion, even of the most lobulated form of tumor, from the cavities in which the several portions lie. If there is adhesion of the skin to the cyst, you are sometimes obliged to dissect off the skin, and then you can tear away the rest of the tumor; and if the tumor is very large, and the skin is thin, or ulcerated on the surface, a portion of it may be removed, by a double incision, with the tumor.

If an adipose tumor is situated under a fascia, its diagnosis is more difficult, as it loses much of its elasticity, and feels more solid, or sometimes more like an abscess; but directly an incision is made through the fascia, the tumor starts out, and can be easily torn away. If the tumor has been subject to much pressure, however, the operation becomes more difficult, from the necessity you are then under of using the knife, which is to be done with caution, in order to distinguish the fat of the tumor from that around. A gentleman with whom I am acquainted had a fatty tumor on one side of the nates, which an eminent surgeon advised his doing nothing to. He followed this advice rather longer than was intended, I presume—namely, for twenty years; at the end of which time it was very large, and could not be concealed in his clothes, and was a somewhat inconvenient cushion to sit upon; in fact, he hardly liked to stir out. He came to town while I was absent, so that I did not see him, but the tumor was removed by Sir Benjamin Brodie, who informed me that the pressure to which the tumor had been subjected had pushed it deep even into the sciatic notch, and had produced a good deal of adhesion to the important parts there situated, making the dissection both difficult and dangerous.

There is a singular situation in which the fatty tumor is sometimes found—namely, in the fat below the transversalis abdominis: one such case I saw in this hospital, removed safely by Sir B. Brodie. The tumor here makes its way through the abdominal muscles, so as to be perceptible externally, and has been called, erroneously, a fatty hernia (*Hernie graisseuse*). It requires caution, however, in the operation, as it is sometimes attached to the peritoneum, and this membrane may be dragged out and endangered, if untended to.

b. A second species of adipose tumor possesses a reflected cyst. The common fatty tumor has an entire membranous covering, but the one I am now alluding to has a loose reflected bag around the

other, and connected with it at the base, like the two layers of the pericardium. The tumor, in short, has no attachment except where the vessels enter, and the surface is smooth, instead of the rough cellular connexion which it usually possesses to the surrounding fat. Here is a small tumor, said to have been of this kind, but it is rare, and I have not seen it in the living body. I suppose, however, there is no apparent distinction between the two forms before operation, since no fluid is secreted between the attached and the reflected layers of the cyst. In fact, I should regard it as a mere accidental circumstance, such as I have sometimes seen with *navi* and other tumors which usually are attached all round. The operation must be equally necessary as in the other case; and I presume the loose cyst need not be removed, since it has nothing to do with the formation of the fat of the tumor, and is probably no more capable of reproducing the tumor than the less organized cellular membrane attached to the cyst of the common adipose tumor.

c. The third genus I call the *cutaneous adipose tumor*. In this preparation you may see a great mass, of half a pound weight, attached to the body by a very narrow pedicle, from which it hung loose on the skin, and which was cut through, leaving a surface of not more than a third of an inch for a tumor nearly four inches in diameter. It is composed of a thicker kind of fat than usual, whiter and firmer, more lardaceous in appearance, and it is covered only by a thin white skin, without any natural fat below it, and scarcely capable of being dissected off, from fibrous bands running from the cutis into it. There are sometimes a great number of these tumors attached to the skin, and connected with it; and from their whiter and more solid appearance they may be mistaken for medullary tumors of this structure, especially when, after having removed one or more of the little tumors, you perceive others rising near the cicatrix perhaps. This form of disease is not well described any where, and from the resemblance to malignant disease in the circumstances I have mentioned, you must bear it in mind. In this plate of Albert's, under the name of *kupes graisseux*, is what appears to have been a case of this form of disease, since you seldom see such an immense number of the common adipose tumors. It is not a common form of tumor, but I have known it subside under the use of liquor potassæ continued for a great length of time, which I have combined with sarsaparilla in one person of a weak constitution. Of this medicine you may give from half a drachm to a drachm three times a day. I think a little benefit

might be derived from the local application of a solution of iodine and hydrodate of potash, such as you see used in the hospital occasionally; but I have not tried it. If this medicine does not disperse the tumors, any one of them that is growing faster than the rest may easily be removed by the knife; or, if there is only one, the patient may have the choice of its removal at once by this means, and a shorter course of medicine afterwards, to lessen the chance of a return.

d. Another form of adipose growth may be called *hypertrophy* of the adipose texture, in which there is a vast increase of fat below the skin; but the fat has no boundary, no cyst, as in the former genera. Sometimes there is only a general growth of fat in all parts of the body, as in this plate of a child, making the unfortunate object of it quite unwieldy, and seriously incommode him; but this is only common obesity in a remarkable degree. At another time, however, the fat of a particular part of the body only increases in this manner, and this is generally of the chin, where you may sometimes see an enormous bag of fat, as large as the head, which might well be called a tumor, though not quite in accordance with the definition before given. Sometimes, instead of being on the chin, you see a great mass hanging on the front of the abdomen, or some other part of the body. In a man now in the hospital under my care, many of you have seen a remarkable specimen of this tendency to adipose hypertrophy; at least, I believe it to be this form of complaint rather than the first genus. This man has two masses of fat on the back of the neck, each as large almost as your double fist, the division having been, in fact, made by the pressure of his neckcloth. Another great mass hangs below his chin; he has another over each deltoid muscle; and on the abdomen is a large quantity, divided into the appearance of two tumors by his waistband; besides which he is tolerably fat every where, so as to form altogether a remarkable figure.

Such tumors as these do not admit of removal by the knife, since there is no distinction between the increased mass and the rest of the fat; but, as it is a constitutional disease, it is capable of being remedied by medical treatment, and the masses of fat disappear under the use of liquor potassæ, which you should gradually increase till the patient takes a drachm and a half, or two drachms, three times daily in milk or beer. My patient has taken it now nearly three months, and the tumors, though not obviously lessened, are considerably less full, and the lobules are more distinct where the cellular tex-

ture divides the separate portions; but it requires to be continued for nearly a year, I believe; and from his having epilepsy and rheumatism, the medicine has hardly yet had fair play. I recollect, when a pupil, a servant who was in attendance at the hospital for a great mass of this description under the chin, which was so unsightly as to prevent the man from gaining his livelihood; this man took the potash for a twelvemonth, I believe, and the fat at last disappeared, leaving an empty loose bag of skin; and I mention the case, because Sir Benjamin Brodie, whose patient he was, informs me that he was accosted ten or eleven years afterwards by a servant, in a house which he was visiting, who proved to have been this individual, and who had remained quite well since, the redundant skin having, of course, contracted. Wherever, then, you have several masses of fat of this kind, try this medicine in the large doses I have mentioned, for a considerable time. How it acts I don't know, unless it forms a kind of soap in the body. It generally produces some degree of universal emaciation after a time, by its effects probably on the stomach; but the effect is evident in a much greater proportion upon the tumors than upon the rest of the body; and this general thinness is recovered from, as it would seem, without reproduction of the fat. Sir Benjamin Brodie once tried iodine in this kind of disease; but although it caused the usual emaciation which that medicine creates, it did not influence the local deposit of fat, which increased notwithstanding the general falling away.

III. 2. The second genus of solid tumors is the *fibrous tumors*, which may be subdivided into three species.

a. One form of fibrous tumor is connected with *fibrous textures*, such as fascia, ligament, dura mater, and the contractile fibrous tissue of the uterus. A common situation is on the fascia, under the skin, in the arms or legs. Here is one which I removed from over the annular ligament of the instep. I have seen three or four, in the same person, on both fore-arms; I have seen it also in the thigh, arising from the fascia lata, and on the back of the hand.

Examine the tumor, and you find it from any of these situations hard, whitish, semi-cartilaginous; the knife cuts it with difficulty, like a cancerous tumor; and it cures, as the French call it, when cut; it is, in fact, just like a mass of firm condensed ligament, in a circumscribed and generally a circular form. To distinguish it below the skin, you will feel it hard and firm, nearly unattended with pain;

moveable, except at the point of attachment, which is easily perceived; possessing little elasticity, smooth on the surface, and the skin generally moving freely upon it.

When increasing, the tumor may easily be removed by the knife; but you must be very careful to remove not only the tumor itself, but the whole of the fascial or ligamentous structure to which it is attached, otherwise the disease will return. A patient came under Mr. Babington's care for a tumor of considerable size, apparently of this kind, connected with the upper and outer part of the thigh, which had already been twice removed by operation, and which, I believe, was perfectly innocent in its character, and thus returned from want of attention to this circumstance in the former operations. There is often no necessity, however, for operation, if the patient is unwilling to have it performed, as the tumor is very slow in its progress; for instance, a man came under my care for some other cause, in whom, over the edge of each radius, a tumor of this kind existed; but although seven or eight years had elapsed since their first formation, they were not so large as a walnut, and he had no inconvenience from them, and would not, therefore, have them excised.

Fibrous tumors of the *dura mater* of the spine, or brain, are somewhat softer, and more yellow in texture, and produce of course the symptoms of pressure, but very slowly. I have seen the spinal marrow compressed to half its natural diameter. It is unnecessary to dwell upon them, however, from their incurable nature.

The *fibrous tumor of the uterus*, or *fleshy tubercle* of this organ, as it is also denominated, is very common, and is generally, like the structure of the uterus itself, a little condensed; it is consequently considerably less ligamentous than the former varieties. They grow in this situation in any direction; here is a section of one which I found filling the uterus, which was thinly expanded over it; it was nearly as large as a child's head, and the surface is a mass of firm bone, which required the saw to divide it. You may see them projecting into the pelvis and abdomen, covered by peritoneum. I once saw one larger than any one's head, which had been removed by operation. The operator who shewed it me very naturally expected to find a diseased ovary in a moveable tumor of this magnitude, and the patient as naturally died of peritonitis. There is a long paper by Dupuytren, on the Fibrous Tumor of the Uterus, and a complete account of them by Dr. Lee, in the 19th volume of the *Medico-Chirurgical Transactions*.

When situated towards the interior of

the uterus, they project into the cavity of that organ, and into the vagina; and when accessible, they may be removed by the double canula, or by excision, but not so easily as the ordinary polypus. The operation is not, however, devoid of risk; for instance, in the present year I had a woman under my care, with a tumor of the size of the fist in the vagina, with a neck an inch and a half wide. I tried to bring it down so as to excise it, but could not do so without more force than I liked to employ, and therefore tied it with a ligature. The mass separated by ulceration; but when the patient was apparently getting well, a low inflammation spread to the uterus, and to the peritoneum, and destroyed her. I found on examination, that besides the mass which I had removed, and which was attached by its neck to one side of the cervix uteri, another, which had met with less pressure and resistance in that direction, had enlarged to the size of the fist within the cavity of the uterus. They are fatal by irritation and hæmorrhage, in some cases, without operation. I attended a woman with Mr. Malton lately, and removed a polypus from the cervix uteri by excision, but was obliged to leave a fibrous tumor distending the cavity of the uterus, which proved fatal a few months afterwards. When of very large size, a change takes place occasionally in the interior, and the tumor becomes very vascular, and ulcerates, so as to look like a malignant tumor, as in this drawing from a patient of Dr. Seymour's, who died in the hospital, with this immense tumor hanging down in the vagina, and connected with the fundus of the uterus.

b. A second form of fibrous tumor seems to originate in the cellular texture: it is somewhat similar to the last, but much softer, and more elastic, the fibres softer and less distinct, and not so white and shining; and it contains some fluid, which exudes on a section being made, and thus leaves the tough fibrous cellular membrane, which composes the solid part, to a certain degree flaccid. It is smooth and elastic in the living body, little painful, and grows to a considerable size sometimes. The most frequent situation in which it is met with seems to be in the cellular texture, by the side of the vagina, perhaps from the stronger and more fibrous nature of the cellular membrane in this situation. Here is one of several inches length, which I saw removed by Sir Benjamin Brodie from this situation, which it was difficult to extract entire, from the great depth to which the end dipped into the pelvic cavity. This circumstance makes the operation difficult, and requires care in the dissection; but its nature seems

to be innocent, so that it does not return if the whole tumor be extracted from its cellular bed. A very remarkable case has been published by Mr. Lawrence, of this disease, which he calls a cellular tumor; but as it contains no cells, and is tolerably firm in structure, I think the name fibrous tumor of the cellular membrane less likely to be misunderstood. In this case the tumor had grown for four years, was thirty-two inches in circumference, and was as large as a double head; it returned after the first operation, from the end being cut across; and a second operation being performed two years after the first, this end was dragged out of its deep situation, and the disease did not return again.

This tumor must not be confounded with a peculiar growth of the labia and nymphæ, which will be described to you by and by, a kind of hypertrophy only of the cellular membrane, which becomes distended to a great size; this is a disease of the same kind as that which is called the Barbadoes leg. The fibrous tumor has, on the contrary, no connexion with the skin, and is hard and firm.

c. The third species of tumor in this genus may be called the *painful subcutaneous fibrous tumor*, or, as it was called originally by some English authors, *painful subcutaneous tubercle*. The best description, however, of the disease, is by Dupuytren, who remarks that it has been erroneously called a nervous tumor, and who calls it himself a fibro-cellular tumor. It contains no cells, however, as this name would seem to imply, and is really a fibrous tumor, with a singular painful sensation.

It is observed as a little hard lump below the skin, the size of a grain of wheat, perhaps, and never more than as large as a pea or small bean; you have seen me lately remove one of seven years' growth, which was even then not half as large as a small pea; I could not find it this evening to show it you, though it is somewhere in the museum. Its texture is firm and fibrous, sometimes almost like cartilage in hardness, whitish, or brownish white, and with a rather firm covering. It is exquisitely tender, the least touch exciting violent pain; even the passage of cold air over it, or the friction of the clothes, may produce complete torture. The pain is sudden, darting, like an electric shock; sometimes so severe and frequent, as to induce convulsions, and become quite wearing and injurious to the general health. I amputated the leg of a man this year, who had one near the tubercle of the tibia, and the pressure of the wooden leg after the operation obliged me to remove the little tumor, from the pain which was produced by it, and which seemed to extend over the whole side. Sir Benjamin

Brodie not long since removed one from the cheek, in which situation they are not uncommon, though they may be met with any where. You can scarcely account for the production of such violent pain in these little tumors; occasionally, indeed, you can trace some nervous twig into them, or over them, but very often you will look in vain for any distinct fibre of nerve. Dupuytren attributes the pain to the density of the kind of capsule which surrounds the tumor; but this is by no means always met with; in the one I have just alluded to, the capsule was rather delicate, and there was a reflected cyst besides, which, like a bursa, must have still further defended the tumor from friction; in this case, by the by, the pain was only occasioned by pressure in one direction, which dragged probably on a nerve.

The skin is generally loose and moveable, but is sometimes attached to the tumor. It may occur from falls, or blows, or other injuries, but often takes place without any evident cause. Dupuytren says that women are more subject to it than men: it so happens, as to myself, that I have only seen it in the male sex; but yet he may be right. There is scarcely ever more than one in the same person. The only disease for which it can well be mistaken is the donlounrenx, the pain being similar in the two cases; but the existence of the little tumor, and the production of pain by pressure, easily serve as diagnostic marks between them.

Narcotics and various other remedies have often been tried for a great length of time, without any avail, and the removal of the little tumor by the knife is the best remedy, which is also generally easily and safely done, though some of you have seen extensive abscess follow the operation in the recent case of my own, which I have alluded to before. You may make an incision through the skin over it, and catch hold of it with a hook to cut it out. Dupuytren gives an additional reason for the operation; viz., the tendency of the tumor to become cancerous, and end in carcinomatous ulceration. I think he is probably not correct, however, as to their having any natural tendency of this kind; all tumors no doubt may become changed into cancer, but certainly very many years will elapse without any alteration. The operation is, however, right, for the pain produced by the tumor, whether Dupuytren be correct or not as to its probable termination.

III. 3. The third genus of sarcomatous tumor is really a tumor of a nerve, or rather in the coverings of a nerve, and you may call it the *neuromatous tumor*. It is not common, however,

a. Sir Everard Home has given a very good description of one that he removed in this hospital from the axillary plexus. The tumor was situated in the texture of the nerve, between its fibres, and was of a tolerably firm consistence, with a mixture of fluid of a white or light brown colour, and the nerve was flattened into two portions by the separation of its fibres. It was firm in its feeling; and after growing slowly for several years, it had increased more rapidly. It could be moved laterally without pain; but if dragged upon, violent pain was produced in the course of the nerve. A small cord could be felt at each end of the tumor, which was the continuation of the nerve, and the neurilema of the nerve formed the covering of the tumor. Dr. Gills, who was house-surgeon here, I believe, and who paid us a visit not long since from St. Petersburg, where he resides, removed another from the muscular spinal nerve, the fibres of which were more reticulated and expanded, and the fluid in the centre was dark coloured, like blood. In the removal, a large artery in the centre bled freely. The return of sensation and motion took place here slowly; but you must be prepared, if such a case presented itself to you, for the patient suffering great inconvenience for a long time, from want of nervous power, and should not be in haste to operate.

The situation of the tumor, its connexion with a nerve above and below, the peculiar pain and nervous symptoms in the parts supplied by the nerve, will form grounds for an accurate diagnosis; unless, as I have seen, a tumor involves a nerve in its structure, in which case it may erroneously appear to have originated in the nerve.

I apprehend such tumors are of the same kind as what you may see in this preparation, and which frequently thus form in stumps, and occasionally require a second amputation, from the severity of the pain; or such as you may examine in this preparation formed in the median nerve after an apparent injury which divided the nerve into two portions. The tumor is on the upper end of the nerve, and when recent was two inches long and one broad. In this case, too, the power of the part below seems to have been restored by anastomosing branches between the muscular spinal nerve and the lower end of the median.

Such tumors do not appear to result from common inflammation, which only thickens and condenses the cellular texture and neurilema of the nerve, but they seem rather to arise from some peculiar action, by which a fluid is effused between the fibres of the nerve, with a cellular covering. And judging from what

is seen after amputation, there is probably a peculiar condition of the nervous system, that would make one indisposed to meddle with the tumor, if it can be avoided. The loss of motion, sensation, and vital properties, is of serious consequence; and in one case of tumor of a nerve, in which Sir Everard Home operated, the patient died on the fifth day afterwards, with violent nervous excitement, after the small nerve had thus been cut through in which it was situated.

b. The case, however, which I have just alluded to, was a tumor of a different kind from what I have been describing. It was one in which he found that the tumor was an encysted tumor, which could be turned out of its bed in the nerve, leaving the fibres uninvolved. And it would appear from preparations which I have seen in the College of Surgeons, and elsewhere, that a variety of tumors may possibly be met with of a fibrous or other character, merely situated in a nerve, and not deriving any peculiarity from that locality, and which probably cannot be distinguished from the neuromatous tumor till a dissection of the parts is made. But I can say nothing on this subject from my own experience.

MEDICAL GAZETTE.

Saturday, March 10, 1838.

“*Flect omnibus, flect etiam nihil, dignitatem Artis Medicæ tueri; potestas modo venendi in publicum sit, dicendi periculum non recuso.*”

CICERO.

ILLUSTRATIONS OF MEDICAL COMPETITION.

WE were somewhat struck, not long ago, by a paragraph which appeared in a daily newspaper*, with the quaint heading *Her Merchants are Princes*. The body of the article was in a strange and facetious contradiction with the title; for while the latter alluded to the princely liberality for which the merchants of London have long been famous, the former betrayed the astounding fact that it is a usual thing for a surgeon on board a merchant vessel to receive the same pay as a common sailor, or even less. Accustomed, as we have

long been, to contemplate the various phases of medical life, we yet could not help being staggered by this announcement. The shrewd practitioner, skilled in the twofold art of curing diseases by drugs and by manual operation, or, to adopt the language of the ship-advertisements, “an experienced surgeon,” his head full of knowledge, his chest of books and instruments—placed side by side with Jack Tar, and rewarded on the same scale by the merchant-princes, seemed at first a contrast too strange to be true. Yet, after all, except in the circumstances of the juxtaposition of persons, the table of precedence being inverted, and the surgeon (in the pence-table at any rate) coming after the mate and the cook, there is, unfortunately, nothing very new in the case. The extreme tonuity of the pay may easily be matched or exceeded in our profession. The Lancashire surgeon, for instance, who attends 1,173 factory operatives for six guineas a year*, is even worse paid than the surgeon-sailor on board the *Nancy*, or the *Susan*. There is, indeed, some slight nominal difference between the cases. The surgeon confined on board the whaler, or emigration ship, has his modicum of indurated pork, or granitic beef, besides his lodgings gratis, in what Dr. Johnson defined as “a prison with the chance of being drowned.” The factory doctor, on the other hand, has neither board nor hammock, but, in compensation, may take private practice—if he can get it. Yet, in the main, both situations are very similar; for, in both, large services are expected for an infinitesimal remuneration, and in each case this is submitted to, under the vain delusion that the painful drudgery which is but nominally rewarded, is a prologue to better things.

The more we see of the disappointed expectations of medical men, on the one

* The Globe of February 10th, 1838.

* MED. GAZ. vol. xx p. 243.

hand, and the popular notions concerning medical profits on the other, the more we are convinced that an honest and candid writer might do the community some service, who should set forth the real state of the matter, and thus diminish the number of the deluded. Such a philanthropist, however, must be satisfied with effecting good, and should not be disheartened if he finds that he meets with much obloquy, and little gratitude. It is a thankless office to endeavour to persuade children that tinsel is not gold.

If we aspired to so unpopular an office as that of censor and adviser, we should address ourselves to parents; to those especially who had but little capital to bestow on the education of their children, and none to set them afloat with when their education was ended. We should ask them to look at that division of the newspapers which, above any other, shows "the very age and body of the time, its form and pressure," (we mean the advertisements); and to ascertain what kind of offers are made to and by medical assistants. Let them inquire among their professional friends for additional information as to the salaries doled out to medical assistants. Will they find it a common thing for these useful ministers of a liberal art to receive 200*l.*, or 150*l.*, or 100*l.* per annum? On the contrary, they will learn, that in the medical market such rewards would be laughed at, as ridiculous and exorbitant; the real wages being 40*l.*, or 30*l.*, or 20*l.*, or even nothing at all per annum! Yes, nothing at all; as, take for example, the following advertisement: "Medical. — Wanted immediately, an humble young lad, to take charge of a small retail at the West End. He must be competent to read and make up prescriptions. A comfortable home, and advantages for study, and, should his conduct entitle him, liberty to attend a lecture or two

daily, at the adjacent school, would be considered equivalent to salary. Good references will be necessary *."

Surely the advertiser need not so specially have demanded "an humble" assistant: even if the very taking such a situation as the one described were not in itself an act of the most saint-like humility, assuredly the place would soon humble the lad to any desirable point below the zero of pride.

How strange that any one should wilfully expend capital in bringing up his son to such a lot as this! Is it not as clear as day, that many situations which are nominally lower on the social scale, are infinitely above it in salary and comfort? Does any one offer a butler a salary of £0 0*s.* 0*d.* a year, and desire, besides, that he should be "humble?" Is he ever made to sleep under a counter?

Or, to drop the topic of assistants, and resume the question as it regards those who are practising independently, let us turn to a controversy which has been raging in the columns of a contemporary. Mr. Bree, a surgeon, of Stowmarket, in Suffolk, writes to complain of the Rev. Mr. Steggall, of that neighbourhood, who has two strings to his bow, and practises physie as well as divinity. He asserts that the charges of his clerical rival, particularly in midwifery, are rather less than half of those of the regular practitioners in the neighbourhood; and that, in consequence of this, a representation was made to the solicitor of the Apothecaries' Company. He replied, however, that Mr. Steggall claimed exemption from the act, having been in practice before 1815: if the medical remonstrants could bring evidence to disprove this, the Company would prosecute; if not, Mr. Steggall might continue to be an apothecary. As it did

* *Times*, Feb. 14, 1838.

not seem easy, or even possible, to obtain the negative evidence required, the undersold doctors next betook themselves to Mr. Steggall's spiritual superior, the Bishop of Ely; but here, again, they were destined to disappointment, as the right reverend Prelate replied that he could not interfere, as no complaint was made of the reverend *Doctor* having neglected his clerical duties.

But it is plain that he must sometimes neglect his medical, chirurgical, or obstetrical duties. With us ordinary medical men, a professional engagement is paramount to every other consideration; the messenger finds the *Doctor* at church, and off they go together. But with Mr. Steggall the case must be quite different,—as, for example, it is Sunday morning; the bell has just done ringing for service, and Mr. Steggall is walking up to the reading-desk, in his canonicals, when a horseman is seen coming up to the church-door at a fearful pace. He has galloped all the way from Rattlesden, where Mr. Steggall practises medically, to Ashfield, where he practises spiritually—a distance of no less than seven miles, according to Mr. Bree, though only five according to his reverend rival. Be this as it may, he has come at a spanking pace, as his reeking horse full well displays, and has hardly breath enough to say that Mr. Steggall is wanted on the instant. A lady in an interesting situation, at Rattlesden, was seized with premonitory pains early in the morning, and at half-past ten precisely was crying out, *Juno Lucina, fer opem*, with great vivacity. Never was clerk or doctor in a more perplexing quandary: if he leaves the church, he will be hauled up before the Bishop; if he clings to the desk, the ladies of Rattlesden will begin to suspect that half-price midwifery is not an unmingled good. He cannot expect any aid from the remonstrants, and is lost unless he has some "humble" lad

in his service ready to take his place on such occasions.

In the same letter Mr. Bree complains of prescribing chemists, and of an "impudent quack" who visits his patients, taking medicines to them, and fees from them. The writer also wishes that some member of the House of Commons would move for a return of the number of the prosecutions instituted by the Company since 1815. The fact is, that this return is to be found annexed to the evidence given by the Worshipful Society before Mr. Warburton's Committee*.

So far Mr. Bree is at least always intelligible. But he goes far out of our reach, when, in speaking of the act of 1815, he says, "Let the world see, that instead of providing for the *safety* of the public, the only effect of that act has been to establish a monopoly alike degrading to the character of the profession, and ineffective in protecting its interests." A monopoly! Why, all the facts he brings forward (to which every one could add something similar from his own experience), go to prove the existence of the most ruinous competition—that is, of a state of things the very antipodes of monopoly. The grinding monopolist sells at his own price, and has no competitor to dread; while the apothecaries of Rattlesden sell physic and advice at a painfully low rate, and then find themselves undersold by their clerical antagonist. Monopoly, indeed! Why, Mrs. Malaprop never used a word more unseasonably.

Mr. Steggall, in his answer, gives Mr. Bree at least as good as he brought. He observes "that the accusation of low charges comes with peculiar ill grace from one who bled and drew teeth at Bidleston, at sixpence a head—who wanted to establish the same charges in

* The reader who does not know it already, may learn from this paper that law is one of the most expensive of luxuries; many of the defendants, too, were not worth powder and shot, being unable to pay costs and damages.

his penny club—and who first introduced half-guinea midwifery into Stow-market." He gives another instance of overwrought competition (or, as Mr. Bree would call it, monopoly), in the person of a clerical magistrate, who is a physician to boot.

On the whole, we think that our clerical competitor comes out of the contest with flying colours: he seems to be perfectly entitled to go on as he began; and the efforts which have been made to dislodge him from his Janus-like position have but served to fix him there more firmly:—

"As the loud blast that rends the skies,
Serves but to root thy native oak!"

So far the present controversy has gone; but as to the rest, the ladies of Rattlesdun must take care how their hour of tribulation comes to clash with the hour of the church-service at Ashfield.

DR. MARSHALL HALL AND MR. NEWPORT.

WE have received the following letter from Dr. Marshall Hall:—

To the Editor of the Medical Gazette.

SIR,

As there is a "decided misrepresentation" in your remarks respecting my conduct towards Mr. Newport, I beg you to do me the justice to correct it, by the insertion of the few following lines, in your next number:—

It seems that I *thrice* responded to a request on the part of Mr. Newport to lend him 5*l*. I had forgotten the precise sum. I only remembered my willingness, nay my *desire*, to serve him. Was not this pretty well towards an *utter stranger*? towards a stranger who had no claim whatever upon me, except that created by his wants?

But this was not all. For I next *devised* a plan for his assistance, of a more extensive kind. I procured him a pupil (although he was but a pupil himself,) and on that gentleman's account I paid him *ten times* the sum of 5*l*. 8*s*. Was all this, viz. 15*l*. + 56*l*. 4*s*. such trifling assistance to a stranger, that I need be ashamed of it?

Nor would this have been all; for Mr. Newport himself abruptly terminated this arrangement by his own disgraceful conduct,—prying into his pupil's private letters, and addressing him with vulgar oaths. Can you be surprised that a parent removed a son under such circumstances, or that he even refused to pay the last monthly sum, partly due?

I will only add, that what I did I would do again, under similar circumstances. More I could not do.

I am, sir,
Your obedient servant,
MARSHALL HALL.

14, Manchester Square,
March 7, 1838.

On the preceding epistle we have to remark, in the first place, that when a correspondent accuses us of "decided misrepresentation," it is to be expected that he should take the trouble to state in what such misrepresentation consisted. But so far is this from being the case, that he does not even give us a clue to it; and on re-perusing our observations of last week we can discover nothing which is not fully borne out by Dr. M. Hall's present communication.

In our last number we called attention to two letters recently published, one having the signature of Dr. Grant, of University College, and the other of Dr. Marshall Hall, in which these gentlemen heaped upon Mr. Newport such a collection of epithets as shewed that they were tolerably versed in other terms besides those of science. The impression made upon our minds by the perusal of these documents was greatly in favour of the party whom they attacked—an impression which, it will be in the recollection of our readers, we did not hesitate to express. Since that time, however, we have seen a reply from the pen of Mr. Newport; and we must say, that unless Dr. Grant and Dr. M. Hall disprove his allegations, they have placed themselves in a predicament by no means to be envied.

Mr. Newport, the temperate and dignified tone of whose letter presents a

strong contrast to the vituperation and abuse of his opponents, charges both of them with untruth. Dr. Grant and Dr. M. Hall have mixed up private affairs with a question of science, and endeavoured to throw discredit on Mr. Newport as a physiologist, by referring to certain favours conferred on that gentleman at a time when he was not in circumstances to carry on his studies without some assistance. We never said that Dr. Marshall Hall "need be ashamed" of having rendered such aid; what we complained of was his playing the braggart, and bringing forward his own liberal loan of 5*l.* at three several times, as bearing upon a question in physiology.

But as Dr. Marshall Hall has thought fit to prolong the discussion, instead of adopting the more prudent course of letting it drop, it is due to Mr. Newport to let his side of the question be heard. Speaking of this gentleman, the Dr. says, "I fed, and clothed, and housed, this same individual! yes, I lent him money, I procured him a home in his distress for many months."

To this, Mr. Newport makes the following reply:—

"I shall reply to the charges of Dr. Hall *seriatim*. As to his having either housed or clothed me, the assertion is false, and Dr. Hall knows it to be so; I never slept in his house, or in any house of which the rent was paid by his money. He never procured clothes of any description for me. As to his having fed me, here I plead guilty, in so far as that I occasionally dined at Dr. Hall's house, by special invitations, and also took tea there, when engaged with him in prosecuting anatomical and physiological inquiries. Could I for a moment have believed that when I was accepting Dr. Hall's pressing invitations I was regarded by him as being fed like a wild beast, and destined afterwards to have this published to the world as an obligation? To the other charges, that I borrowed money of Dr. Hall, I grieve to say I must plead guilty. On three several occasions I solicited Dr. Hall to advance me 5*l.*, and he did so; for the two last sums I gave him a written receipt, and so soon

as it was convenient for me to discharge this debt of 15*l.* I did so. This is every farthing of money I ever received from him. If the payment has been sought late, like Shylock's, it has been, like Shylock's, claimed from near the heart. It ought to be known that, long before the period of this loan, it was offered to me, without solicitation, by Dr. Hall, and that before I asked for it I had spent much time in assisting him in his experiments. The statement of Dr. Hall, that he had procured me a house for many months "*in my distress*," is equally incorrect with the rest; and the designing statement, that the engagement with my pupil was abruptly terminated by my prying into his letters, &c., is equally untrue, since it was broken off in consequence of certain circumstances which I need not be the party to state. "On referring," says Dr. Hall, "to my account with Sir Claude Scott, Bart., and Co.," &c.;—had not this statement appeared in print, and with Dr. Hall's signature attached to it, it is impossible to conceive that it could have been made by him. Of course this statement must convey, and must be intended to convey to the reader, the impression that during the long period mentioned I was supplied by Dr. Hall's bounty; and yet what was the real fact? I received not one farthing of his own money during the period mentioned. The monthly payments to which Dr. Hall alludes were due to me on account of the gentleman who was living with me. It is true that it was through his recommendation that I received this pupil, but it is equally untrue, I believe, that any portion of the money came from the pocket of Dr. Hall. The terms on which I received this pupil were arranged with Dr. Hall, and the father of the gentleman in question, when it was then determined that I should receive the payments monthly, through the medium of Dr. Hall, on account of his friend, and he gave me cheques accordingly on Sir Claude Scott's bank. The obligation, then, which I lie under to Dr. Hall on this score, which appears, as he has stated it, so vast, amounts to this, that I owe to him the benefit of having had a pupil. Surely there never was such a monstrous practical sophistry attempted to be maintained, and hardly ever, I think, was

* This is the only part of Mr. Newport's answer which appears to us incomplete.—*Ed. Gaz.*

there put on record charges more unjust, or more insidious, than those which I have now exposed."

The next time that Dr. Marshall Hall trumpets forth his patronage, we recommend to Mr. Newport to content himself with the duty—

"To John I owed some obligation,
But John thought fit
To publish it to all the nation;
Sure John and I are more than quit."

IRISH COLLEGE OF SURGEONS.

To the Editor of the Medical Gazette.

SIR,

PERMIT me to correct two errors into which you have been led in your editorial remarks upon the "controversy," not "regarding the Irish College of Surgeons," as you have expressed it, but regarding the change proposed to be made in its character and functions. You state that a letter has been addressed to your publishers "by authority of the College of Surgeons in Dublin," to "require" that the name of the writer of the article in your number of February 10th, should be given up. Sir, I know not by what authority that letter was written; but I do assure you, on the authority of the Registrar of the College, that the College did not give authority for it; at least, that there is no record of such in the College minutes. And, sir, it is a most inexcusable proceeding, that any member or number of members shall venture thus to involve the name and authority of the College, and, at the same time, to cast an unfounded and indirect imputation of College displeasure upon any individual. You have also stated that I have entered the lists "against the Irish College." Allow me to assure you, that you have altogether mistaken, as well my intentions, as my conduct. I have not entered the lists against the College, but against that party in the College which seeks to subvert the original intention and constitution of the College. I am a member of it, as warmly attached to its true honour and dignity as any other. I contend for the preservation of the College, such as it was originally founded—such as it has ever since continued—and such as it still is. I am, therefore, the advocate of the College—its defender—and I have en-

tered the lists against those who would revolutionize it; but not against it.

Allow me now to protect myself against another misconception on the part of the profession. I am an advocate for the possession by every physician and surgeon of a right to give medicine if they please; but not to deal in, or to sell it.

I remain,

Your obedient servant,

BENJAMIN ALCOCK,

M.B. M.R.C.S.I.

Dublin, March 6.

[With all due deference to Dr. Alcock, we really do not see that we have fallen into any error. 1. A controversy regarding "the change proposed to be made in its character and functions," is surely still a controversy "regarding the Irish College of Surgeons," the character and functions of which it is proposed to change. 2. The letter which we alluded to is dated "Royal College of Surgeons, Dublin," and signed "C. O'KEEFE, Registrar," and this, we submit, was sufficient ground for our asserting that it was written by authority of the College. 3. In stating that Dr. Alcock had entered the lists "against the Irish College," we meant no reproach, as indeed the context shews, but simply that he was contending against certain measures which the ruling body in the College contemplated adopting.—ED. GAZ.]

WESTMINSTER HOSPITAL.

CLINICAL REMARKS BY MESSRS. WHITE
AND GUTHRIE,

*On Compound Fractures, Gun-shot Wounds,
and Necrosis.*

JOHN DACE, æt. 62, a hardy old man, with a frame (to use a Cockney simile) as tough as pin-wire, was admitted, under the care of Mr. White, December 27, 1837, having a compound fracture of the leg. Being employed at a floor-cloth manufactory, he fell from the top of a frame to the ground, a distance of six feet, and his leg came in contact with a piece of timber, which occasioned the fracture. When brought to the hospital, the patient had a wound two inches above the internal malleolus, extending from the spine of the tibia backwards to the fibula. The broken ends of the tibia, which were very uneven, had become entangled with the linen with which the leg had been rolled. Upon putting his finger into the wound, Mr. Dasent dis-

covered several loose pieces of bone. The fibula also was broken, as was evidenced by the distortion exhibited.

Such a severe injury, in so old a subject, indicated an unfavourable prognosis, and Mr. White was therefore sent for. On his arrival, he removed the entangled linen from the bony spicula, by extending the foot. A long spike of bone, projecting from the upper portion of the tibia, was sawn off. It was now ascertained that the fracture was not only compound and comminuted, but also that it extended into the joint.

Notwithstanding this complication of mischief, Mr. White determined to give the man a chance of saving his limb. Several of the fragments of bone were removed, after which the pulsations of the posterior tibial artery could be distinctly felt. The edges of the wound were brought together, and retained by means of ligatures. The leg was placed on Amesbury's apparatus, and very slightly bent. A piece of lint pledget was placed over the wound, and cold lotion was applied over the entire limb: a cup of tea, with brandy in it, was given to him, and when he was left he said he felt comfortable.

R Hydrarg. Chlorid gr. iv.; Extr. Col. Comp. gr. vi. ft. pul. ij. hora somni sumenda. Habcat haustum Senne sumenda.

4 p.m.—His pulse beating full and quick, he is ordered the following medicine:—

R Liq. Ammon. Aect. ℥ij.; Vin. Antim. Pot. Tart. ℥ij.; Mist. Camphora. ℥viij. M. Cap. ℥j. itis horis.

28th.—He has passed a comfortable night, and his pulse is quiet; his bowels have been opened.

2 p.m.—His pulse quick, 120, and not compressible; he is extremely restless. The skin is hot and dry. V. S. ad ⅔.

10 p.m.—The patient's pulse frequent, but a little softer; his skin is pungently hot, and he is restless.

29th, 2 a.m.—The nurse called Mr. Dasent up, stating that the man was dying. This gentleman (the house surgeon) found him "throwing himself about the bed," breathing hard, with his teeth clenched, and passing his faeces and urine in bed. He was insensible of what occurred. Mr. Dasent, with some difficulty, got some brandy down his throat. After some time he became tranquil.

9 a.m.—The patient is sleepy; his pulse is weak; the skin moist. He is ordered to continue the brandy.

12 m.—The wound presents a sloughy aspect. The integuments of the leg are pitted on pressure. The Amesbury apparatus is removed, and the leg is placed on

a pillow, a poultice being applied over the wound.

R Ammon. Sesquicarbon. ℥j.; Aquæ distillatæ, ℥viij. Solve, bat Mistura ejus cap. ℥j. in impetu effervescentiæ succi limonis recentis, ⅔ss. quartæ quæq. hora. Rice pudding for his diet.

30th.—The patient passed a tolerable night, the pulse is small and quick, the skin natural, tongue moist. Castor oil immediately, and the following mixture every two hours:—

R Ammon Carbon. ℥j.; Pulv. Aromat. ℥j.; Aquæ puræ, ℥viij. M. It. Mist. de q. a. sumat. 30. or ⅔ss. 2ds horis. A beef ground poultice to be applied to the wound.

31st.—The wound is beginning to suppurate, the pulse is stronger, but slightly intermittent.

1st Feb., New Year's day.—A superficial slough is forming in the middle third of the leg, near its inner side. The suppuration continues. The patient feels hungry, bowels stopped.

R Ol. Ricini. Tinct. Senne, aa. ⅔ss. M. It. potio statim sumenda.

2d.—Has occasional rigors. The pulse is small and weak.

3d.—Much constitutional disturbance, tongue dry, face flushed; pulse frequent and feeble. Extensive sloughing of the skin and fascia of the leg.

5th.—A collection of pus having formed on the fibular side of the leg, under the integuments, is let out by several long incisions.

6th.—The sloughs, both near the fracture and at a distance, are beginning to separate from the sound parts. His pulse is better. Half a pint of ale daily, in addition to his brandy.

8th.—The greater part of the slough is detached.

17th.—All the sloughs are removed; the granulations appear healthy. The limb has been again placed on Amesbury's apparatus.

Feb. 13th.—The wound continued to secrete healthy pus, and its surfaces to exhibit florid granulations, but no union could be produced between the disjoined portions of the bones. The continual discharge likewise began to produce an unfavourable impression on the patient's health. Under these circumstances Mr. White formed the resolution of amputating the affected limb, and carried his intention into effect to day. The limb was removed below the knee by the ordinary circular incision. After the patient had been removed to his bed Mr. White examined the amputated limb, and pointed

out the pathological appearances. No particle of granulation was visible on the fractured surfaces either of the tibia or fibula. Considerable laceration of the ligaments of the ankle-joint had taken place; the cartilaginous surfaces were seen to be completely absorbed, and the adjacent osseous surfaces were carious.

It was obvious, Mr. White observed, that the patient's powers were inadequate to repair the injury that had been inflicted on him. It would always, in his opinion, be a nice question to decide, which cases were fittest for immediate, and which for secondary, amputation. In civil practice he thought it the duty of the surgeon to lean to the side which suggested the saving of the limb, when the patient's constitution and temperament were favourable. The army surgeon had his judgment influenced by many casualties from which the civil surgeon is exempt; such as bad roads, rude means of transport, and a distant hospital. On shipboard, also, the rolling of the vessel is a circumstance that would prompt the surgeon to amputate, rather than leave a compound fracture to be exposed to multiplied risks. In the case before us, he said, he was justified in endeavouring to preserve a useful member for his patient, whose constitution had gone through the ordeal without receiving any detriment.

21st.—The patient has gone on well since the operation, not a single untoward symptom having shown itself. The wound is suppurating, and he feels quite comfortable.

In reference to this case Mr. Guthrie made the following clinical observations: If the rent of the integuments in a compound fracture unite when the cut edges are put in apposition, the compound fracture becomes a simple one, and the cure is effected by a very easy natural process. If it should not unite, the wound remains open, and the process of cure is much more complicated. A gun-shot fracture must always be a compound fracture, because the shot-hole cannot unite by the adhesive inflammatory process; but a compound fracture caused by a heavy cart-wheel going over the part must be a more dangerous injury than any musket-ball fracture. The treatment, therefore, in all serious cases, is pretty much the same.

In a gun-shot fracture the bone or bones may be more or less broken, and the splinters larger or smaller. In the first place, examine the wound at the moment, if you can. The object is to ascertain the state of the fracture and to remove the splinters, and the extent of the incision must depend on them. If the ball has merely struck the bone and passed out, causing a transverse fracture only, there is

no necessity for dilating such a wound at the moment, because the cut would have closed up before any bone would be ready to come away. This is one of the differences between a compound fracture from a slight accident and a gun-shot fracture. In the accidental fracture, the force being often applied to the ends of the bone, no injury is done to the broken part by direct collision. In the simplest gun-shot fracture the ball grazes and injures the bone, depriving it of a part of its periosteum, and a scale or portion will exfoliate at that place; but this is a process that takes time, and it is to give a free passage to this exfoliated bone that an incision is made. If the ball lodges in the soft parts after breaking the bones, an incision for its removal is proper, as it also is to give free vent to copious purulent discharge.

The peculiar danger from a gun-shot fracture, and which is not so great in a common compound fracture, arises from inflammation taking place in the membrane lining the shaft, and the cancellated structure of the bone. When this inflammation exceeds a certain point, it causes the death of the bone, and gives rise to a corresponding but peculiar action in the periosteum covering the bone externally. This membrane swells, thickens, and begins to deposit new bony matter, not only in its own structure but external to it, and induces the neighbouring vessels to take on a similar action, often to the extent of an inch or more. This ossific deposition begins early. I have seen it by the twentieth day, and even sooner, and by the end of the fourth week it is often very remarkable.

In a gun-shot fracture you have to dread the danger of shutting up splinters of bone by the newly-formed ossific matter; and the object you have in view is to prevent the extension, if not the establishment of this process. This is to be done by suppressing the inflammation in the internal part of the shaft, and in the limb generally. The splinters should be removed, if possible; the limb should be placed and maintained at perfect rest. Vascular action must be subdued by cold lotions and by leeches; and when cold is disagreeable, warm fomentations may be resorted to. When suppuration is fully established, the remaining splinters may be sought for and removed: but if this cannot be done, the gradual thickening of the limb begins to indicate the deposition of bony matter; and after a certain lapse of time, a spot of inflammation, at a particular part, implies an abscess in the new deposit. This is caused by the irritation of the dead splinter, and when it breaks, the probe will find its way through a hole to the rough *sequestrum*. The dead bone must be immediately removed. The earlier

it is done the softer is the deposit, which in such cases, will act like Parmesan cheese mixed with lime.

Mr. Guthrie gave the following rules for the guidance of the young military surgeon:—

1. An upper extremity should be amputated for scarcely any accident that may occur to it from a musket-ball.

2. If the head of the humerus is broken to pieces, that portion of the bone should be sawn off, but the arm must be saved.

3. If the elbow-joint is shot through, it is to be cut out, and the forearm brought into the bent position.

4. A forearm will bear so much fracturing and cutting, that it should not be amputated without especial reasons.

5. A finger and thumb, or two fingers, are worth saving; but a serious injury to the wrist generally requires amputation.

L.

ST. GEORGE'S HOSPITAL.

CASES OF CHOREA.

To the Editor of the Medical Gazette.

SIR,

HAVING noted the following cases, which were admitted into the physicians' wards of St. George's Hospital, I forward them to your journal for insertion.

I remain, sir,

Your obedient servant,

C. J. B. ALDIS, M.D.

13, Burlington-street,
Feb. 13, 1838.

CASE I.—Sarah Haines, æt. 12, admitted October 27, 1832. Pulse 96, soft; skin natural; tongue yellow, furred; bowels not open to-day.

Chorea Sancti Viti; no pain in the head. Had a slight attack of the same complaint twelve months ago; the present attack came on three weeks back.

℞ Hydrarg. Submur. gr. iv.; Jalapæ Radic. ʒss.; Muc. Acac. q. s. Fiat pil. iij. statim, et tertio quoque mane repetendæ.

Baln. Imbriferum diebus intermediis; with a draught composed of Senna et Mist. Camph. ter die.

27th.—Some giddiness.

Hirudines xij. temporibus.

29th.—Motions dark-coloured.—Perstet.
31st.—Bowels acted three times yesterday after the pills.

Rep. Pil. Hydrarg. et Jalapæ statim.

℞ Ferri Subcarb. ʒss.; Valer. in pulvere, ʒj ter die.

℞ Extract. Coloc. e. ʒss.; Extract. Hyosc. gr. iij. omni nocte. Omit. Haust. Sennæ.

She continued to improve under the same medicines, the doses of which were occasionally varied, according to circumstances, and on Dec. 12 was cured.

CASE II.—Ann Collier, æt. 12, admitted Feb. 22, 1833. Pulse 84, rather small; skin warm; tongue slightly coated; bowels open from medicine; emaciated.

Is affected with irregular motions of the muscles, chiefly of her hands, over which she has a partial control; no headache. Has been affected in this way two months.

℞ Haust. Sennæ; Decoct. Aloes e. aa. ʒvj. mane et meridie quotidie.
Diata ordinaria.

March 1st.—Perstet cum Haustu alterno mane. Balneum Imbrif. tepidum diebus intermediis.

4th.—Much steady; tongue clean; pulse weak and small; skin cool; bowels open four times; motions natural.

℞ Mist. Ferri e. ʒj. ter die. ℞ Pil. Aloes e. ʒss. o. n. Rep. Balneum ut antea.

18th.—℞ Infus. Rosæ e. ʒxj.; Quinæ Sulph., Ferri Sulph. aa. gr. j.; Tinct. Cardam. e. ʒj. ter die. Pil. Aloes ut antea.

22d.—Not quite so steady.

℞ Hydrarg. Submur. gr. v. horâ somni. Haust. Sennæ crâs, postea perstet.

25th.—Not quite so steady; complains of headache.

℞ Calomel, Extract. Coloc. e. aa. gr. iv. hæc nocte. Haust. Sennæ crâs, postea perstet.

April 1st.—Tongue clean; bowels well opened.—Perstet.

10th.—Cured.

CASE III.—Jane Elderfield, æt. 10, admitted July 31, 1833. Five weeks ago she was seized with convulsive actions in her left hand, which were involuntary, every day at five or six o'clock. She cries occasionally, and throws herself about; no headache; bowels open; tongue white, which protrudes to the left side of her mouth when she attempts to speak; much difficulty and hesitation of speech. The convulsive motions affect both hands, legs, face, and neck.

℞ Tinct. Ferri Muriat. ℥x.; Infus. Quassiae, ʒj.; Aq. Piment. ʒij. bis die.

℞ Calomel. gr. ij.; Pulv. Jalapæ, gr. v.; Zingib. gr. j. omni nocte.

R Haust. Sennæ, ʒj. omni mane. Balneum Imbriferum alt. mane.

Aug. 1st.—Bowels well opened by the medicine; spirits low.

Omit. Baln. Imbrif. Broth diet. Perstet.

3d.—Projects the tongue readily.—Perstet.

5th.—Much better.

6th.—Walks better.—Perstet.

7th.—Perstet cum Haustu bis in die.

R Pulv. Jalap. e. gr. x. omni nocte.
Enema Terebinth. omni alt. mane.

9th.—Slept very well; much quieter; puts out her tongue with less effort; very much improved.—Perstet.

R Ferri Carbon. ʒj. ter die. Perstet cum aliis. Pulv. Jalap. c. omni alt. nocte.

10th.—Can move her left arm; very much improved.—Perstet.

16th.—No spasm in sleep at night — Perstet.

19th.—Better; the motion of left arm is very much improved.

Diæta ordinaria.

She recovered by degrees.

CASE IV.—Charlotte Clements, æt. 15, single, Fulham, admitted May 22, 1833. Pulse 115, soft; skin moist and cool; tongue slightly furred; bowels open; urine free; catamenia have not appeared.

Chorea; twitching of all parts, one side not more than the other; no pain in the head or any where else; flatulence. Attacked a week ago, and her complaint is attributed to fright.

R Pil. Aloes e. ʒss. sextis horis.

R Tinct. Valer. Ammon. ʒj.; C. Decoct. Hordei sextis horis intermediis.
Diæta ordinaria.

May 24th.—Has brought up her draught each time she has taken it, and the pills have produced little effect; chorea the same; a large boil on the chin.

R Hydrarg. Submur. gr. v. statim.
Extract. Coloc. c. ʒss. tertiis horis donec exoneretur alvus.

25th.—Enema Oleosum. Perstet.

29th.—Skin cool; tongue nearly clean.

R Mist. Ferri e. ʒj. ter die. Rep. Pil. Coloc. c. ter die.

31st.—Rep. Pil. bis die. Perstet.

June 3d.—Somewhat steadier than she was; glands confined; pulse 52, soft; skin natural; tongue furred.

R Haust. Sennæ statim, et post horas quatuor. Repetatur postea pilula ter die. Adde Haustui Ferri Spir. Æth. Sulph. c. ʒss.

7th.—Much quieter; pulse 60.

Balneum Imbrif. tepidum alt. mane.
Rep. alia.

17th.—The bowels now act freely with only two pills; pulse 70; skin cool; tongue somewhat furred.—P.

She continued the use of purgatives and steel until July 8, when she was reported to be quite steady.

10th.—Cured.

CASE V.—Ann Blade, æt. 16, admitted August 14, 1833. Pulse 80; skin cool and moist; tongue slightly furred, rather dry; bowels open; urine free; catamenia regular.

Chorea; headache; throbbing in the head, relieved by being recumbent. Ailing two months, and has been getting worse up to the present time.

Applic. Hirud. xij. fronti.

R Hydrarg. Submur. gr. v. omni nocte.
H. Sennæ omni mane. Diæta ordinaria.

16th.—Balneum Imbrif. tepidum alt. mane. Haust. Sennæ alt. mane intermedio.

19th.—Bowels well opened; motions feculent, mud like, and very offensive; skin cool; tongue white.

R Hydrarg. Submur. gr. v. alt. noct.
Haust. Sennæ alt. diebus. Balneum ut antehac.

20th.—Not any steadier.

27th.—Motions of the limbs still the same; bowels well opened; pulse 100, small; skin cool.

R Mist. Ferri e. ʒj. ter die. Perstet.

30th.—Not any steadier; bowels opened.

Habeat Baln. Imbrif. frigidum vice tepidi. Perstet.

Sept. 2d.—Convulsive movements considerably less troublesome.

R Extract. Coloc. c., Pil. Galb. c. aa. gr. v. fiant pil. duæ omni nocte sumend. Augentur Mist. Ferri e. ad ʒjss. ter die.

7th.—Much improved; convulsive movements almost ceased; bowels open. Perstet.

16th.—Very little remains of the convulsive muscular movements can be perceived; in other respects quite well.

Discharged in a few days after, cured.

REMARKS.—If we refer to the ages of the persons whose cases have already been detailed, we shall find that the eldest was 16, and the youngest 12 years old. In two other cases, which I have not forwarded to

your journal, I find that the disease commenced in one about 17^s, in the other at 24^h. Dr. Reeve has stated, in the eighth volume of the *Edinburgh Medical and Surgical Journal*, that of 84 patients, only one man was 40, and four females above the age of 21. This is one of the nervous diseases in which modern practice has improved upon the older system of blood-letting, the most powerful remedy we possess against some of them. Sydenham's method of cure consisted in venesection and purgatives. Dr. Hamilton has more recently cured it with purgatives alone; besides which, the authority of many other medical men might be brought forward to prove the frequency of the cure of chorea without the assistance of venesection, clearly showing that it must have been formerly employed in several cases unnecessarily. Occasionally symptoms appear which indicate the necessity of the patients losing blood. I have seen the method adopted in the above cases practised a good many years at St. George's Hospital; and only one case has come under my observation in which it failed, and that was a patient in whom the disease had existed five years. I allude to Mary Preston, mentioned in the first note. The case of Lucy Blaydon, referred to in the second note, was also very obstinate, but it yielded to detraction of blood, blisters, and diuretics of digitalis and cantharides. The other cases treated with purgatives and steel produced a most satisfactory result. Some of my notes mention the utility of the liquor potassæ arsenitis combined with purgatives.

REMARKABLE CASE

OF

INFLAMMATION AND SUPPURATION OF THE PLACENTA.

THERESA RAFFO, of Leghorn, aged 32, of a good constitution, being pregnant for the seventh time, had miscarried in October, in the second month of pregnancy, in consequence of a blow which she had received upon the abdomen. She recovered with facility, and a month afterwards became pregnant again; nor did she complain of any ailment until the end of the sixth month. She was then attacked with pains in the loins, in consequence of over-fatigue and vexation; they became more and more severe, and on the

5th of June they extended to the uterus and thighs. On the seventh day, uterine contractions appeared, and the patient was obliged to keep her bed. M. Landini was now called in. He found her labouring under burning fever, anxiety, great thirst, want of appetite, difficulty of making water, sharp uterine pains, a slight sanguineous discharge from the vagina, headache, and vomiting. The midwife who attended the patient, believing that delivery was near at hand, made her exhaust herself by useless efforts, and gave her three glasses of Spanish wine. The abdomen was very painful when touched, and there were frequent but ineffectual attempts to make water. A consultation took place, and it was agreed that there was inflammation of the uterus. The measures ordered were the immediate introduction of the catheter; a large bleeding from the arm; leeches to the abdomen and the anus; emollient poultices; a purgative of castor oil; and cooling drinks. The bleeding was repeated three times, with a marked amendment of the symptoms; the fever, however, continued, though in a less degree. The patient complained of dull pain in the left side of the uterus, in the loins, and the thighs, which continued until the 19th of June. The patient now suffered from violent contractions of the uterus; moreover turbid and very fetid water flowed from the vagina, and afterwards putrid blood.

This flux lasted till the 23d of June, but it did not appear, from examination *per vaginam*, that delivery was about to take place. The antiphlogistic treatment was still continued.

On the 27th of June the woman seemed extremely exhausted; the abdomen was very painful when touched; there was difficulty in making water; diarrhoea, with very fetid stools; and sores on the sacrum, from the recumbent posture. On examination, the uterus was found to be very high, and its cervix very narrow. A second consultation was held, and M. Landini was of opinion that the disease had been originally an inflammation of the placenta, which had afterwards spread to the uterus, the intestines, and the peritoneum. The continuation of the antiphlogistic treatment was advised.

On the 29th of June the patient was worse; there were uterine contractions, short, but very painful; and frequent desire of making water. In sitting on the pot for this purpose, she voided a soft substance from the vagina; this was the placenta in a state of suppuration.

M. Landini ascertained, by attentive inspection, that it really was the placenta. The suppuration extended over the whole

* Mary Preston, æt. 22, admitted June 29, 1831, with chorea of five years' duration.

† Lucy Blaydon, æt. 25, admitted Jan. 30, 1833, affected one year with chorea.

surface; its form was not lobular; the chorion was perfect, and served as a bag for the whole puriform placenta, which was contained in it; and a portion of the funis was to be seen, half a foot long, which had almost become ligamentous.

On examination, the cervix uteri was found to be lower than on the preceding days, but hard and resisting. The ergot of rye was prescribed with the intention of promoting parturition; the pains became more frequent, but were neither lasting nor expulsive; the contractions were excessively painful, especially on the left side of the uterus, where it was supposed that the placenta had been adherent.

Meanwhile, the patient grew worse; in the evening she was covered with cold sweats all over the body; there was general oppression; ardent thirst; very great anxiety; derangement of the cerebral functions; ringing in the ears; vomiting; a very small and frequent pulse; and fainting fits. The vagina was very hot to the touch, the os uteri soft and dilatable, and the child's foot was to be felt in it. An ethereal draught was prescribed, with warm fomentations to the extremities, and on the abdomen; clysters of mallow water and olive oil, and the same injected into the vagina. It was thought that the patient would sink in the night. Nevertheless, the next morning she was rather better, having slept three hours, and perspired copiously; she made water easily, and had had very fetid stools; felt stronger, and less thirst; the pulse, too, was better.

M. Landini attributed the violent symptoms of the previous day to the four doses of the ergot which had been administered. It is well known that the most prominent effect of this medicine consists in lowering the vital powers, like all contra-stimulant remedies.

The child's hand having passed through the cervix uteri, M. Landini waited till the patient's strength was sufficient to allow him to turn, which he did with ease in the course of the day. The child was putrid. Having again introduced his hand into the uterus, M. Landini found a small portion of placenta strongly adhering to the fundus, which he did not force away. The left side of the interior of the uterus seemed to him covered with fungous growths.

The consequences of the operation were at first alarming; there was hæmorrhage, very intense inflammatory reaction, with anxiety, delirium, a small pulse, cold extremities, and the abdomen painful and tympanitic. The treatment was regulated accordingly, and these symptoms were gradually dissi-

pated. The woman seemed out of danger on the fifth day, when the right leg was attacked with *phlegmasia alba dolens*, which was ascribed to the diseased state of the uterus. The leg was ordered to be fomented with an infusion of digitalis leaves and belladonna, and improvement took place.

The lochial discharge was bloody, and as if putrid; small bits of placenta were distinguished in it, supposed to come from the breaking down of the remaining portion mentioned above. Injections of mallow water into the vagina were ordered, and oily clysters.

The 8th day after the operation the state of the woman was sufficiently satisfactory. After this period, however, she began to complain of giddiness, buzzing in the ears, and fever. Leeches were applied to the nape of the neck with success, and the patient's convalescence was no longer doubtful; indeed, she began to eat with such appetite, that there was some difficulty in checking her voracity. On the 15th of July she complained of dyspnoea and fever; and the left leg was attacked with *phlegmasia alba dolens*. Fomentations of digitalis and belladonna were ordered, and a gentle purgative. Two days afterwards the oppression increased, especially on the left side of the chest, and there was orthopnoea. The diagnosis was, that the disease was acute hydro-pericarditis: pills of digitalis and squill were prescribed, with a grain of extract of aconite in the evening, and milk diet. She now grew better; two blisters were applied to the calves; convalescence again took place, and the patient left her bed. The lochial discharge continued to be fetid and purulent, but less so than before. The patient's health continued to improve.

Some days afterwards, the woman, who was now far advanced in her convalescence, underwent a strong mental emotion by the unexpected return of her husband from Rome. The fever and dyspnoea returned; the legs became swelled and anasarcous; the lochial discharge stopped; there were vomitings, and symptoms of meningitis. She died on the 7th of August.

Post-mortem Examination twenty-four Hours after Death.

1. *The lower extremities.*—These limbs having suffered from *phlegmasia alba dolens*, first engaged the attention of M. Landini. An incision was made on the inside of the right leg, extending from the malleolus to the groin; and the sub-cutaneous cellular tissue was found to be copiously infiltrated with a yellowish serum, which flowed forth at each stroke of the bistoury.

The aponeurosis and subjacent muscles were healthy. The vein and corresponding artery adhered to the surrounding cellular tissue, which was itself thickened, and soaked with water. The external surface of the vein was of a dull red throughout its whole length; the artery was in its natural state.

The left leg afforded exactly the same appearances. In continuing the dissection of these vessels from the foot to the groin, it was seen that the inflammation of the cellular sheath was greater the nearer it was to the crural arch; that is to say, its thickening, its adhesions and the infiltration, were more obvious in proportion as the spot was higher.

2. *The abdomen.*—A vertical incision, which was extended as far as the vulva, and another one in a transverse direction, divided the skin and muscles, without injuring the peritoneum. It was attempted to dissect their layers without touching the serous membrane, but this was found to be impossible, for the peritoneum, which seemed much altered by disease, adhered strongly to the corresponding parietis of the abdomen. The liver was in a state of extreme atrophy, particularly the right lobe, which filled the upper half of the abdominal cavity. The lymphatic vessels meandering over its surface, and filled with a coloured fluid, were easily distinguished upon it. The right lobe was of a deep yellow, and so soft that the pressure of the finger was sufficient to break it down. Its internal tissue was very yellow, and seemed formed by the union of a multitude of small bodies of a shining yellow. On cutting it in different directions not a drop of blood flowed out, its blood-vessels being very contracted and quite empty. The gall bladder was larger than natural, and full of air, but not containing a drop of bile; its parietes were very thin, transparent, and whitish. The hepatic and cystic ducts were small, indurated, and obstructed; the *ductus choledochus communis* was wasted away, and entirely closed at its duodenal extremity. The two other lobes of the liver were not so much disorganized. The whole organ adhered strongly to the diaphragm, which was itself red and thickened. The vena porta and its ramifications had the same structural alterations as the diaphragm.

The spleen was blackish, very small, and contained a minute quantity of very black blood.

The stomach was distended by very fetid gases; its external surface was of a yellowish white colour; its cavity contained a little ptisan. The mucous membrane bore no trace of disease, and its vessels were not even filled with blood. The

mucous membrane of the œsophagus was also healthy.

All the intestines were of a yellowish white colour externally, and they were distended by gases, especially the colon.

The mesentery was healthy, but the peritoneum, reflected over the fundus of the uterus, the bladder, and the mesorectum, was decidedly gangrenous. The interior of the bladder was healthy; the uterus was of the regular shape and size, but its external tissue was denser and redder than in the natural state; the left fallopian tube and ovary were completely decomposed. The inside of the uterus and the vagina bore no marks of structural disease, except that the uterus was lined with a false membrane of a blackish colour, which was easily scraped off with the scalpel, the subjacent membrane of the uterus was slightly thickened, but was otherwise of the natural appearance. At the orifice of the left fallopian tube there was a smooth and shining tumor, like the kernel of a peach, and this tumor was surrounded by a rim, formed by the swelling of the uterine membrane. When cut into vertically, the uterus showed numerous openings of vessels, which poured forth blood.

3. *The Thorax.*—The pleura was thickened, and adhered to the ribs. The lower lobe of the right lung was very small, and manifestly gangrenous; the upper and middle lobes were hepatized. The mediastinum was disorganized like the pleura. The left lung was diminished in size, and full of blood; its lower lobe was gangrenous.

The pericardium, which was yellow and much thickened, contained a glassful of yellowish serum. The heart, which was very small, was softened, flaccid, and brownish; the right auricle was inflamed, large, and lined with a great quantity of plastic lymph.

4. *The Blood-vessels.*—All the principal vessels of the limbs and internal cavities were examined. In the legs and thighs the veins were very red internally, decidedly inflamed, and lined with plastic lymph, like the right ventricle. The internal iliac veins, and the ascending vena cava in a still more marked degree, bore the clearest traces of intense inflammation; they, too, were lined with lymph. The aorta, the arteries of the abdomen, and those of the thighs and legs, were healthy.

5. *The Head.*—The dura mater was thickened, and very red; its vessels were filled with blood. The cerebellum was covered with numerous vessels full of blood, but was of the natural size. The pia mater was red, dense, thickened, and

vascular, and it adhered to the brain. The ventricles contained much yellow serum, and the plexus choroides was very vascular. The arachnoid was much inflamed and thickened, and was separated from the pia mater by sanguineous serum. The grey substance of the brain, the corpus callosum, and still more the cerebellum, the pons varolii, and the medulla oblongata, were of a reddish yellow colour, and steeped in red serum. All these parts were considerably softened; and the substance of the brain stuck to the fingers, like so much pulraceous matter.

The medulla oblongata [the spinal marrow?] could not be examined, as night was coming on. — *Gazette des Hôpitaux*, Feb. 13, 1838.

CASE OF LEAD COLIC FROM SWALLOWING SMALL SHOT.

A MAN, named Pezonzino, aged 45, a husbandman, at Celavegna, in the Sardinian States, having used too much cinchona in the intermittent fevers with which he was attacked almost every year towards the autumn, was seized with a violent gastro enteritis. He got well, but such an irritability of the alimentary canal remained behind, that the slightest cause brought on colic and diarrhoea. In 1828 he was attacked with a severe pneumonia, and after it was cured, he changed his occupation, and became a pedlar. In this new course of life he took to drinking, and thus revived the gastro-intestinal affection, which from this period grew worse, under the influence of the smallest atmospheric change. After a variety of treatment, the patient not being cured as soon as he wished, applied to a professed wizard, who promised to cure him, and made him swallow six ounces of shot*.

The patient immediately felt an uncomfortable weight in his stomach, accompanied by cructations, with a disagreeable metallic taste. These were soon followed by dreadful colic, which did not leave him till the sixth day, in consequence of the evacuation by stool of the lead which he had swallowed. As he was relieved by this salutary evacuation, and attributed it to the effect of the remedy,

he suffered himself to be persuaded to swallow some more the next day. But this time he was immediately attacked anew with the most violent pains of the bowels, with vomiting, and painful sensibility of the abdomen. His state became alarming, and he was obliged again to call in Dr. Ruva.

He was now labouring under violent headache, with profound drowsiness; his tongue was foul, with a bitter taste in his mouth; the skin burning; the pulse full, but not frequent; the expression of the face extremely altered. Every now and then there came on genuine paroxysms, accompanied by violent cramps in the legs, and retraction of the testes towards the inguinal ring. There had been obstinate constipation for six days; the urine was voided but seldom, and in small quantity. The patient vomited all that he swallowed.

The first indication that Dr. Ruva endeavoured to fulfil, was to relieve the bowels from the lead which they still contained, by means of purgative clysters. After two bleedings, twelve leeches were applied to the anus, and, at a later period, to the epigastrium; fomentations and poultices were applied to the abdomen. Under this treatment the pains diminished, the stomach was able to retain some spoonfuls of cherry laurel water, and soon afterwards a draught containing castor-oil. At last a stool was procured, containing porraceous and fetid matter, from which about three ounces of shot were collected. From this moment the state of the patient changed; intolerable pain was followed by refreshing tranquillity, and he was able to enjoy repose. Every symptom now grew better, and disappeared; the hiccup, which previously was excited by the smallest quantity of drink, yielded to the employment of magnesia, and of sulphuric acid diluted with a large quantity of water. The fever, too, went off, and with it the sharpness of the abdominal pains. Some wandering pains, however, with a sensation of weight, were still felt, sometimes in the region of the cæcum, sometimes in that of the descending colon. Moreover, the return of the cramps, and of some of the preceding symptoms, gave reason to believe that the whole of the lead swallowed had not been expelled; the use, therefore, of the castor oil, and of the magnesia, was continued.

Forty-six days had now elapsed since Pezonzino had swallowed the first dose of shot, and he thought himself quite cured, when a slight excess in eating brought back all his symptoms with such violence that it was necessary to have recourse again to bleeding and the other remedies

* This remedy is of long standing in the treatment of volvulus. Van Helmont, in particular, made the practice general, by laying down this dangerous aphorism:—"Neminem illo perire si globuli plumbei scolopetarii deglutiantur, ut sub pondere propellant impedimentum in intestinis hærens."—(*De Flatibus*, § 31.) But Sydenham, with more reason, disapproved of it, as dangerous:—"Hæc parum conducere, et noxam sæpe haud contemnendam inferre."—(*Op. Med. Sect. 1. cap. xiv.*)

above mentioned. This relapse produced an alvine evacuation, from which two more ounces of shot were collected, making ten altogether. His convalescence was very laborious, and lasted two months; but it was followed by complete recovery. Pezonzino has now been cured eight months, and has ever since enjoyed perfect health.—*Annali Universali*.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Feb. 28, 1838.

THE PRESIDENT IN THE CHAIR.

A PAPER was read, from Mr. Hadwen, house-surgeon of the Lincoln Hospital (communicated by Mr. Quain) detailing the particulars of

A Case of Popliteal Aneurism,

for the relief of which Mr. Hewson, the surgeon of the hospital, tied the superficial femoral artery on the 18th of July last. Hæmorrhage having taken place from the wound to a large amount, on the 13th day, the common trunk of the femoral artery was tied immediately below Poupart's ligament. Notwithstanding the second operation, however, hæmorrhage recurred two days afterwards, from the situation of the first ligature, which rendered amputation necessary on the fifteenth day after the first operation. From this time the case proceeded satisfactorily, until the eighteenth day after the operation, when blood began to issue from the wound in the groin. The hæmorrhage becoming more alarming during the night and on the following morning, Mr. Hewson determined on tying the external iliac artery, as the only means of saving the patient's life. The operation was accordingly accomplished by Sir Astley Cooper's method, and the patient afterwards recovered with scarcely a bad symptom. The relation of the case is followed by some observations on its principal points of interest.

The author takes occasion to observe, that the common femoral artery, from the uncertainty of the points of origin of the profunda, epigastric, and circumflexa ilii, as well as from the probability of the internal circumflex arising from it instead of from the profunda, is singularly ineligible for an operation of this description. In confirmation of this opinion, he cites six recorded cases in which this artery has been tied; two of which were followed by fatal hæmorrhage, which would probably have also occurred in the

other form, had not the external iliac been tied. The author contrasts this mortality with the success which has attended the ligature of the external iliac artery, which, we believe, is universally admitted by surgeons of experience. Mr. Holmes relates twenty-two cases in which this artery has been tied, in none of which was the operation succeeded by secondary hæmorrhage. Indeed, it is the opinion of that distinguished authority that the external iliac may be tied with as much security as any artery to which a ligature has been applied. From these premises, the author deduces the conclusion, that the operation of tying the common femoral artery should be entirely exploded.

A paper was afterwards read—

On Division of Stricture of the Rectum high up in the gut. By R. A. STAFFORD, Esq., Surgeon to the St. Marylebone Infirmary.

The author describes two cases in which he has performed this operation—the first in 1831, the latter in the last year. In the former case the stricture existed at about two inches and a half from the anus, and was of nine years' standing when the patient was admitted into the St. Marylebone Infirmary. The contracted part was indurated, and would only admit a No. 12 urethra bougie. The author divided the contraction towards the sacrum with an instrument which was shown to the Society, and was enabled to pass the middle finger through the stricture immediately afterwards. The operation caused very little pain, and only a few drops of blood were lost. The case went on favourably for more than three weeks, after which the patient was seized with erysipelas of the face, of which she died in a few days. Dissection proved to the author that disease of long standing which had existed in the whole course of the intestinal canal had been in a great measure relieved by the operation, and he expresses a conviction that had it not been for the occurrence of the erysipelas, the patient would have been entirely cured.

In the other case, the author divided two strictures by the same instrument—the first so contracted as only to admit a bougie of the size of No. 10. Slight hæmorrhage followed the division of the second obstruction, and in two hours afterwards the bowels began to discharge fæces of a very offensive character, which continued to come away in large quantity for two or three days. In this case diarrhoea continued for some time, but the patient was ultimately discharged perfectly well, the passage of the rectum being completely open.

PHYSICAL SOCIETY, GUY'S HOSPITAL,

Saturday, Feb. 24, 1838.

DR. GUY IN THE CHAIR.

MR. BIRKETT read some

Cases of Syphilis, with Remarks.

The cases were of superficial sores, in one instance followed by secondary symptoms. Mr. B. alluded to the few cases of Hunterian chancre met with in the present day, and was inclined to attribute this to the immediate use of mercury, which the lower classes now adopted on the first appearance of pimples or excoriations on the genital organs. The primary sores he believed to be indications of absorption of the poison into the system; he could not imagine that the peculiar suppurating surface was produced as a local effect by the irritating nature of the poison. The author gave it as his opinion that there was but one kind of syphilitic virus, and that the varieties of sore observed were the results of differences of constitution and habits among patients. When speaking of treatment, Mr. Birkett recommended practitioners to wait a day or two before they exhibited mercury, as he thought by that time a correct judgment might be formed as to whether or not that remedy was needful for the cure. The discussion which followed the paper was quite of a practical nature.

At the next meeting of the Society, Mr. Chapman in the chair, Mr. Brereton will read some observations *On Cerebral Physiology derived from Cerebral Pathology.*

MEDICO-BOTANICAL SOCIETY OF LONDON.

Feb. 28, 1838.

EARL STANHOPE, PRESIDENT, IN THE CHAIR.

A PAPER was read, by Edwin Saunders, Esq. one of the secretaries,

On the Employment of Creosote as an Odonalgic Remedy.

After giving a brief account of the drug, its discovery, qualities, and the various diseases in which it has been found useful, the author proceeded to distinguish tooth-ache into two or three distinct species, each requiring different treatment. In only one of these is the creosote useful, —viz. that which arises from sudden exposure of the internal membrane, or

nerve, which may be termed the acute form of tooth-ache. The rheumatic form of tooth-ache, in which pain is experienced over the whole side of the face, in neighbouring teeth, the ear, temples, &c. is not susceptible of relief from it; at least, in its undiluted state. The cotton, or wool, which serves as the vehicle for its introduction, should be small, and care should be taken to prevent any portion of the drug from falling upon the gums and lips. The oil of burnt paper, an old and popular remedy for tooth-ache, was supposed by the author to owe its virtue to the creosote which it contains.

ACCIDENTS ADMITTED AT THE LONDON HOSPITAL.

Surgeon, MR. ANDREWS.

Assistant-Surgeon, MR. ADAMS.

Feb.	Sex.	Age.	Case.
12.	M.	46	Lacerated forearm.
	M.	45	Compound fractured leg.
	M.	19	Contused testis.
	M.	20	Hernia.
13.	M.	28	Retention of urine.
15.	M.	24	Contused finger.
	F.	10	Burn.
	M.	42	Fractured humerus.
	M.	42	Compound fractured leg.
16.	M.	18	Sprained ankle.
	M.	67	Strangulated hernia.
	F.	20	Contused side.
17.	F.	6	Burn.
	F.	77	Burn.
	M.	51	Cut face.
18.	F.	2	Scald.
19.	M.	43	Sprained ankle.
	M.	45	Fractured fibula.
	F.	8	Burn.
	F.	9	Burn.
	F.	50	Fractured leg.
	F.	50	Contused side.

In-patients 22

Out-patients 27

Total 49

INSURANCE OFFICES.

To the Editor of the Medical Gazette.

SIR,

As the subject of remuneration to medical practitioners by Insurance Offices is now becoming very generally commented upon, there is no occasion for apology in offering the following remarks.

What has been the result of the "resolution" adopted by the medical practitioners at Leeds I have not been able to

ascertain. If such a resolution was to become general, the Insurance Offices must eventually succumb. Unfortunately the profession is composed of too discordant materials to anticipate such a conclusion; however, "*stillicidii casus lapidem cavat*," so may such a commencement as the Leeds resolution.

In my opinion the offices ought to remunerate the medical practitioners, not only because "they require" the information, but they can well afford to do it; for, maugre their sentimentality about a man providing for his family, &c., the advantage to those connected with these offices is as valuable (if not more so) as to the insurer himself. If the profits were not very considerable, would so many of these offices be starting every day, and so few which do not succeed? and every one acquainted with their proceedings is fully aware of their immense returns. At the present time there are offices whose "subscribed capital" is only a few thousands, one I am acquainted with is under 20,000*l.* and have a floating capital of hundreds of thousands. Now I would simply ask, what diminution in the profits of the shareholders or assured would arise from this act of justice to medical practitioners? The Directors know very well it would be a mere trifle; still they have the "assurance" to demand a favour which they are certain medical practitioners cannot generally refuse.

As for the patient paying his medical attendant, it is unreasonable; a patient naturally says, I do not wish to trouble you for a certificate, the medical officer at the office examines me, and they have likewise the testimony of two friends as to my habits, &c.; and as they require, after all this, the certificate of my medical attendant, they are the parties who are bound to remunerate him.

The offices are, I understand, expressing surprise that there should be so much feeling displayed upon the subject by the profession, as they state the amount would be so inconsiderable to any individual practitioner in the course of a year; but the Directors of these offices should bear in mind, it is not the amount, but the 'principle involved' that the profession 'is contending for.' Would the Directors think of sending a parcel of questions to a solicitor (and the solicitor's client relatively quite as much interested as the practitioner's patient) (without intending to remunerate him)? No. They go further than this; they actually pay the solicitor a per centage for merely recommending parties to their offices.

Finally, I am convinced medical men

would be more highly esteemed and better spoken of, if they were more chary of doing so much gratuitously; for what is the necessary result of being so liberal? Why, that the public consider it no favour to require what they would otherwise be ashamed to ask. Let us hope that those who are solely dependent upon their profession, and those who from their station and practice can support their medical brethren, adopt the "Leeds resolution." As for any general resolution of the profession in London, it is, I am afraid, hopeless. much may be effected by those who have the respectability and well-doing of the profession at heart; for the rest, we must trust to time and more liberal feelings on the part of the public towards (in reality) a too liberal profession.

I am, sir,

Your obedient servant,

MEDICALS.

March 5, 1838.

TRIBUTE TO MR.

To the Editor of the Medical Gazette.

SIR,

In reply to the letter of Mr. Middlemore, in your number for Feb. 24, I am directed to state that the committee for managing the fund subscribed to raise a tribute to the late Mr. Earle, have resolved to extend the period for receiving subscriptions to the end of the present month.

I am, sir,

Your obedient servant,

HENRY SMITH,
Hon. Sec.

St. Bartholomew's Hospital,
March 1, 1838

OSSIFICATION OF THE CRYSTALLINE LENS.

PROFESSOR GRILLO, of Naples, has communicated a curious case, that of a sailor who had suffered from gout for twenty-five years, and who from it was affected with an obstinate gouty inflammation of the eyes, opacity of the cornea, general atrophy of the eye, and perfect blindness, and both eyes were by degrees converted into white balls. This man died of apoplexy. The remarkable part of this case is, that it was not alone the external surface of the crystalline body which was ossified, but the whole substance was perfectly converted into solid bone.—*Observatore Medico di Napoli*; and *Dublin Journal*.

COLLEGE OF SURGEONS.

LIST OF GENTLEMEN WHO HAVE RECEIVED

DIPLOMAS.

February, 1838.

F. Margetts, Hilton, St. Ives.—T. M. Hoveil, Clapton.—J. Edze, Ipswich.—W. Shillito, Calcutta.—W. Reed, Whitwell.—J. Turner, Swansea.—G. W. Rame, Elmsthorpe.—E. Parry Tretower, Crickhowell.—M. A. Gerrard, Calcutta.—W. Moorhead, A.—F. Wakefield, A.—J. Lidderdale, Hungerford.—H. Stephens, Exeter.—J. Prankerd, Langport.—J. Scott, Jamaica.—J. Hope Wraith, Blackburn.—N. Rogers, Grantham.—W. H. B. Winchester, A.—W. R. Milner, Wakefield.—F. A. Laking, London.—J. C. Kent, London.—I. S. Bunce, London.—R. T. C. Baxter, London.—R. M. Gabbett, Limerick.—C. Marrison, High Barnet.—T. H. Barton, Long Stratton.—W. Purnell, Bombay.—W. Taylor, Leicester.—E. Y. Knowles, Godalming.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED

CERTIFICATES.

Thursday, Feb. 22, 1838.

John Moulden Burton, Rochester, Kent.—John Hicks Nankivell, St. Columb, Cornwall.—Wm. Benjamin Hemming, Edward Young, Newton Abbott.—Robert John De Hylton Scott, Exeter.—Thomas Norway Arthur, St. Columb.

Thursday, March 1, 1838.

Joseph Benson Woolmer, Aimerham.—Nathaniel John Haydon, Starcrop Kenton, Devon.—Joseph Stone, Manchester.—James Burton Moore, Kirby Fields, Leicestershire.

Thursday, March 8, 1838.

Charles William Pickering.—Wm. Henry Williams, Halkerton Lodge, Tiverton, Devon.—Chas. Greiz.—Wm. Thomas Christopher Robinson.—Chas. Smith, Kirriemuir, Forfarshire.—James Leonard, 18, Craven-street, Strand.—Wm. Punshon Banson, South Shields.

NEW MEDICAL WORKS.

Coulson on Diseases of the Bladder. 8vo. price 5s.

A Treatise on the Nature, Causes, and Treatment of Insanity. By Sir W. C. Ellis, M.D. 8vo. 10s.

Practical Observations on Typhus Fever. By A. H. Callanan, M.D. 8vo. 5s.

Diet and Regimen. By Robert Dick, M.D. Post 8vo. 6s. 6d.

Notes on the Medical History and Statistics of the British Legion of Spain. By R. Alcock. 8vo. 5s.

Dr. Holland's Principles and Practice of Medicine, vol. ii. Part 1, Cholera. 8vo. 7s. 6d.

WEEKLY ACCOUNT OF BURIALS.

From BILLS OF MORTALITY, March 6, 1838.

Abscess	1	Heart, diseased . . .	3
Age and Debility . .	49	Hooping Cough . . .	8
Apoplexy	8	Inflammation . . .	26
Asthma	30	Brain	4
Childbirth	7	Lungs and Pleura . .	8
Consumption	46	Influenza	6
Constipation of the .		Insanity	4
Bowels	1	Liver, diseased . . .	4
Convulsions	34	Measles	9
Dentition or Teething .	2	Miscarriage	2
Diabetes	1	Mortification	1
Dropsy	12	Paralysis	2
Dropsy in the Brain .	11	Rheumatism	1
Epilepsy	1	Small-pox	4
Erysipelas	1	Spasms	2
Fever	15	Unknown Causes . .	330
Fever, Scarlet	5		
Fever, Typhus	9	Casualties	6

Increase of Burials, as compared with
the preceding week } 354

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, Latitude 51° 37' 32" N.
Longitude 0° 3' 51" W. of Greenwich.

<i>Feb.</i>	THERMOMETER.		BAROMETER.
	from 28	to 37	
Thursday . 22	29	36	29.66 to 29.60
Friday . . 23	29	36	29.46 29.36
Saturday . 24	30	41	29.20 29.08
Sunday . . 25	49	35	28.74 28.87
Monday . . 26	32	39	28.90 29.02
Tuesday . . 27	30	39	29.10 Stat.
Wednesday 28	33	50	29.11 29.10

Winds N.E. and S.E.
Generally cloudy, with frequent and heavy
showers of rain.
Rain fallen, 1 inch.

<i>March.</i>	THERMOMETER.		BAROMETER.
	from 36	to 47	
Thursday . 1	36	47	29.10 Stat.
Friday . . 2	36	47	29.09 to 29.11
Saturday . 3	37	46	29.19 29.21
Sunday . . 4	28	47	29.08 29.15
Monday . . 5	36	45	29.48 29.70
Tuesday . 6	33	50	29.70 29.73
Wednesday 7	30	49	29.80 29.88

Winds N.E., N.W., and S.W.
Except the 6th and 7th, cloudy; with frequent
and heavy showers of rain.
Rain fallen, .9125 of an inch.

CHARLES HENRY ADAMS.

NOTICES.

We cannot insert the letter of H. H.

Will K. be good enough to suffer his name to be attached to his paper? We do not like to publish anonymous contributions on practical subjects.

We regret very much that we cannot make room for the Case of Opisthotonos sent to us some time ago from Bury; it is detailed too much at length.

Communications have been received from Mr. Julius Jeffreys, Dr. Conquest, Mr. Nunn, Dr. Fergusson, Dr. Staberoh, Mr. Newport, Dr. Thompson.

WILSON & SON, Printers, 57, Skinner-st., London

(*Extra limites.*)

PROPOSED CHANGES IN THE
ROYAL COLLEGE OF SURGEONS IN IRELAND.

To the Editor of the Medical Gazette.

SIR,

I HAVE delayed a second communication, expecting that a reply would have been made to my former one, in which the offensive imputation complained of would have been either justified or retracted by its author. He, however, has not condescended; and I am left at liberty to conclude, that he had advanced a charge which he was either unable or unwilling to maintain. A letter *has* appeared from "A Member of the Royal College of Surgeons in Ireland," professed to be "a comment," rather than "reply," to mine, and, however strange it may seem that one of the party, who have assailed us, should endeavour to escape under shelter of a comment, I am yet willing to take it for what it professes to be, and, before I proceed further in the case, to offer a comment in return.

The *animus* of the writer is sufficiently manifest from his use of the epithet, "*the soi-disant professor*," &c. But I regard not such provocations as these: they are beneath my notice. However, as I consider myself engaged in a contest rather of general than individual concern, I dismiss them with the wise though homely admonition, "that those who live in houses made of glass, ought not to throw stones." "Member of the College of Surgeons," as he subscribes himself!—why not, manfully, Professor in the School of the Collège?—he has, doubtless, no desire to humble the pride of its professors; and I repeat, that the school of the Collège was established without any authority of charter, and neither in its present nor its former one is there provision for the foundation of a single professorship. Therefore, the teachers of the two schools hold precisely

the same position, and may hail each other as *soi-disant professors* all, if it so please the Member. Meantime, if he will take the trouble to consult Johnson, he will find that it requires neither charter nor act of parliament to make a professor. The second paragraph of the Member's letter sets forth, that the great bulk of my letter "being purely of a personal nature, does not, in his opinion, demand either notice or reply." This, I must admit, is very proper and dignified, and becomes amazingly a gentleman who, in the very next sentence, casts a personal and individual aspersion upon myself. The express object of my letter was to prove that the charge against which I protested was a calumny, and an unfounded one. It was of a personal nature doubtless. What else could it be? Of what kind was the wanton assault? Personal, purely personal; and must not the defence be the same? But it shall not pass. He that attacks and chooses his weapons is not at liberty to decline. He is bound either to submit or see it out, and the announcement, coming as it does from the reserve champion of the party—for be it observed, that it is no longer the original calumniator, "the Graduate of three Colleges," who writes—amounts to an acknowledgment that he was unable to grapple with that which he affects to disregard. He is too magnanimous, indeed, or else too well assured, to "hairsplit the letters of an official oath," in order to prove another a renegade! Yes, yes—but though he will not hairsplit, he seems not to consider it beneath him to gloss and to pervert. It is enough for him to know, that I have "thought proper to unite myself in intimate relation with the bitter and avowed enemies" of the Collège! Alas! this is but a sorry shift. Were it true, I should agree with him.

But it is *not—emphatically* not. I have not united myself with the bitter and avowed enemies of the College; but the party, of which the Member is the champion, have thought proper to make enemies to themselves of the Members of the Corporation, which *had* elected me and my colleagues their professors. And am I to be responsible for their misconduct? or to be the victim of their cunning? Assuredly not. Nor shall any power make me so. The enmity of which they complain has been raised by the men themselves; and as they have raised, so let them allay it. They commenced their hostilities at the beginning of the present session with the lecture to which I adverted in my last,—a lecture introductory to a course of Anatomy, delivered by a professor and *president!* of the College, the object and end of which were, as I stated, the malpractices of Apothecaries, and the consequent necessity that the Members of the College should make Apothecaries of themselves! What could have been the design of this proceeding, if not to create enmity between the two bodies? What other effect could it have but to commit the College and the Hall, and to render it, and every man connected with it, hostile to the College? I cannot say that such was its design, but had it been, the means for its accomplishment could not have been better devised. And now let the party who allowed themselves to be betrayed into so rash and unseemly a step retrace it as they best can, for it was altogether their own doing. If not, let them pay the penalty, and pay it they will.

But I must hasten, else my friend the Member will have fallen asleep. I see him nodding already. The next paragraph of his letter furnishes matter for much and grave comment. He must therefore extend his indulgence, and shake off the anodyne (!) effects of the preceding remarks. In my former letter I stated that the apothecary is compelled, as well by the wants of society, as by the position in which he is placed by others, rather than by himself, to act as a general practitioner. From this the Member dissents, and he requests that he may be permitted to state the case as it *really* is. Can you, sir, after this, have any doubt that my statement is incorrect, if not false, and that that with which he has favoured you is the true one. I really feel it necessary, be-

fore I proceed, to bespeak the patient and impartial attention of your readers, for it can hardly be suspected that any man would so commit himself as to contradict another, and even to cast a polite imputation of duplicity or falsehood upon his statement, without being assured that he was himself in the right, or that he would expose himself to be convicted of absolute and inexcusable ignorance—for I cannot suppose that he knew better—of a subject upon which he has undertaken to guide public opinion. He asserts that “the medical profession in Ireland has been hitherto entirely composed of physicians and surgeons who are in the habit of practising only as prescribers or consultants: no class equivalent to the general practitioner or apothecary of England having ever been sanctioned by law or custom in this country.” This statement contains two errors so monstrous, that it is impossible any man who knew better could have ventured to assert them. 1. It is an error that the medical profession in Ireland has been hitherto *entirely* composed of physicians and surgeons, the profession containing, even according to the writer’s own shewing, since 1791 at latest, a third estate; viz. the Apothecaries. 2. It is an error, that “no class *equivalent* to the general practitioner or apothecary of England has ever been sanctioned by *law* or *custom* in this country.” 1. Every graduate in medicine, or licentiate in surgery, who has taken the license of the Apothecaries’ Hall, Dublin, is *by law* entitled to practise both as apothecary and physician or surgeon throughout Ireland, except in Dublin, or within a few miles thereof. 2. It is notorious that Ireland abounds with men who practise in the double capacity—that in the north the practitioners are chiefly of that class, and that in Dublin and the south they are numerous also. How they must have stared at the Member’s assertion! And further, that apothecaries do, and have long acted in that capacity—according to the licentiates of the Dublin College of Surgeons resident in Sligo, “from time immemorial.” In the next statement to which I shall refer, the Member asserts himself one half of my statement, for he admits that the medical profession itself had consigned to the apothecary the offices of applying leeches, &c. Next he asserts, that the

apothecaries "have boldly set themselves forward, not merely as general practitioners, but as *the sole licensers and instructors of that class of the profession.*" What his authority may be for the latter part of this assertion I cannot even imagine. Every member of the College of Surgeons, at all conversant with the matter, must know that the Hall were willing to receive, and did receive, the certificates of the Professors of the College School so long as they were allowed to do so; *i. e.* until the College were induced to reject theirs; that the Hall do receive the certificates of the Professors of the School of Physic, and of others; and that the body of the apothecaries are desirous that the general practitioner shall be licensed by a Board, in which each of the three Medical Corporations shall be represented. I cannot permit myself to comment at length upon the assertions which I have adduced. Wilful misrepresentation I do not impute to the gentleman; ignorance of such familiar circumstances I cannot, without his own authority, admit; but the man must be silly indeed, who could persuade himself that such errors would be permitted to pass without exposure. I have left only the alternative, *aut insuit homo, aut versus facit*; and those who have perused his succeeding paragraph will, I suspect, entertain little doubt upon this point. What does it say?—Hear, College of Surgeons in London!—Hear, College of Surgeons in Ireland!—Hear, ye sister and hitherto rival institutions, now such no longer! "The apothecaries of England are the brethren of the Irish surgeons," and of course the Apo-

thecaries' Society at London is the sister institution to the Royal College of Surgeons in Ireland! This speaks for itself, and needs no commentary. But will the College of Surgeons in Ireland permit the statement to pass without notice and without contradiction? If it be true, it is time to admit that all her past claims have been but pretensions; that she has robbed alike the public and the profession; and that she has been as dishonest as pretending; for the Society of Apothecaries neither profess surgery, nor even require attendance upon a surgical lecture. It cannot be. The institution which could be so degenerate as to sanction such a disavowal of duty and station has fulfilled its destinies, and is unworthy to continue. Its sun has gone down, and the hour of its dissolution has arrived. But let us hope for better things. The profession can now, indeed, decide whether in what the party, of which the Member is the spokesman, are doing, they are desirous or not to degrade the College and its members from the station they have hitherto held.

I beg to conclude with an apology to the Member for the length of this epistle; at the same time reminding him of his pledge to expose the "calumnies" of my letter. Or shall we have the third of the Curiatii?

I have the honor to remain,

Your very obedient servant,

A PROFESSOR IN THE SCHOOL OF
THE APOTHECARIES' HALL.

Dublin, Feb. 10, 1838*.

* The insertion of this letter has been delayed for various reasons.—ED. GAZ.



THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF
Medicine and the Collateral Sciences.

SATURDAY, MARCH 17, 1838.

LECTURES ON TUMORS,

Delivered at St. George's Hospital,

By CÆSAR HAWKINS, ESQ.

LECTURE IV.

III. SARCOMATOUS TUMORS (*continued.*)

1. Conglomerate or Pancreatic Tumors.
5. Conglobate Tumors.
 - a. Simple Conglobate Tumors.
Tumors in the Parotid Gland.
 - b. Tubercular Conglobate Tumors.
6. Scrofulous Tumors.
7. Cystic Tumors.
8. Tubercous Cystic Tumor.

Circumstances in which Cysts are met with in Tumors.

Theories with regard to Cysts in Tumors.

III. SARCOMATOUS TUMORS (*continued.*)

III. 4. THE next genus of sarcomatous tumors we will call the *conglomerate tumors*; by which term I mean to designate the kind of tumor which Mr. Abernethy has termed *pancreatic*, i. e. a new substance, consisting of numerous small portions, each resembling the rest, and the whole bearing some similarity of appearance to that of the pancreas or salivary glands. Some tumors, however, of apparently the same kind, consist of much more minute elementary particles, more like the acini of the liver in size, than the separate portions of the salivary glands; and I therefore prefer the name *conglomerate*, implying, as with the conglomerate glands of all kinds, that the tumor is composed of a great number of similar elementary portions firmly joined together, so as to compose one mass.

This tumor possesses a firm texture, it is of a whitish colour, and it has little vascularity; it has a general coat or covering

of cellular membrane, from which it may be dissected, or pulled out, so as to shew as in this preparation, the separate portions of which it is composed; and on making a section of the tumor, it has a granulated appearance, which is here very distinct, from the number of packets which are cut across; and in some, which are softer than others, you can squeeze out the granular bodies from their cellular beds. They sometimes attain a considerable size, as in this case, where the tumor is six inches in diameter; but in other cases the growth of several years may not be larger than a marble.

When the tumor is tolerably large, it feels irregular, and lobulated, and hard; it is separate from the part in which it is embedded, but the cellular connexion is sometimes so strong, that you cannot trace its boundaries very accurately. When thus firmly bound down, it feels almost of a scirrhus hardness; but its more irregular surface is very different from that of a cancerous tumor. There is also no attachment to the skin, unless it be of very large size, and it is moveable upon the deeper parts; and it generally gives little pain or inconvenience, though this circumstance is not always the case; so that it sometimes requires a good deal of care to distinguish it from a cancerous tumor in its early stage. If the tumor is composed of smaller elements, it is smoother than usual on the surface, so as to make it in some situations impossible to be distinguished without dissection from the next, or conglobate tumor. It is a disease of middle age chiefly, not appearing to attack children or young persons, and seldom coming for the first time at an advanced age.

Mr. Abernethy supposes that this tumor originates in an absorbent gland; but it hardly seems to do so, and certainly is, in general, in parts where no absorbent gland is known to exist. Mr. Lawrence conjectures, with apparently more probability,

that the conglomerate or pancreatic tumor is a new structure, deriving its peculiar appearance from its being like the structure in which it is situated, just as a bone will give an osseous texture to a tumor upon or near it. If this conjecture be well founded, the conglomerate tumor would be nearly confined to the breast, in which it is most frequently met with; to the parotid and submaxillary glands, in which it occurs next in order of frequency; and to the orbit near the lachrymal gland, where it is occasionally, though not often observed. I am not certain, however, that the tumor is really confined to these situations; for instance, here are some tumors apparently of this nature, said to have been situated near the thyroid gland; but I do not know their exact history, nor whether they were high enough to be near the submaxillary gland, as well as the thyroid, to which latter structure they have no similarity.

Sir A. Cooper has called this kind of tumor, when situated in the breast, the chronic mammary tumor; but the term mammary tumor, you will recollect, as it is employed by Abernethy and other persons, means a form of growth like udder, rather than what we are now considering, and which occurs in any part of the body, and is, in fact, a kind of cancerous structure. Dr. Carswell also, in his admirable plates of morbid structures, places the mammary tumor of Sir A. Cooper, *i. e.* the conglomerate tumor now before us, among the forms of cancerous tumors, as if it were exactly the same as the udder-like tumor. I am myself, however, strongly inclined to believe, that the true conglomerate tumor is not in any respect malignant; I never heard of its returning in the breast, though it is very frequently removed from that situation, and not seldom from the neck also, with perfect success.

You will find in practice that the growth of this tumor, and the pain or inconvenience it occasions, are materially dependent on the general health, and consequently that you can relieve it, and check the rapidity of its increase, by alterative treatment. When painful, a few leeches may now and then be employed, with cold or tepid evaporating lotions; at other times a warm plaster over the tumor is of service; but the subject will be mentioned to you again with the diseases of the breast. When large, or when inconvenient and harassing to the patient, it may be removed with perfect safety, and without fear of a return; and the best method of operating, though somewhat more difficult, is to cut into the cyst which surrounds it, and tear and dissect it out of its bed, this plan occasioning less hæmorrhage, and affording consequently

more chance of union, than if dissection is made round the tumor. In the operations for the removal of these tumors from the neck, more difficulty is experienced, and more caution necessary, from the irregular processes which sometimes dip down among the important vessels of this region. Mr. Lawrence has published the particulars of one case, in which the tumor passed behind the pharynx, and between the external and internal carotid arteries, both of which were laid bare, while the portio dura was also divided, so as to cause permanent paralysis; and I mention this case to you as a great encouragement in even extensive tumors, since that gentleman saw the patient five years after the operation, at which time there had been no return of the disease, although the tumor had been of the extent described.

III. 5. *Conglobate tumor*.—As the last genus of sarcomatous tumors is connected with, or similar to, the conglomerate glands, so is the next either situated in, or it resembles, the conglobate or absorbent glands; and I will divide the genus of *conglobate tumors* into two species.

a. Simple conglobate tumor.—This tumor is chiefly found in middle life from the age of 35 to 50, at which time one or two bodies are found, round or oval in shape, firm, hard, or slightly elastic, in the situation of some of the absorbent glands. Not unfrequently it is the sequel of the common serofulous enlargement of early life, in which case, out of a number of glands somewhat larger than natural, but which have remained perfectly quiet for years, one or two will suddenly increase and become painful, still being smooth on the surface, but feeling harder and less yielding than they had before been. When cut the tumor shews a firm uniform structure, something like scirrhus in appearance, but still looking slightly glandular. This one was situated behind the mastoid muscle, and I assisted Mr. Keate in removing it from a lady nearly 40 years of age, in whom it had thus suddenly begun to increase; it shews the structure very well. In this preparation is a larger mass, by the side of the larynx and œsophagus, which had obstructed the circulation, so as to induce apoplectic symptoms. After a time, the tumor, having perhaps increased to the size of a cricket-ball, becomes attached to the skin; it ulcerates, and an unhealthy bleeding surface is exposed, which is hollowed out by sloughing and ulceration, while the edges are thickened, everted, and fungous. The health now suffers considerably from the irritation of the tumor; the patient becomes weak and emaciated, and dies with hectic fever, worn out by the pain and irritation and

bleeding of the tumor; or perhaps a temporary respite is afforded by the greater part of the diseased mass sloughing away, after which the health is restored for a few months, till the part which is left again increases.

I have said that the tumor looks like scirrhus, and its termination locally is like that of a malignant and cancerous sore; yet I am inclined to think that it is not ordinary cancer, not malignant in its effects upon the *system*, but only *locally* malignant in its influence upon the neighbouring structures. Certainly, at least, the patient has not commonly any scirrhus or malignant disease in other parts of the body, and certainly the tumor may very often be removed without any return of the disease, and that, too, even after ulceration has commenced; and it ought undoubtedly to be removed by operation as soon as its nature is ascertained, whether, on the one hand, I am correct in placing it thus by itself as a separate disease, or whether, on the other, it is only scirrhus modified by its originating primarily in an absorbent gland.

The situation in which the conglobate tumor is most frequently met with is in the submaxillary and parotid glands; and in the latter place it is bound down by fascia, so as generally to ulcerate early. Now you will find in surgical writers numerous descriptions of diseases of the parotid gland, for which that gland has been removed by operation. But the real fact is, that the salivary glands are very seldom altered in structure, so as to require removal; and the greater number of the tumors here met with are not enlargements of the parotid glands, but of the absorbent glands attached to them. In general the tumor is therefore on the surface of the parotid, or slightly covered by it; but after a time the pressure of the tumor may cause the absorption of the salivary gland, and the new structure will thus occupy its place. In many of the reports of cases in which the parotid gland is supposed to have been removed, there is no mention of paralysis of the face, nor of great hæmorrhage, nor of deep and intricate dissection, which almost always present themselves when the parotid itself is intimately connected with the diseased mass; and in many I have no doubt the operators have deceived themselves, and have removed a tumor, which has pushed aside the parotid, when they imagined they were dissecting out the salivary gland also.

Certainly no one should attempt to remove the parotid, or a tumor which has produced absorption of that gland, who is not conscious that he possesses considerable anatomical skill, and the boldness

which is derived from such knowledge, for it is undoubtedly a formidable operation. I have seen very good surgeons obliged to desist in the middle of the operation, from the impossibility of dissecting out the whole of the tumor, from its passing below the jaw, or behind the pharynx, with an intimate connexion with the important arteries, and vein, and nerves of the upper part of the neck. In other cases, on the contrary, the removal of a tumor from this part is an easy operation, and you have nothing to do but to dissect out the tumor from its bed in the parotid gland, without inflicting any injury upon the portio dura or the carotid arteries. Let me caution you, however, not to be deceived by the apparent mobility of this tumor, into a belief that it is superficial, when it is really very deeply situated; for it has occasionally a sort of hour-glass shape, and you can move one-half of it freely, while the rest dips down among the important parts below.

There would seem, however, to be several kinds of tumors in the situation of the parotid gland, besides that which we have just been considering. Sometimes there is a serous encephaloid tumor, sometimes an osteo-sarcomatous or an osteo-sarcomatous tumor; and, of the solid structures, you may find a fibrous tumor, or a conglomerate tumor, or a conglobate, or a serofibrous tumor. Now all of these may usually be removed from the parotid safely and with facility early in their growth; they may also be dissected out at a later period, but with more difficulty, from the processes arising irregularly from the chief part of the new growth, or from their connexion with blood-vessels and nerves; but still safely, if the whole of the disease is removed. Here, for instance, are some large tumors, which were not followed by any return of the disease after the operation. But with regard to the conglobate tumors, if the least portion of the disease be left, a new tumor is likely to appear in the seat of the operation, and the patient will die of ulceration and hæmorrhage; such was the sequel of the cases from which these two tumors were removed, although they were much smaller than the others which I before showed you. I have seen several, however, in which, on a narrow examination, it was evident that a little piece was cut off in the operation. Still, even in these cases in which the disease returns, I believe the malignant action is confined to the part; that it is local, and affects the textures in contact with the tumor, but does not contaminate the glands, nor the whole system.

But, finally, there are other tumors in the parotid gland, which are really malign-

nant in a higher sense; which are of the nature of cancer, or of fungus hæmatodes, melanosis or medullary structure, and in which consequently the disease will in all probability return, as usual, in the glands of the neck, or in other parts of the body, or in the internal viscera.

b. Tubercular Conglobate Tumor.

The second form of conglobate tumor is in some respects like the former, but is yet distinct from it in other particulars. I suppose it is to this disease that Mr. Abernethy gave the name of tubercular sarcoma, and as I wish as far as possible to speak to you in the same language as other persons, I will adopt in this arrangement the term *tubercular conglobate tumor*.

The chief difference in the appearance of this disease from the simple conglobate tumor, is, that it begins not as a single mass, but consists of a great number of round bodies, like swelled absorbent glands, more or less joined together; in fact, they much resemble common scrofulous enlarged glands at first. Here is a cast which I had taken from a boy under my care in the hospital, but from an accidental delay it only appears of half the size it had been a short time before. These numerous round bodies vary in size from a pea to a walnut or hen's egg, and are connected together by loose cellular texture, which inflames from time to time, with a temporary increase in the bulk of the separate round bodies, which then form one uniform mass of an irregular and *tuberculated* figure; but on the subsidence of the disease, the globular bodies can again be moved upon one another. These tubercles are of a white or light brown colour, as you may see in these preparations, and possess a firm, hard, and uniform texture, not in the least scrofulous in appearance, nor so soft and white as cerebriform tumors. When hardened into one mass, and increasing much, the skin over the tumor ulcerates, and a painful intractable sore, with fungous edges, is formed, with occasional hæmorrhage, and the patient dies of irritation, with the aspect of a person labouring under malignant disease. I have seen great irritation and emaciation before the ulcerative stage commences, and when situated in the lower part of the neck and mediastinum, irritation of the lungs is produced; or when around the trachea and œsophagus, ulceration may proceed into those tubes with the usual symptoms of such a connexion, as seems to have been the case in the patient from whom this large mass was taken; and in the abdomen, ascites will result from a similar formation.

The disease may, in fact, occur in any part, though the neck is its most common

situation; as in the glands of the axilla or groin, or along the aorta, in the chest or abdomen. But further, during the progress of the complaint little round bodies may make their appearance under the skin in various situations, where no absorbent glands are known to exist. In this boy a great number existed over the front of the sternum and below the clavicles, which disappeared under medicine, while the chief mass in the neck was materially lessened in bulk. I have not usually seen the patients suffer much pain either in the chief tumor, nor in these separate globular tubercles (which, by the by, are not frequent); but Mr. Abernethy, under this name, describes one case, where the little tumors, which were numerous, were so exceedingly sensitive, that the patient, in describing the pain he experienced, employed the expressive simile of his lying on hob-nails. It would appear, finally, that the disease may exist at any age from ten to fifty, but the few I have seen have been in lads from sixteen to twenty years of age. I have recently met with an observation of Dr. Hodgkin's, in the 17th vol. of the Med.-Chir. Transactions, in which he mentions several cases of apparently this disease, and remarks that in every case except one he has found a number of little bodies of a cyst-like form in the spleen. I have not myself had an opportunity of verifying this observation since I noticed his paper, nor do I know (if it be well founded) whether the coincidence is of importance or not.

As to the nature of tubercular conglobate tumor, Dr. Hodgkin quotes a case from Dr. Carswell's valuable work, in which it is called cerebriform disease of the absorbent glands. I agree with Dr. Hodgkin, however, in thinking it a different disease to a certain extent; the tumors are harder and firmer than cerebriform formations, and they run a somewhat different course. I have, indeed, seen cases of fungoid disease of the cerebriform variety, which resembled the disease in question. Mr. Keate, for instance, had an elderly man under his care in the hospital with a great number of superficial tumors on the head and face and other parts of the body, but they affected the skin more intimately, and were whiter and softer than the superficial subcutaneous tumors of the tubercular conglobate tumor; and in this case the glandular mass in the neck had occurred secondarily by absorption from some of the other tumors, and there were tumors of fungous character in other parts of the body.

Still I am inclined to look upon it as to a certain extent a modification of malignant disease, if there is not confusion in the accounts related of cases of

the disease. As I have myself seen it, indeed, it appears to be a peculiar local disease of the absorbent glands, (and I have therefore placed it in this genus of tumors), confined generally to the glands, though, as we have seen, accompanied sometimes by small cutaneous tumors where no glands are known to exist. I have already observed, however, that Dr. Carswell does not distinguish it from cerebriform disease, and Mr. Abernethy describes a case in which a tumor supposed by him to be of this nature in the axilla was succeeded by tubercles in the lungs, heart, liver, omentum, and other parts. Now if this be so, we must evidently regard it as only a modification of cerebriform fungous disease, in which the whole body is less affected than is usually the case with a fungous tumor of that or any other sort.

In the treatment, however, of the disease there is sufficient to demonstrate at the least a very considerable modification of malignant disease, since it is sometimes very much influenced by medicine, which cannot often be said of fungous diseases. As it occurs in lads, and looks like serofulous enlargement, it will at least be right to try some remedies, before having recourse to operation. I have told you that in the lad of whom the case is before you, the tumors in the neck had become lessened to half the former size, and the separate cutaneous tubercles disappeared; when I saw him some months afterwards, however, the disease was again increasing, and his health a good deal affected, and I dare say the disease proved ultimately fatal. Now in this lad what seemed to do most good (when a great variety of means were tried), was the occasional employment of a few leeches, with collutions, whenever the tumor was attacked by the temporary inflammation I have alluded to (at which time, besides the pain in the tumor, he suffered much from pain in the head, as if the circulation in the brain was interfered with by the pressure of the tumor on the jugular veins,) while he took internally, during a long time, small doses of blue pill with sarsaparilla. Mercury, however, must not be given so as to affect the gums, for I have observed, that whenever the action of the medicine was greater than that of an alterative, the disease became worse, and I have been obliged to intermit its use. Some good was done at this time in this lad by liquor potassæ, of which he took as much as ʒij. three times a day. Some good is occasionally done by iodine, but not so much, I think, as by the other means, and sometimes iodine does harm, probably when there is a disposition to inflammation. This patient had been the servant of a medical man, who informed me that he had employed a tincture with

double the usual quantity of iodine, and that he had at least given as much as ʒij. of this strong tincture twice daily, while you know that five drops will be too much for some persons.

It only remains for me to speak of an operation, when your medicines fail, in checking the growth of the disease. Now in general this is useless; the disease is a constitutional malady, and while you remove some of the tubercular masses, others will continue to form. I had a consultation on this lad for instance, but both sides of the neck being immensely large, while tubercles existed elsewhere, it was considered out of the question. Where, however, the diseased mass is more local, and is troublesome, and the patient is desirous of an operation, it may be performed, though not always completely. In this preparation you may see a great number of the tumors which I assisted Sir Benjamin Brodie in removing from the upper part of the neck of a boy in this hospital; many were drawn out from their cellular beds, exposing the carotid artery and jugular vein; but even then, many small glands or tubercles were obliged to be left. In this case, however, I understand the tumors had not returned for a year or two after the operation; but in many other cases the removal of all the glands and tubercles then enlarged, has not prevented the subsequent formation of many others, and the death of the patient from ulceration or irritation.

III. 6. The next genus of sarcomatous tumors may be called *Serofulous Tumors*. Serofulous or cheesy matter is a secretion from the vessels of the body in a particular state of constitution, and may be deposited in various situations, and in several ways. You meet with it in the internal viscera, in the form of tubercles, cyst like in appearance—as in this preparation of the liver—the serofulous matter being a mere deposit from the cyst, which Dr. Carswell has demonstrated to be frequently at least an enlarged biliary duct in that organ, or an air-vesicle in the lungs; you find it, also, in the cellular structure below the skin, or in the cancelli of the bones, or in the absorbent glands, with or without swelling of the affected part; and thirdly, but more rarely, it is deposited more universally in the structure of any part, so as to constitute not merely an inflamed swelling with some serofulous matter, but a distinct chronic tumor, consisting in part of the unorganized matter in the cellular tissue of any organ, which tumor is sometimes of considerable size.

It is only in the latter form that it can be called a tumor; and you may here see such masses in the thyroid gland, the brain, (where it causes epilepsy and compression)

and in the testis and breast; and it occurs in other parts also. The serofulous tumor in any organ is a soft, elastic, globular, or oval mass, semifluid occasionally in feeling, though generally firmer in consistence, resembling a fatty tumor in this respect, but deeper and more globular; and often not single, but two or three are at once felt in the same part. Such tumors may be of the size of a nut or walnut, or small apple, giving very little pain, and possessing very little tenderness; nor have they much disposition to suppurate, unless the serofulous tendency be very strong, so that years may elapse without any abscess being formed.

Serofulous tumors are very much under the control of medicine, as you might expect, so that I have seen very large ones disperse when in the breast, or thyroid gland, or when the absorbent glands have been thus enlarged so as to form a large solid mass in the neck or axilla. The most useful remedies are, alternative purgatives, such as the hydr. c. cretâ, and rhubarb, with alkaline medicine, especially the liquor potassæ in large doses, continued for a considerable time. With potash you may combine bitter medicines, or sarsaparilla sometimes; or the potash may be given at the same time with small doses of the tincture of iodine, or hydriodate of potassa. I shall not dwell, however, upon the medicinal treatment at present, as I have recently spoken to you of the cure of serofula in whatever way it may be developed in the body. Locally, I think a mercurial or ammoniacum plaster alone will be of most service, or a stimulant lotion of salt solution, or the hydriodate of potassa ointment, or the strong solution of iodine in a solution of the hydriodate, applied daily with a camel's-hair pencil. Constitutional remedies have more influence, however, if judiciously employed, than any local application.

III. 7. We next proceed to the *Cystic Tumor*, which is composed of a vast number of cysts, united by more or less cellular substance, and varying in size from that of a pin's head to a pea or marble, or in the ovary to a greater size still. The cysts are vascular, and separate from one another, as you may see in these preparations, and secrete various fluids; generally thin and transparent mucilaginous water, like the fluids of the serous encysted tumors; sometimes opaque and dark brown, or green, or almost black; occasionally, a few cells of cheesy matter are mixed with the rest, and sometimes the contents are semi-purulent.

In speaking of this tumor in the breast, Sir Astley Cooper calls it the cellular hydatid disease; but, as you see, the cysts

are not hydatids, but are vascular, and highly organized; and if a few hydatids are found in some of the cysts, this is merely a coincident circumstance, just as you occasionally find these animals in fungus hæmatodes and other tumors.

Sir Astley attributes the formation of the tumors to the obstruction of ducts; this will account for their formation in the breast or testis where the tumor is sometimes met with, or in the kidney, as in this preparation and plate, where a vast number of watery cysts are seen with hardly any of the natural structure remaining. But it will not explain the growth of the tumor in the ovarium, where it is most frequent, and the cysts are, in this situation, a great deal too numerous to be easily attributed to enlargement of the Graafian vesicles. But here you will see a tumor of apparently this texture in the situation of a fractured and ununited rib: and Dr. McFarlane has met with such a tumor between the peritoneum and the abdominal muscles. Just, then, as we have already seen with regard to a single serous or aqueous encysted tumor, so with this cystic tumor also, if, as is no doubt the case, cysts of fluid arise from obstruction of the ducts of secreting organs, yet, if it is formed in simple cellular structure, where no ducts exist, we must at least look to more than one cause for its origin; and in the ovary, where the cellular structure is so tough, enlargement of these cells will scarcely arise very easily. But, after all, I cannot explain to you why in a certain individual all the ducts of the breast or testis, or all the cells of the cellular membrane in a given part of the body, and in that only, should be obstructed and enlarged, and secrete peculiar materials.

Examine the cystic tumor and you will find it soft, elastic, with obscure fluctuation, the fluid not being so evident as in simple cysts, from the numerous partitions of the tumors. It is not very heavy when poised in the hand, as a completely solid tumor of fungous disease would be. It is of slow growth, several years elapsing before it often attains a large size; and it has little disposition to inflame, or to adhere to the skin, or to ulcerate. It sometimes, however, has one cell after another in a state of suppuration, and bursting, as a small abscess, without the inflammation spreading to the mass of the tumor. This is a part of the cystic tumor which grew in the breast, and must have weighed several pounds; and yet it presents no mark of much diseased action. Sometimes enlarged veins are seen on the surface, as in malignant disease; but this may be observed in any tumor, in which the passage of the blood through it is retarded, without being necessarily a sign of

malignant action. In the ovarium the tumor grows to an enormous size. I have seen one of seventy pounds weight, which was tapped in three places, and the fluid of as many of the largest cysts evacuated, which immediately became as solid as jelly or glue.

Cravillier, in speaking of this tumor, I believe, in the ovarium, calls it the same disease as what has been termed *cancer aréolaire* or *gelatiniforme*, which disease I show you here in the stomach: even he, however, calls the cystic tumor a local disease, only affecting the neighbouring textures by continuity. For my own part, however, I have never seen any evidence of even the largest (not even the one I have just mentioned of seventy pounds weight,) which seemed to affect any part besides the organ attacked; and I am not inclined to attribute any malignant action at all to this disease, not even the power of contaminating the parts around; believing that it is not the same affection as the gelatiniform cancer.

Whenever, then, a part of the body is affected with this tumor, and is becoming inconvenient from its size and weight, you may remove it, I believe, if accessible, with perfect safety, and without fear of an unsuccessful result from any constitutional taint. I say little of any thing but removal, since local remedies have little influence, even upon the fluid part, and none whatever, I believe, upon the solid portion of it. You may try, however, some of the stimulant lotions I recommended for the single cysts, the muriate of ammonia, or the camphor spirit and goulard, and so on, or iodine applications. You may also try internal remedies for a time, at least early in the disease, such as potassa and iodine.

III. 8. The next genus of sarcomatous tumors on our list is more rare than the last, and I have been accustomed to call it, for distinction, the *tuberos cystic tumor*; and I scarcely know whether it should be placed by itself as a separate genus, or only considered as a species of the last. It consists, as you perceive, of a large cyst, which generally contains a yellowish or brownish fluid, into the interior of which cyst there project a number of bodies, like a bunch of grapes sometimes, of various sizes and shapes. These tubera look like portions of the cystic tumor, and are soft, of a white or yellow colour, and a section of them shows that they are also cells with a similar fluid or semifluid jelly inclosed in a membrane similar to that of the large cyst which surrounds the whole. In these preparations from the breast there seems little difference, except in the form and connexions of these portions, from the cystic disease, and both are not

very unfrequently met with in that organ. In the ovarium, the tuberos cystic tumor is not uncommon, of which this is a good example. Here is also a beautiful drawing, from Dr. Seymour's excellent work on the Ovaria (to which I may refer you for a detail both of this and of the cystic tumor, as they present themselves in that organ, which shows you the recent appearance of the same tumor, in a case which I have cause to remember, as it cost me three months' illness from a wound, after the death of the lady. There was a large quantity of fluid, which I drew off several times by tapping, and the section of the tuberos mass bears some resemblance to the areolar cancer of the mucous membranes. The tumor was remarkable before it contained much fluid, from the contradictory opinions given with regard to it by different medical men, whether it was the ovarium, or spleen, or liver, or some new structure altogether.

In speaking of this disease in the breast, Sir A. Cooper calls it a form of hydatid disease, and expresses his opinion that hydatids, having been subjected to pressure, have been killed, and subsequently become attached to the cyst around, into which they hang like polypi; there is not, however, the smallest evidence of the hydatid origin of the disease, nor, indeed, of hydatids in any part ever becoming attached when deprived of their independent vitality.

I cannot, then, explain the peculiarity of the appearance of this tumor; but I may add, that it would appear as if such pendulous bodies as are here met with did really become sometimes strangulated, as it were, so that they are bloody and soft, as if mortified, with coagulium in their substance.

The appearance of the tuberos cystic tumor in the living body is that of a generally smooth surface, but sometimes of more irregular figure than the cystic tumor; it has a sensation of more distinct fluctuation, but this will depend on the relative quantity of fluid; and when it is pressed deeply, it is generally easy to feel the mixture of solid matter presented by the projecting tubera. The skin is dark-coloured and purple, over a large space sometimes, and is more frequently attached to this than to the cystic tumor, and the enlarged and tortuous veins on the surface are more evident. In the several particulars I have just mentioned, the tumor bears much resemblance to one of fungus hæmatodes, nor do I know any positive distinction between the two before removal, in all cases. Here is one which Mr. Babington removed from the breast, which was extremely dark coloured, but from the perfect health of the patient, with some

trifling circumstances in the part, I believe we all formed a correct diagnosis before the operation, which was confirmed by the dissection and by the result; at least there was no evidence of any return three years afterwards. In the ovary you can often distinguish the tuberos cystic tumor from the aqueous eneysted tumor, and from the cystic tumors, by the irregular solid masses which you can feel through the parietes in a thin person, and which sometimes have hardly afforded me room to tap the patient. In the lady from whom this was taken, by the time of the fifth operation, hardly any space was left, as the tubera increased in size and number between the intervals of theappings.

There is no cure, I believe, for this disease; the quantity of fluid may be made to vary, but the whole tumor should, if possible, be removed, and the operation, as for the cystic tumor, may be undertaken with a fair prospect of success: at least I have not seen any more evidence of malignancy than in cases of the cystic tumor. I have mentioned, in this case from the breast, that the patient was quite well three years after removal: in the case from which this larger tumor, with more solid matter, was removed by Sir Benjamin Brodie, from the same part, a longer time, I believe, elapsed; and Sir Benjamin informs me (and Sir Astley Cooper says the same, in his work on the Breast), that the tumor has not returned, nor affected the system, in several cases where these gentlemen have operated on large tumors of this kind. It is true, indeed, that in the ovary, where it grows to so great a bulk, the patient suffers great irritation, and becomes emaciated, and dies worn out by her disease much earlier than is usual with other forms of ovarian cysts, and with a countenance sallow and sunk, as if with malignant disease; and it is thought by some for whose opinion I have a high regard that it is a malignant form of structure. But though I have seen cases of malignant disease, with cysts too, in the ovary, yet, even when the patient has died of the tuberos cystic tumor, with what has been regarded as the appearance of the system being impregnated with the poison of malignant disease, I have not seen it evinced in any tumor of the glands communicating with the ovary, nor in other parts of the body; and sometimes, when it is of great size even in this internal situation, the irritation is much less than usual. I tapped a woman, for instance, about fifteen times in nearly three years, taking away as much as 74 pints of fluid at a time, and yet, but for an accidental attack of erysipelas and inflammation of the sac, which had numerous tubera within the cyst, the patient

appeared likely to have lived some time longer.

I have now mentioned to you a great number of tumors, of which one or more cysts form the whole, or a part:—sebaceous cysts, serous cysts, congenital cysts, hydatid cysts; 2dly, osteo-steatomatous tumors; and 3dly (of the sarcomatous tumors), the cystic and tuberos cystic;—in all of which we have found that the disease is innocent, or rather not malignant. But further, cysts are often found connected with malignant disease, either in the same situation, or in some other part of the body; and tumors with such a combination especially resemble the two last genera of tumors. Here is a testis which I removed by operation, with a great number of cysts in its substance, and in many respects like a cystic tumor, but it is, on the contrary, a malignant tumor, and returned in the glands in the abdomen. Here, again, is a tumor like a case of tuberos cystic tumor, which I punctured in the neck, and dissected out after death; but which also followed fungus hæmatodes of the testis, like the other specimen, and was accompanied moreover by a large fungous tumor in the abdomen.

In the living body I do not think you can always distinguish one case from the other, where there are no constitutional symptoms to guide you, and no tumors elsewhere to indicate the malignancy of the disease. This is of less consequence, however, since wherever a tumor contains many cysts, or even a single cyst is joined to much solid matter, you must remove it, whether it be malignant or not; and having examined it after the operation, you can form, at least, a probable opinion of its nature, and give your prognosis accordingly to the patient and his friends. If, for instance, a tumor contains a number of cysts, the contents of which are the ordinary secretions of such cysts—watery, gelatinous, cheesy, and so on; and what solid matter there is in the substance of the cysts, and in the cellular membrane between them, appears to be the ordinary deposits of inflammation, you may assure your patient, with tolerable confidence, that he is safe from any return of disease; if, on the other hand, the secretions and the solid deposits have any signs of the various malignant deposits, cerebriform, melanotic, and so on—even if the solid materials bear a very small proportion to the cysts, yet you may feel assured of the malignant nature of the tumor; the existence of the cysts, in such a case, is an accidental circumstance only, that does not lessen the chance of the tumor returning. But further, when the solid matter bears a large proportion to the cysts, even

if you cannot quite satisfy yourselves of the exact nature of such deposits, yet you should, I think, always regard the combination with suspicion. And lastly, if a tumor containing cysts is growing rapidly, and is accompanied with severe constitutional disturbance, such as irritative fever, you will learn to anticipate the malignant nature of the tumor: for instance, a woman came under my care, with a tumor of only nine weeks' duration, in one mamma, but which was already two feet in circumference. I punctured several large cysts, with great relief to her sufferings, and felt the solid matter between them; but the tumor, growing thus rapidly, had caused so much fever, that, on consultation, its removal was not considered advisable. The circumstances I have mentioned led us to believe the tumor malignant; and when she died with fever and extensive gangrene of the tumor, three weeks afterwards, its solid substance was distinctly cerebriiform and hæmatoid, mixed with the large cysts, and the same disease was just commencing in the axillary glands.

The numerous circumstances in which cysts are connected with morbid growths, have led to the formation of numerous theories with regard to them. One of these theories is, that all tumors whatever owe their existence to the previous formation of cysts; and some theorists, going still further, affirm that these cysts are hydatids. Mr. Carmichael, for instance, proposes the cure of cancer by the use of carbonate of iron, to kill the hydatids of the disease, as that substance destroys the intestinal worms. I need say no more, however, upon this subject.

One of the most ingenious theories in connexion with tumors is afforded by Dr. Hodgkin, in the fifteenth volume of the *Medico-Chirurgical Transactions*. This gentleman, like the pathologists I have alluded to, attributes the structure of all tumors, even of solid scirrhus tumors, to the previous formation of cysts. This diagram, and the tuberos cystic tumors, will explain his theory to you. It is, that a cyst having been formed, a second is formed thus between the layers of the cyst, and projects into its interior; then a third between the layers of the second, and so on *ad infinitum*, so as to constitute a large mass. Now this theory would undoubtedly seem to be true as to this particular form of tumor (the tuberos cystic), to a certain degree; but he goes on to explain the solid structure of hard tumors in the same way. There is in them no fluid; but the secondary and tertiary cysts having been developed, are, according to the theory, pressed together and joined, so

that, on a section, the white lines and bands of solid substance then exhibited, especially in cancer, are the successively formed cysts, pressed together, which in this manner constitute the tumor. Now this theory does not appear to me to be at all well founded, as far as I have tried to unravel the tumors in question; nor is Dr. Hodgkin's opinion adopted, I believe, by any pathologist who has examined the subject.

ON THE

INTRODUCTION OF AIR INTO THE VEINS.

BY M. VELPEAU.

THE numerous incidental questions which have been raised on occasion of the discussion concerning the introduction of air into the veins, have made the original point be lost sight of. It was desired to know if air can spontaneously enter the human veins during surgical operations; if science possesses facts tending to establish the reality of this accident; and lastly, whether art has any method calculated to prevent or to remedy it: such were the points first debated. In spite of my efforts to keep the discussion within its proper boundaries, it left them almost at the outset, never more to return within them. As I shall not be able to treat the subject again, I think it will be useful to my colleagues if I publish in detail my researches on the direct question of the introduction of air into the human veins.

Surgical practice had long afforded examples of almost sudden death taking place during certain operations; but these occurrences had been attributed at one time to hæmorrhage; at another to the exhaustion of the patients by excess of pain; at other times to terror, and sometimes to syncope. I know myself that patients have died in a few minutes, while the operator was endeavouring to remove a diseased thyroid gland, or a tumor in the axilla; or, to give another instance, that the accident has taken place during tracheotomy, and that no explanation has been attempted except those which I have just given. Modern surgeons, however, not satisfied by these explanations, and recollecting the experiments of physiologists upon animals, have explained such cases by supposing that air entered the veins. The facts of this kind which I have met with are about forty in number. The question is, whether they can be really compared with those which are furnished by experiments on animals, or whether, in any other manner, they contain the proof

of the fact in favour of which they are quoted. I consequently think that it will be useful to review them, and give an abridged analysis of them. I shall take care to point out the work where they are to be found, that the reader may refer to all the details, if he thinks proper.

CASE I.—The first instance of the kind which was published in France, occurred in the practice of Bauchéne, at St. Antoine's Hospital, in July 1818 (*Piedagnel, Thèse*, No. 250, Paris, 1827). He was removing a large tumor on the right shoulder of a young man of 23; but little blood had flowed, and the clavicle had been turned outwards, when M. Piedagnel heard a noise like that which is caused by the entrance of the air through a small hole into the chest of a living animal. The patient cried out, "My blood is falling into my heart; I am dead!" The same sound was heard a second time, and the young man fainted, and died a quarter of an hour after the operation, which, moreover, had been long. The body was opened eighteen hours after death, and it was found that the external jugular vein had been wounded; *there was no blood in the heart*; its right cavities were flaccid and thin; all the vessels contained a considerable quantity of air-bubbles.

Here the external jugular vein was wounded. There was a hissing noise, followed by syncope, but no mention is made of convulsive movements; and there was air every where, except in the heart. Experiments made on living animals show precisely the reverse; in them the heart contains air more than any other part, and when air enters the veins, animals do not sink in this manner.

CASE II.—Before the fact just related had drawn public attention, another occurred at the Hôtel-Dieu. On the 19th of November, 1822, Dupuytren had to extirpate a large tumor from the right supra-clavicular region of a young girl, aged about twenty (*Arch. Gén. de Méd.* t. v. p. 430). During the dissection of this tumor, and before it was completely separated, a hissing noise was heard, as if air was entering a pneumatic apparatus. The patient, who had lost only a few drops of blood, exclaimed, "I am dead," sank upon her chair, and expired instantly.

The body was opened twenty-four hours after death. The right auricle, which was tense and elastic, was filled with air, but contained no blood; fluid blood, however, was found in the other cavities of the heart. All the vessels in the rest of the body contained a large quantity of air, mixed with a certain proportion of blood.

It cannot be denied that a fact like this has something strange about it; nevertheless, if we compare it with what happens

in experiments upon animals, we see immediately that the cases are dissimilar. In fact, there is nothing to show that the internal jugular, or even the external jugular vein, was opened during the operation; which, besides, was not finished. There was air alone in the auricle, and blood alone in the ventricle. But we have seen in animals that there was always a close union of blood and air, in the form of a bright-red froth, both in the auricle and the ventricle. In the case just detailed, death occurred suddenly, and without convulsions; but it never occurred in this manner in our experiments. Hence the case leaves much to be desired, even taking it as it is narrated.

CASE III.—At the same period it was said that a similar accident had happened to Gräfe, of Berlin; but I cannot find the particulars of this case any where, and believe it to be a mere *on dit*; unless, indeed, the name of Gräfe was confounded with the journal which he edits, and a case by Klein was really meant.

CASE IV.—We find, indeed, in Gräfe and Walther's journal (vol. i. p. 120), that Klein, of Stuttgart, while extirpating the thyroid gland in a deaf and dumb child, lost his patient in less than a minute. But in 1814, when this occurred, the danger of the introduction of air into the veins had not been thought of; and it was not till long afterwards that this case was referred to, in order to support Dupuytren's.

CASE V.—Dupuytren also quoted an instance of the same kind, as occurring in Sir Astley Cooper's practice; but nothing of the kind is to be found in his works, and M. Olivier, of Angers, alone has thought it necessary, on the strength of an *on dit*, to mention the case briefly, in the *Dictionnaire de Médecine*, tom. ii. p. 70.

I think I need not add, that, in so important a question, evidence of this kind is of no value.

CASE VI.—In 1826, M. Castara, a surgeon of Lunéville*, while dissecting out a tumor situated in the sub spinal fossa of the right shoulder, suddenly heard, at the bottom of the wound, a sound like the gurgling of a narrow-necked bottle which is being emptied (this sound is called, in French, *glouglou*). The patient, who was twenty-one years of age, fainted, and died suddenly, without any convulsive movement. Twenty-four hours after death, the right cavities of the heart were found filled with fluid blood, mixed with a great quantity of air-bubbles. The left cavities also contained some. The whole venous system of the right arm (including the fore-arm) was filled in the same way. The only vein which had been opened was a

* Sauerotte, *Thèses de Strasbourg*, March 1828.

branch of the sub-scapular, and the incision in it was less than a line in diameter.

There are some very remarkable peculiarities in this case; it is the only one of those we have hitherto narrated which can be compared with the results of experiments upon animals, with regard to the state of the blood in the heart. But the patient died suddenly; and this never occurs in direct experiments. So small an opening, and that too in a vein beyond the axillary, does not allow of the entrance of air enough to cause real danger, either in dogs or horses; nor can one understand how the air should be present in the veins of the right fore-arm, when none was found either in the vena cava inferior, or in the left arm. Hence M. Castana's case, though one of the most authentic, leaves some doubts in one's mind.

CASE VII.—Two years afterwards, in 1828, Mr. Mott, a celebrated surgeon of New York, who is now in Paris, published an account of the extirpation of a tumor, which extended from the parotid region to the face (*Journal of Surgical and Medical Science*, Nov. 1828, p. 127.) During the operation the facial vein was opened; a peculiar sound was heard; the patient uttered a cry of distress, and nearly fainted; but death did not take place. Hence there is nothing to shew that the symptoms were caused by air entering the veins, especially as the facial vein does not seem capable, anatomically, of allowing it.

CASE VIII.—Delpech, while extirpating the arm for a fungous tumor in a patient of about 30, heard two snuffling sounds (*rouffemens*) before he had got through the joint. The patient fainted twice, and died instantly. There had been but little hæmorrhage. The dead body was placed under water, and a considerable quantity of air-bubbles were found in the right cavities of the heart (*Mémorial des Hôpitaux du Midi, deuxième année*, pp. 231, 634.)

Here, again, we are immediately stopped by a number of serious difficulties. First of all, it is neither the axillary nor the subclavian vein which is in question, but merely the veins of the stump of the humerus; and we have already seen* that, at this distance from the thorax, the absorption of air appears to be impossible. Then it is not a snuffling sound which is heard in animals, nor do they sink suddenly; moreover, we do not learn what was the state of the air which filled the right cavities alone of the heart.

M. Clémot, of Rochefort, happening to be at the Hôtel-Dieu, told Dupuytren (who

had just been attempting to remove a tumor from the thyroid gland), the three following cases:—

CASE IX.—A woman, from whom he had removed a cancerous breast weighing twelve pounds, died a few hours after the operation; and air was found in the veins running from the wound to the heart, as well as in its right cavities (*Putegnat, Thèse*, No. 156; Paris, 1831.)

CASE X.—In another instance M. Clémot, while dissecting a tumor out of the axilla, suddenly heard a respiratory sound. It was thought that the chest had been opened; the patient fainted, but soon came to himself again.

Is it not clear that no one can be convinced by facts so vaguely set forth, and which, in addition, have not been published by their author?

CASE XI.—In the third case attributed to M. Clémot, the subclavian artery was tied. A small vein had been opened, and a respiratory sound was heard; but the sound was stopped by putting a finger on the vein, and it was thus allowed to reappear, and made to cease, several times. No bad symptom followed (*Gaz. des Hôpitaux*, tom. iv. p. 95; *Putegnat*, in the thesis above quoted, p. 38.)

Mr. Warren, a distinguished surgeon of Boston, published two cases in the American journals in 1832, and has again related them in an interesting work which he has lately published (*Surgical Observations on Tumors*, 1837.) They are to be found also in the *Gaz. Méd.* for 1833, p. 226, and in the *Arch. Gén. de Méd.* t. xxxi. p. 119. To me, however, they seem to have been admitted as evidence in the present discussion without sufficient reason.

CASE XII.—W. Buril, aged 60, had a cancerous swelling in the right parotid region and on the face. In laying bare the carotid artery, which he wished to tie, Mr. Warren opened a vein, on which he heard a sound like that of air-bubbles passing through water. The patient said he felt ill, and apoplectic symptoms followed. The temporal artery was opened, and the symptoms began to go off at the end of two hours. The next morning they had entirely disappeared (*On Tumors*, &c. p. 298.)

CASE XIII.—While dissecting a tumor out of the axilla of a patient named Nancy Barker, Mr. Warren heard an indistinct bubbling or clucking sound; the patient became insensible, and her respiration was apoplectic. In spite of stimulants, the introduction of brandy into the larynx, tracheotomy, and blowing air into the lungs, the patient died in a few hours. Post-mortem examination was not allowed (*op. cit.* p. 259.)

If I am not mistaken, these cases are

* That is to say, in the experiments on animals.
—TRANSLATOR'S NOTE.

not of a kind to satisfy all the demands of the critical. In the first one, we see hardly any of the characteristic symptoms which indicate the entrance of air into the veins of animals; and the patient did not die. In the second, only a small subscapular vein was opened, almost an inch distant from the trunk of the axillary. Moreover, those apoplectic symptoms lasting several hours in a patient who remained insensible, in whom the temporal artery was opened, who was exposed to the vapours of ammonia, into whose bronchi alcohol was introduced, and on whom laryngotomy was performed, have but little resemblance, as it seems to me, with the results of our experiments. All that one can say is, that the veins opened in these two patients were in a part where the entrance of air is, in truth, possible.

CASE XIV.—A case mentioned by MM. Putegnat and Guérétin, in their theses, and attributed to Mr. Hodge, or Lodge (*Gaz. Méd.* 1833, p. 27), seems stranger than any which I have hitherto cited. If we were to believe this physician, a patient died suddenly from the entrance of air into his veins, at the very instant that Dupuytren had opened the internal saphena!

This case, however, which nobody knew of at Paris, and which has come back to us from America, where it was communicated to Mr. Warren by the author, is too improbable to deserve discussion.

CASE XV.—M. Puydebat (*Gaz. Méd.* 1833, p. 498), and M. Forget (*Transact. Méd.* t. x, p. 75), have published the case of a young woman, aged 18, who was operated upon by M. Roux in 1832, for a lymphatic tumor in the parotid region. While removing the tumor a hissing noise was heard; the patient immediately cried out, and was violently agitated. A fainting fit followed, but did not last long. The patient went on well for several days, and did not die till the end of a week. Air-bubbles were found in all the vessels!

Although it is true that some of the symptoms which are produced by the entrance of air into the venous system occurred in this instance, still it is impossible to consider the thing as proved, and not rather to believe that it was merely a case of syncope. In the first place, we do not know what vein was opened; and as death did not take place till seven days had elapsed, it is unquestionable that it did not arise from the air which might have reached the heart during the operation.

CASE XVI.—In the *Gaz. Méd.* of 1833, and in the thesis of M. Putegnat, the following case is to be found, communicated by M. Duplat. M. Goulard wishing to remove a cancer which extended from the

breast to the axilla, injured a vein which he believed to be the axillary, from which, however, but little blood flowed. At the same instant the patient was seized with convulsive movements of the face, and died. The body was not examined.

I would not maintain, certainly, that the death of this woman was not caused by air entering the veins; but I cannot help saying that no proof of this is to be found in M. Duplat's case. When the particulars are so vague, it is impossible that a case can prove any thing.

CASE XVII.—I will say the same of a case taken from M. Ulrich (*Journ. des Conn. Méd.-Chir.* tom. ij. p. 91, or in the *Berlin Med. Gaz.*) In extirpating a tumor from the neck, the internal jugular vein was opened. A hissing sound was heard, and death ensued in a minute. The right auricle was distended with air, and contained no blood. There was black and fluid blood, but no air, in the corresponding ventricle.

Now, it is certain that in animals death never ensues in a minute; that the auricle contains both blood and air; that it is the same with the ventricle; and that, far from being black, the blood is always of a very bright red.

CASE XVIII.—The same remark may be applied, with still more force, to Mr. Barlow's cases. While removing a tumor from the cheek, he divided some varicose veins; a fainting fit occurred, which he attributed to the entrance of air into the veins, though the patient soon came to herself again.

CASE XIX.—In the other case, Mr. Barlow was extirpating a tumor from a woman's neck, and, while dissecting the skin, he heard a hissing, and a gurgling; the patient died suddenly, without sighing or convulsions. The body was not examined.

Thus, in one instance, it was in the cheek, and experiments show us that in this part the veins do not absorb air; in the other instance, the vein opened could only be the external jugular at most; death was sudden, and without convulsions; and yet it is never so in animals! Moreover, the absence of cadaveric examination makes the case quite incomplete.

CASE XX.—A case published by M. Rigaud (*Quelques faits de Pratique Chirurg.*: Paris, 1836) is less known than the preceding ones.

In exposing the subclavian artery to cure an aneurism in the axilla, he was enlarging the external incision, when he opened a vein which he believed to be the external jugular. A peculiar sound, a sort of respiration, was heard three times. Nevertheless, M. Rigaud, who was much alarmed (from recollecting Dupuytren's

case) found that no bad symptom supervened. The patient died six weeks afterwards, from causes which had nothing to do with the entrance of air into the veins.

We have evidence here of the existence of the sound, of the exit of air-bubbles from the ends of the divided vein, of a sort of bubbling at the bottom of the wound, and yet no particular symptom appeared affecting the visceral functions! We are, therefore, in some measure forced to conclude either that M. Rigaud was deceived, or that the introduction of a large quantity of air into the veins is not always dangerous.

CASE XXI.—In passing to the examination of the case which, next to that of Dupuytren, has most excited public attention, I mean M. Roux's, I meet with a mixture of circumstances amidst which it is difficult to find conviction.

A man of good constitution was admitted into the Hôtel Dieu for a burn, which affected nearly the whole right side of his head, a great part of the hip and of the thigh of the same side, and which had, so to say, sphacelated the whole of the arm up to a short distance below the shoulder. After having first refused to allow the disarticulation of the arm, he consented to it four days afterwards, namely, on the 30th of April, 1836, and was carried to the operating-room in the beginning of the period of re-action. M. Roux had hardly cut the posterior flap in the deltoid, when he perceived that the patient was pale, and seemed to be going to faint. The capsule was speedily opened; an assistant compressed the axillary vessels, and the disarticulation was finished by cutting the internal or anterior flap, after Desault's manner. But the patient had given no sign of life, and it was impossible to revive his circulation. Some of the bystanders afterwards said that they *thought* they had heard a sort of noise, a hissing. On examination, gases were found in the vessels and the heart.—*Journ. des Conn. Méd. Chirurg.*, t. iv. p. 108. *Revue Méd.*, 1836, t. ii. p. 417.

One cannot help remarking, that in this case a crowd of data are wanting, and that it does not fulfil the conditions mentioned in the account of the direct experiments relating to the entrance of air into the veins. It is not very certain that a sound was heard; and the opened veins were at most but scapular branches of the internal jugular. Death was sudden, without premonitory symptoms, and without convulsions. There was no frothy fluid in the heart, and the air spoken of is very imperfectly characterised. Lastly, is it necessary to have recourse to the entrance of blood into the veins in order to explain the death of a patient suffering from

so enormous a burn, while undergoing disarticulation of the shoulder?

CASE XXII.—While this discussion was going on in the Academy, M. Dupuytren was made to say, that Dupuytren had lost another patient by the introduction of air into the veins, while he was endeavouring to remove a tumor from the axilla (*Gaz. Méd.* 1837, p. 757; *Lancet. Franc.* 1837, p. 122.)

These journals add, that in this same operation the accident has happened six times to M. Roux and other practitioners; but every thing combines to shew that these are mere assertions, without proofs, arising from some mistake, or only founded on hearsay.

CASE XXIII.—The following extract is found in the *Bulletin de l'Académie* (t. i. p. 132). M. Delaporte, while removing a tumor from the neck of a woman, aged 60, was alarmed by a hissing sound, and the occurrence of syncope. However, this accident had no bad consequences, and the woman recovered perfectly.

In this case we see that the jugular vein may have been wounded, and that the air may have entered it. But allowing this to be established, we must conclude that this accident is far from being always fatal, being the same conclusion which we unquestionably derive from our experiments on living animals. On the other hand, it must be allowed that the case is insufficient for conviction.

CASE XXIV.—I must say the same of a case published by M. Malgaigne (*Gaz. Méd.* 1836, p. 167.) In removing the jaw, and the ganglia of the neck, he opened the external jugular. The hissing, which has been erroneously called a characteristic sound, was heard; but no bad symptom followed.

CASE XXV.—Let us see how far the case communicated by M. Amussat himself is conclusive. He was extirpating a tumor from the breast of a woman, aged 47, when a stroke of the bistoury, carried below the clavicle, in order to separate some scirrhous granulations, was followed by a jerking noise, as it were in a zig-zag direction. The patient said she was dying, and almost fainted. The wounded spot was compressed, and a jerking compression (*compression saccadée*) was also applied to the chest; the fainting was not followed by any bad symptom, and the operation was terminated without any other accident (*Bull. de l'Acad.* t. i. p. 894.)

Here the breast was operated on, and the incision was made several inches below the clavicle. Nothing could be opened there but some branches of the mammary veins; a zig-zag noise was heard; the patient said she was dying, and then came to herself again. It seems to me that all

this is far from proving that air entered the heart, and that it is difficult to find a less conclusive fact.

CASE XXVI.—At the same period a case of the kind occurred to me, which I communicated to the Academy. (*Bullet. t. i. p. 896.*) While I was removing by its base a tumor of the neck, which extended to the carotid vessels, in a woman of about 36, I opened the internal jugular vein. An evident hissing sound was heard; a kind of bubbling was then perceived at the bottom of the wound; and the patient, like the former one, cried out that she was dying, and fainted away. I made an assistant place his finger on the orifice of the vein, and finished the separation of the tumor with a stroke of the bistoury. I had immediate recourse to the remedies employed for common syncope, and the symptoms soon disappeared. The woman left the hospital a month afterwards, the very day that M. Amussat communicated his case to the Academy.

Undoubtedly this case has some points of resemblance to what is observed in direct experiments upon animals. The sound and bubbling caused by the agitation of a mixture of blood and bubbles at the bottom of the wound, this immediate tendency to fainting, accompanied by anxiety and slight convulsive movements, are altogether something striking and surprising. When I saw all this combined in this patient, I must confess that I was alarmed. Yet, would it be possible for me at present to demonstrate that these symptoms were produced by air entering the veins? Are all the elements of well-founded conviction present in this case? I cannot venture to affirm it.

CASE XXVII.—M. Malle has related a case nearly resembling mine (*Presse Méd. p. 463*). The internal jugular vein was opened, while M. Begin was extirpating a tumor from the neck, and a sound was heard like that which is caused by emptying a narrow-necked bottle (in French, *glouglou*.) No particular symptom, however, supervened!

CASE XXVIII.—According to M. Guérin (*Thèses, Paris, 1837, n. 191*) it would seem that an accident like Dupuytren's happened to M. Mirault, of Angers.

While dissecting out a tumor on the right side of the neck, M. Mirault, who had undoubtedly wounded the internal jugular vein, heard a hissing noise, followed by a second and third sound of the same kind. Tetanic movements took place; but the patient came to himself at the end of eight minutes, was comfortable afterwards, and had not lost twelve ounces of blood. Three hours afterwards he died suddenly.

I would not deny the possibility of air

having entered the heart in this instance, as the operation was performed near the internal jugular vein; but I cannot help remarking, that air does not kill animals after three hours of tranquillity. The want of a post-mortem examination, too, deprives this case of its chief value.

CASE XXIX.—In operating upon a woman for cancer of the breast, and while asking her to move the arm away from the body, M. Toulmouche observed, that the incision made by M. Duval was immediately followed by a hissing sound, resembling that of a loud respiration somewhat prolonged. The patient fainted, and the bystanders thought her dead; but she soon came to herself, and ultimately recovered all her senses. (*Bullet. de l'Acad. t. ii. p. 146.*)

Here a small vein only could have been touched, belonging to the trunk neither of the axillary nor the subclavian; so that it would be difficult to deduce any thing very exact from such a case.

CASE XXX.—Another instance is mentioned in M. Putegnat's thesis. He says, that a case had just been told him of a man, who being struck with apoplexy, was immediately bled from the jugular vein. After this bleeding he died suddenly, and air was found in the right auricle. (*Thèse, n. 156, p. 41: Paris, 1834.*)

One does not immediately see why this patient is supposed to have died from the introduction of air into the veins, rather than by apoplexy. Moreover, doubtful facts cannot be cleared up by examples of which the particulars are so vaguely given.

CASE XXXI.—Here is another case still more extraordinary. A woman, seven months pregnant, was attacked with flooding. M. Maugeis bled her in the arm; but eight ounces of blood had hardly flowed when the patient uttered a plaintive cry and died! Nothing was found upon post-mortem examination.

What can we say of a case like this? "If death was not caused in this instance by the entrance of air into the veins," cries M. Maugeis, "what could have caused it?" Were I in this practitioner's place, my answer would be an easy one; I should content myself with saying, "I know nothing about it."

CASE XXXII.—M. Dubourg has just communicated another, which is as little conclusive. M. Roux, who has brought it forward, says so too. Such are the principal facts which I have been able to collect concerning the introduction of air into the human veins. Assuredly, it would be possible to add others, if it were allowed to insert every case of sudden death occurring during a surgical operation without any satisfactory reason. We

might thus, for example, explain the almost instantaneous death of a patient on whom tracheotomy was performed at the Hôtel-Dieu, in 1835, by M. Trousseau; as well as of several of the persons who have died while it was attempted to remove a tumor of the thyroid gland; but as no one has spoken of the introduction of air in these cases, it is useless to dwell upon them. Animals themselves are sometimes subjected to operations which have caused the same accidents, and the same opinions, as in man. Veterinary surgeons have long known, for example, that opening the jugular vein of a horse may suddenly kill it, as I have already said.

CASE XXXIII.—M. Boulay, junior, long since published, in the *Journal de Physiologie Expérimentale*, a fact which he has again brought forward in the present discussion, and which M. Putegnat mentions in his thesis (p. 32;) a fact, too, which is not without its value. He had bled a horse in the neck, and at the moment when he was raising the vein in order to sew up the incision, he heard a peculiar sound, and the animal was seized with trembling, and soon fell down. The blood, however, by continuing to flow, gradually dissipated these symptoms, and the horse soon recovered itself.

CASE XXXIV.—An exactly similar case has been since reported by M. Gerard. (*Putegnat, Thèse*, &c. p. 32.)

CASE XXXV.—I have already said, that a similar example was related by Verrier, as long ago as 1806.

CASE XXXVI.—M. Leblanc has told me that he knows of six other cases of the same kind.

These are good examples of the first symptoms which follow the introduction of air into the veins of the horse; and it is difficult to avoid seeing in them, also, a part of those which have been pointed out in man. It will be confessed, however, that all this is not conclusive, and that facts like these could not be allowed to count in science unless the principal question were removed from the discussion.

CASES XXXVII. XXXVIII. and XXXIX.—Even before the experiments of Nysten, the elder Legallois pointed out, in 1809, symptoms which he attributed to the introduction of air into the veins, and which would be much more surprising even, than all that is asserted in the present day, if we were obliged to accept the interpretation which he gives of them. Thus, Legallois says, that he has three times seen rabbits die suddenly immediately after bringing forth; and this happened because the atmospheric air, penetrating as far as the cornua of the uterus, entered into the venous system and the heart. (*Putegnat, Thèse*, p. 25.)

I do not know whether Legallois is not the experimenter of whom Nysten says that he has to complain; but, assuredly, the facts which he puts forth will not convince any physiologist who has been present at modern experiments.

Nor do I think that much more reliance will be placed in Legallois the younger, who refers *en passant* to his father's experiments (*Journ. Hebd.* t. iii. p. 183, 181.) and who maintains that the cases of sudden death in some women, shortly after delivery, probably arise from air having entered the veins of the uterus. This is too much opposed to all that later researches have taught us, to be now admitted into science.

[To be concluded in our next.]

STRICTURES UPON

DOCTOR ARNOTT'S TREATISE UPON WARMING AND VENTILATING.

To the Editor of the Medical Gazette.

SIR,

SINCE the subjects discussed in the following strictures are attracting the attention of the public at the present time, and since they are of great importance to the community at large, any of the periodicals of the day which invite original communications would have been a suitable channel for the publication of these. Considering, however, the subject of warming and ventilating houses to be of more peculiar interest to the medical profession, through whom knowledge of the kind can best be diffused amongst the public, and being a member of the profession myself, I have been led to select your journal as the organ for submitting my remarks to the considerations of the profession and the public; if you will do me the favour of inserting them. My matter will be divided into three or four successive papers, each not exceeding the limit I suppose to be set in a hebdomadal publication.

I have the honour to be, sir,

Your very obedient servant,

JULIUS JEFFREYS.

Kensington,
3d March, 1838.

Throughout the wide range of sub-

jects to which the attention of educated minds is directed, in a civilized country, while those belonging to what are called the polite arts may be more attractive to many, few can be found of greater importance to the whole community than those upon which Doctor Arnott has lately favoured the public with a treatise. In this work he has undertaken a task, which any author, of less high reputation, would assuredly fail in, namely, to bring about an entire revolution in the opinions, feelings, and habits of the people, upon questions not only affecting their domestic comfort, but also highly important to the public health. The reputation of the author is such, that if this revolution would prove beneficial to the public health, there is strong ground for hoping it will be extensively, if not generally effected. On the other hand, if it would prove injurious to the health of the public, there is equal ground for fearing the influence which his authority may give to his doctrines and proposals.

In coming forward myself on these subjects, I shall entertain no fear of the charge of presumption from those who will afford me attention while I state the grounds upon which I venture to do so. I shall rather suppose, that every one must acquiesce in the opinion that a man cannot, without justly incurring the opposite charge of indifference to the public good, allow any love of retirement to keep back from them knowledge which he himself has received through study, followed out by a long course of experiments, when that knowledge bears directly upon questions occupying public attention at the time, and tending, in their decision, to the most important results.

About fifteen years ago, I commenced, in the East Indies, a series of experiments on the ventilating and cooling of buildings, employing in some instances an upward, and in others, a downward ventilation. I put to trial a variety of mechanical means, and among them a new instrument which, for reasons that will be explained in their proper place, proved to be the most effective of any kind of *pumping* apparatus for ventilation which I have ever seen. Subsequently, I was led to introduce several chemical arts into that country, which, while they were wholly new to India, had to be conducted in a very different manner from similar arts in

Europe. My operations were on so large a scale as to employ, sometimes, 1000 workmen, and were, for the most part, connected with the use of fuel. It became necessary to subject to trial most of the furnaces used in the arts, and then to modify them, or to devise others suited to the materials, fuel, and climate, of the country. By subjecting every operation to a series of unre-mitted experiments, complete success attended all of them in the end; although the difficulties, in some cases, proved such as could scarcely be credited. Indeed most of my experiments were directed to the improvement of manufacturing operations, but of many of them the object was purely scientific. In all these operations my agents were the rude workmen of the country, to whom every thing they saw was new. It became necessary for me to conduct the making not only of large boilers, and other vessels of iron, copper, and lead; of vats, of wood and iron; of pumps, and other hydraulic apparatus; of lathes, presses, and cogged machinery; of fire brick and stone ware of various kinds; but of furnaces also of almost every imaginable form, such as horizontal and dome furnaces; vertical, cylindrical, and prismatic kilns; reverberatory furnaces: one of which, in my saltpetre manufactory, was 12 feet by 10 inside; furnaces with the fire in the centre; furnaces with side chambers; boiler furnaces of many kinds; vaporizing kilns; and a great variety of experimental furnaces, and fire places, much too numerous to detail. In many of these, as is the case in many of the arts, the same circulation of hot air obtained, and upon the same principles, as in Dr. Arnott's thermometric stove.

Having long practised mechanical ventilation, and been extensively engaged in the use of fuel, my thoughts were, at times, turned to plans for warming and ventilating buildings in Europe. In the tropics, the occasion for employing any of these did not exist, but upon my return to England, three years ago, having matured one of them, I had the apparatus necessary made in Birmingham. I had not, however, the opportunity of erecting it at the time, and I laid my plans aside for a while, and devoted my attention to the carrying into effect the principle of an instrument which is now before the public, and which was invented shortly

after my return to this country. About a year after this, I heard that Dr. Arnott had taken up the subject of warming apartments, and considering that it could not be in better hands, I determined to allow my own plans to remain at rest; but upon the examination of his stove, and the perusal of his work, I have been compelled to consider the different kinds of apparatus I have above referred to, of one kind of which the fire is open, and of the other enclosed, to be constructed upon principles so superior, that I purpose, ere long, bringing them before the public.

With the above experience, and as a member of the medical profession, I am justified in holding an opinion upon the matter of the work before us. This work undoubtedly abounds with clear statements of important and well-established truths in medicine and natural philosophy. Principles in these departments of knowledge have been made intelligible to the general reader, which, while they are thoroughly understood by many, have not been comprehended by the bulk of the people, for whose comfort in life it is desirable that they should be acquainted with, and form right judgments upon them. To the full extent to which the author has done this, and wheresoever his reasoning upon, and application of these principles, are in accordance with them, and manifest that most essential quality, practical experience, I would rejoice to add my humble meed of praise to the unbounded admiration which the work has drawn forth. But I am compelled to affirm, that in the most important instances, these principles, laid down in the commencement of the work, have been adhered to neither in the subsequent reasoning, nor in the practical details; while in other instances, contrivances are seriously recommended for adoption, which, it may be shown, are either indefensible even in theory, or wholly unpossessed of practical advantages.

In venturing to affirm these things, and to stand forth to prove the truth of my affirmation, I am aware of the unequal task I undertake. With so much of private worth, and standing so high as does the author in public estimation, I might well be disposed to maintain now that reserve which has kept me from appearing before the public till of late, and of late no more prominently

than the nature of my occupations has rendered indispensable. I am conscious, that weight of some kind is required to give to my observations stability against the midway tide of public estimation, and the force of the object borne upon its surface. Having lived in seclusion, it is not of course in the influence of a name this weight is to be found: I can look for it only at its proper source, namely, whatever of soundness there may be in my arguments, and of value in my experiments.

The revolution which Dr. Arnott would work in the opinions and practice of the public on the subject of ventilation, I believe to be, not undesirable only, but highly dangerous in its consequence; tending to make the visitations of epidemic and pestilential diseases far greater scourges than they are at present to our land: and, waiving even the question of ventilation, I am prepared, and therefore bound to show, that the instrument, which is the chief object of his work, is by no means the one best suited to the purposes it is intended for, and that the modified forms proposed for open fires, to which he returns at the close of the work, are singularly defective.

Wherever the point on which I am compelled to be at issue with the author is a question for reason, my argument is before the reader, and I submit it to his judgment: wherever it is one for trial, a disciple, though a very humble one of Bacon, and a keen experimenter, I call for an experiment.

As I purpose occupying my reader's attention upon those subjects only upon which I consider it necessary to comment, it will be allowable, and it will be conducive to brevity, if I take up these subjects in a different order. Accordingly, they will be discussed under the following heads:—

1. Ventilation in general. 2. Ventilating and warming by recovered animal heat. 3. Ventilating and warming by the aid of combustion. 4. Mechanical ventilation.

1. *On ventilation in general*,—viz. on the quantity of air desirable for man.

If the present state of the public mind is to be disturbed upon this highly important subject, it is of the greatest consequence that the disturbance should take place in the right direction.

The subject of ventilation is so much involved at all times with other questions

which force themselves before our view, demanding prior consideration, such as cold draughts, trouble and expense of warming air, mechanical difficulties, &c. that however rigidly we may commence our task upon right principles, we are in much danger of being forced from our principles, or of seriously compromising them, when the tide of such considerations sets in upon us.

In the commencement of his article on ventilation, while treating of it generally, Dr. Arnott lays down the grand principle of *abundance* in the supply of air, in language so forcible and just, that any reader would consider him the zealous advocate of a system of liberal and copious ventilation, as will appear from the following quotation, Art. 76 and 77, p. 57 :—

“The nature of man's breathing, and the conversion by it, of the pure air which is life to him, into foul air which is poison, have already been explained in Art. 10, as also the beautiful provisions of nature for ensuring the required supply of air; first, by the heating, and consequent rising into the atmosphere above, of air which has been breathed, to be replaced by fresh air from below; and secondly, by the never ceasing greater motions, called winds, of the atmosphere itself, which mingle the whole, and submit it to influences maintaining its general purity. These aerial movements are to man, what the constant gliding past of a clear river stream is to the fishes which inhabit it; and as certainly as we should destroy the trout of a stream by confining them in a small portion of their watery element, until it became a dirty, putrid puddle, or as we should distress and injure them by confinement and privation in a less degree, so do we destroy, or injure, human beings when we too closely confine around them a portion of their aerial element. The arts of life, and usages of society, have led, in many parts of the earth, to the practice of forming apartments, which thus confine the air; but because the air become unfit for breathing is still as invisible as before the change, and the cause of the consequent distress and disease is not obvious, it has been, and still is, in innumerable instances, allowed to continue working its fatal results, without awakening suspicion. Science, however, having at last detected the concealed destroyer of so much life, and

health, and happiness, the mass of the people are beginning to deal with it as it is befitting. A striking instance of popular ignorance with respect to this subject, and of mischievous practice, founded upon that ignorance, was to be witnessed very lately, if it is not still, in Buckinghamshire, among the poor girls who gain their livelihood by lace-making. To save the expense of fire, they were wont, in winter, to choose among the rooms belonging to their families the smallest, which would contain to the number of from twenty to thirty of them, and there to congregate, and keep themselves warm by their breathing. The odour of their breaths, though unperceived by themselves, soon became to a stranger exceedingly offensive. The pale faces, broken healths, and early deaths, of many of the ignorant self-destroyers, told to a better informed observer what they had been doing; but it was very difficult to convince themselves of their folly. Proving how much is yet to be done to inform the public sufficiently on this subject, I need only add, that even now, many schools, manufactories, churches, ball-rooms, courts of law, and other rooms of assembly, are, for hours together, in a state little better than the rooms of the lace-workers above described; indeed, sometimes in a worse state, as shown by the carrying out of persons fainting and half suffocated, and the cry among the company left for open windows. The scene of death in the Black Hole of Calcutta was the consequence of confinement, without ventilation, carried not very much further than has been witnessed, for a short time, in some voluntary English crowds.” And in Art. 77, the author proceeds to the same effect.

These passages should be written in gold. They are, in my humble opinion, by far the most valuable portion of the work. They may well place the reader's mind in a position of confiding security, that whatever Dr. Arnott shall write or propose in his details, an abundant ventilation is his primary object. Although this impression on the mind of his reader may be powerful and lasting, it nevertheless may be but of an indefinite kind. The facts, also, which he relates in the admirable passages quoted above, may not fail to influence the mind of the reader; and yet their influence may only be of a general cha-

rafter, unless he has paused well and measured carefully the quantities denoted by these facts. And it will appear presently that the reader is soon called upon, in our author's work, to admit arguments and adopt opinions of so different a kind, that, in order his judgment may not be offended by them, it is necessary that these first impressions should be only vague and general, involving no measurement of quantities. If the new doctrines to be acquiesced in had followed close upon the first,—if the matter in Art. 82, quoted below, had joined hard upon that in Art. 76, quoted above, while the reader was fresh from the perusal of it,—his mind could scarcely have followed the bias of the author's. In that case, he ought, upon coming on the new arguments, to start back upon the first, and to observe how irreconcilable was the one set with the other; but, unfortunately, the author is led from the grand subject of *quantity* of air suitable for man, into a long digression upon mechanical questions respecting the method of supplying air. Important as these questions are, they ought to have occupied another place. The too probable effect of this digression may be—nay, in order to make it possible for the reader to fall in with the new arguments, its necessary effect must be—such an occupation of his mind with mechanical subjects that it shall retain no more than general impressions upon the previous physical questions, especially if, confiding in his leader, he shall have neglected to study well his facts and the arguments connected with them. Thus, by reducing any clear and particular impressions regarding the quantity of air constituting wholesome ventilation into such as are but vague and general, the long digression from Art. 77 to 82 performs a sophistical part, though not intended to do so by the author: it serves as a bridge for carrying the reader gradually, and without a shock to his understanding, from the pleasant and airy ground he was first placed upon, to the very opposite side, as will now appear.

Art. 82, p. 66:—"There is, with respect to ventilation, a popular misconception and erroneous practice, of a nature the opposite of the total neglect described in the former paragraphs: because ventilation is important, there are persons, not satisfied with enough, but who demand, at heavy sacrifices,

what is excess. It would be a similar error, if a man, from knowing that water is a necessary of life, should abandon the never-failing well in his garden and his convenient home, that he might drink always from the Nile or the Ganges. A man needs, per minute, as explained in Art. 10, the oxygen of one-sixth of a cubical foot of atmospheric air; but, because of the mixture of his breath with the air around him, he requires, to be safe, a ventilation supply of from two to three cubical feet per minute. Now, the ordinary workmanship of house-builders in England, leaves, as crevices round the doors and windows, passage for many times three gallons per minute; besides, that there is the powerful ventilation of the frequent openings of the door when persons come and go. Yet there are in England many persons, who, under all circumstances, call out for open fires and open windows, and by the cold currents and other concomitants of a ventilation, twenty or a hundred times more than necessary, prodigiously waste fuel, and injure or kill their children and friends by catarrhs, rheumatism, pleurisies, &c. To these persons it must appear wonderful, that in Russia, where, all through the winter, there are only close stoves and double windows carefully closed, and no provision made for ventilation beyond accidental crevices, the people are very healthy, and more individuals attain a very advanced age than in almost any other country in Europe. In a room of 12 feet in all its dimensions, and containing therefore 1728 cubical feet of air, there is, without any ventilation whatever, an allowance of 2 feet a minute, for one person, for more than 14 hours."

This paragraph is perhaps the most influential in the book, on account of which, and of the doctrine it inculcates, it may be considered by far the most important. The reader who carefully studies each successive sentence, will perceive a gradually progressing departure in the mind of the author from that demand for copious ventilation expressed formerly, until he at last closes, by giving countenance to the Russian system of what we should call suffocation in England. Impressed with a conviction of the prodigious importance of a right settlement of the question upon which the author has agitated the public mind, I must request

my reader's attention to an analysis of each sentence in this paragraph.

"There is a popular misconception, and erroneous practice, of a nature the opposite of the total neglect described in the former paragraphs."

First, let us consider what is the thing here predicated, what is affirmed, by "the popular misconception, and erroneous practice." The popular misconception, and practice, is something which the people in general *think*, and *do*, in regard to ventilation. Now what they *do* in almost every house in the land, is to close the doors and windows in cold weather, allowing more or less leakage through the crevices, and to employ a fire under an open chimney, which, while it warms the apartment, excites such a steady and powerful draught in the chimney, as to ensure a pressure inwards of fresh air into the room at all the crevices; and what they *think* is, that this fresh air, if inconvenient, is at least highly salubrious to the majority of persons. This, then, is "the popular misconception, and erroneous practice." Again, in the former paragraphs, the "total neglect" of supplying air was not the only thing described; salutary ventilation was also described by comparing it to the constant flowings of a trout stream, and a copious supply of fresh air was contended for, by shewing how the fishes would perish if deprived of this wonted supply of their native element. If illustrations mean any thing, if a long line of argumentation means any thing, the "misconception, and erroneous practice" of the people of England is a demand for, and a command of, a quantity of fresh air in their dwellings, which does, in reality, fall far short of the quantity advocated in those former paragraphs. It is next said, "because ventilation is important, there are persons not satisfied with enough, but who demand, at heavy sacrifices, what is excess." It was formerly argued in those paragraphs, not that ventilation only was important, but that *copious* ventilation was so; the "enough" ought then to be understood as meaning such copious ventilation; but "the demand, at heavy sacrifices, for what is excess," means, by the whole context, only that demand which is usual in England, namely, the common quantity which enters by the crevices, and passes up our chimneys. If the copious ventila-

tion contended for at the beginning of the chapter, meant a less quantity even than this, to what purpose was the strong language there employed, and the still more striking illustrations? Again, as the author is, throughout, contending against things which are popular and general, and not merely against individual cases of eccentricity, it is a great pity that the poignancy of the doctrine now enforced against the usual supply of fresh air, should be softened down by employing the expression, "there are persons not satisfied with enough;" instead of saying, "the people in general are not satisfied with enough." By the latter expression, the reader would at once perceive that the ordinary quantity of air which it has been considered wholesome to let into our houses in the usual way, namely, through the crevices, aided by the chimney draught, is the quantity which he is here required to consider as excessive; so excessive, indeed, that the following comparison is employed to set it forth:—"It would be a similar error, if a man, from knowing that water is a necessary of life, should abandon the never-failing well in his garden that he might drink always from the Nile or the Ganges." The using of this comparison is no other than begging the question. Let it first be shewn by right reason and rigid experiment, that the air we are accustomed to let into our rooms bears a proportion to that we need, even as large, as does the water in a never-failing well to the little that a man can drink. The experiment may appear not practicable, but it is, in fact, being made every day by thousands; and it tells wonderfully against the author's comparison and argument. A man very soon drinks to satiety, and can take no more with benefit: any more water is useless, or injurious to him at the time. The quantity of air a man breathes has also its limit, but there would seem no limit to the quantity of fresh air* which is beneficial, as is proved by every comparison between those who occupy themselves within doors, and those whose occupation is in the open air. All the air, therefore, we ever have at

* The care to have it warmed when it is to be enjoyed within doors, is quite another question; and I engage to present the public with apparatus which shall effect this thoroughly and with abundant economy, and no over-heating of the air.

any one time entering our houses, falls short of the quantity which can do us good; but the water entering a never-failing well greatly exceeds the quantity which can do a man good at any one time; so that the air entering our houses falls short in comparison with the water of a never-failing well even, and how incomparably short of a Gangetic or Nilotic ocean. Before such a comparison had any reality in it, it would be needful to shew that the people of England had left the insides of their houses to live upon the house-tops, or in open sheds. The author proceeds—"A man needs, per minute, as explained in Art. 10, the oxygen of one-sixth of a cubical foot of atmospheric air; but, because of the mixture of his breath with the air around him, he requires, to be safe, a ventilation of from two to three cubic feet per minute. Now, the ordinary workmanship of house-builders in England, leaves, as crevices around doors and windows, passage for many times three gallons per minute; besides, that there is the powerful ventilation of the frequent openings of the door when persons come and go."

I will remark upon the latter part of this sentence first: since cubic feet have been, throughout, the term employed by the author to measure the air by, and, as he had just said, "from two to three cubic feet were needed," it is a pity that the expression "many times three gallons" should have been used, when the actual supply was being spoken of, for the argument would lead a reader to suppose that many times three of the first measure were being promised him; and, unless watchful, he would overlook the new term "gallons." It is therefore to be regretted that, having commenced the argument in *cubic feet*, the author should have closed it in *gallons*. Now, a gallon being barely the sixth part of a cubic foot, many times three gallons may still be under one time three cubic feet. The quantity must exceed four times three gallons, or it will be under two cubic feet even. What the leakage really amounts to, in any one case, or whether the author meant cubic feet, and not gallons, as the measure of it, it is not possible to form any judgment of, without knowing what crevices there are, and what the pressure from without is. The quantity of air entering will vary according to the form and collec-

tive amount of all the crevices by which it is entering the room, and the facility by which it can pass out again, and also according to the square root of the difference between the external and internal pressure. In how disadvantageous a light this law places Dr. Arnott's stove, when compared with open fires, will be shewn hereafter. In the former part of the passage just quoted, reference is made to Art. 10 of the work, where we find it stated:—"In respiration or breathing a man draws into his chest, at one time, about twenty cubic inches of air, and of that a fifth is oxygen, of which again there is converted into carbonic acid gas nearly one-half." To suppose that the lungs employed so much as one-half of the oxygen of the air of ordinary respiration would be to familiarize ourselves with the idea that our lungs are tougher-working organs than they really are; and it might be argued by some, if they can endure to retain air in their vessels until one-half of its oxygen has been vitiated, they cannot be very sensitive,—very needful of perfectly pure air; whereas, if only one-sixth or one-eighth of the oxygen of the air we inhale is employed—if the air vessels, which the air of our ordinary respiration traverses, reject and have done with air, of which, being so delicate, they can only make use of this small quantity; how very pure ought the air to be in the first instance! I am aware of the experiments, on record, of able chemists, showing that, while in the chest, air loses from a fourth to one-half of its free oxygen; and from these, it is to be presumed, the author has drawn his information. If he had afforded the subject more attention he must have been led to draw a distinction between the two very different conditions in which the air in our lungs is. One, and by far the larger portion, is that which is deep seated, occupying the finer ramifications and extremities of the air vessels; its quantity varies, probably, from 100 to 200 cubic inches. With this air the lungs are more or less distended during life; and it manifestly must undergo but a very gradual renewal; for, since a very little of it only is discharged with the air of each respiration, very many acts of ordinary respiration must be performed before this air can be changed. If *this* air should be breathed out into a vessel, I have no doubt that one-half

of the oxygen it had contained would be found converted into carbonic acid; and, I imagine, that it must have been upon this deeper-seated air, which comes forth only by a forced expiration or sigh, that the experiments referred to have been made. Indeed, for the purpose of catching a quantity of the air from the lungs, it is highly probable that a long and deep expiration was practised in those experiments, which were doubtless very correct; but which cannot rightly be applied to the air of our present question: this last air is in a very different predicament; being changed at every ebb and flow of each act of respiration, it is but a short time in the chest, and appears only the upper part and the larger vessels. In quantity it may not exceed from 15 to 25 cubic inches, and it is *this* air, of course, which, being the air of ordinary respiration, is that of which the author is treating.

At his low estimate of fifteen respirations in a minute, about 300 cubic inches of air would be breathed each minute, or nearly 93 grains in weight; and in 24 hours, 133,920 grains, of which, the oxygen would amount to 31,471 grains; if half of this, or 15,735 grains, were converted into carbonic acid, it would require fully three-eighths of its weight, or 5900 grains, of carbon to combine with it, in order that it should be converted into that acid. Now, taking our driest food, bread, even it has so much moisture and other elements in it, that we shall find it does not contain, as it comes from the oven, more than about one-fifth of its weight of pure carbon. A quartern loaf, therefore, may contain about the above quantity, 5900 grains of carbon. In order, then, to supply the carbonic acid generated in his lungs, at the rate stated by the author, a man would have to eat a quartern loaf daily, for this purpose alone, in addition to all the food necessary for his nutrition, and to supply the ordinary excretions of the body; and fearfully rapid would be the emaciation of those whose appetites fell short of such voracity! Had such an out-going of carbon been really necessary for our existence, we should certainly have been constituted, like the termites, or white ants, of the tropics, with appetites to enjoy, and powers to digest, ligneous fibre itself; otherwise, a man's whole labour devoted to tillage would barely

supply him with the primary necessary of life alone. All civilization and philosophy would be sacrificed to a furious and unproductive out-breathing of carbon. The author proceeds—

“Yet there are in England many persons, who, under all circumstances, call out for open fires and open windows, and by the cold currents, and other concomitants of a ventilation twenty or a hundred times more than necessary, prodigiously waste fuel, and injure, or kill, their children and friends by catarrhs, rheumatism, pleurisies, &c.” Open fires are one thing, and open windows another. I confess myself to be a warm advocate for open fires, (though I would have them employed in a different manner) but I do not know of any persons, who, under any circumstances, excepting, perhaps, to sweeten a house of a morning, call out for open windows from the month of October, at least, to that of April; and under certain circumstances only in the summer. It is, indeed, a pity that the author should connect together so very different desires, as that for open fires, and that for open windows; and by the help of an alliteration, by repeating in the reader's ear the sound of the epithet open, should endanger in his mind such a connecting of the two, as would charge the one with the absurdities of the other. Is it the ordinary ventilation excited by open fires through the crevices of *closed* windows and doors, or is it a ventilation unheard-of elsewhere, through windows thrown open, which is said to be so prodigiously more than necessary, and so destructive to the public health? It were far better for the subsidence of error in the public mind to let the subject rest, than to handle it in such a manner: the continuance of any existing error were preferable to that which must arise from interweaving two very different categories—a state of things with open fires, and that with open windows, in such a manner, as that the former shall have to bear, by the implication, accusations which could with justice be applied only to the latter. Of the healthiness of country children, accustomed to ill-fitted doors and windows, I shall presently have occasion to speak; but who ever heard of sitting with windows thrown open, excepting in the dog-days? The context throughout, and the whole drift of the argument, clearly mean, that the

ventilation which prevails universally in England in our houses, is exorbitantly great, and the cause of all these diseases. The usual method of supplying untempered air through crevices only, is a practice no one can more earnestly desire to see altered than myself, as the public will in due time be satisfied; but while I would greatly desire a change in our system of ventilation, I would increase rather than diminish its quantity, convinced that these (the diseases of our climate) arise, in the great majority of cases, from that *occasional* and *unaccustomed* exposure, against which it is almost impossible to be always on our guard, but against the effects of which all our experience proves that we are rendered less and less susceptible, in proportion as we accustom ourselves to a more and more frequent renewal of the air in which we live; and there is no reason why it should not be renewed with tempered air; but in that case I am prepared to show that the thermometer stove is an apparatus by no means well calculated for the purpose. Of this, however, hereafter, under its proper head.

[To be continued.]

THE RESULTS OF TAPPING THE HEAD

IN NINETEEN CASES OF HYDROCEPHALUS.

To the Editor of the Medical Gazette.

SIR,
NEARLY *ten* years having elapsed since I was first induced to attempt the cure of chronic hydrocephalus by withdrawing the fluid from the ventricles, the time seems to have arrived when the profession has a claim on me for some account of the results of these operations; and, indeed, this has become necessary, in consequence of the numerous applications for information on the subject by practitioners, not only in Britain, but in many distant parts of the world. Still, it is a matter involving such important considerations, that until experience has thrown much more light upon it, I do not feel justified in advancing any thing beyond the mere statement of facts, such as the present position of my inquiries warrants, leaving to a future day a more methodical and full inves-

tigation of the origin, nature, and progress of this formidable disease, with its appropriate medical and surgical treatment.

The operation consists in passing a small and delicately constructed trocar into one of the lateral ventricles, and drawing off so much fluid as the powers of the constitution will admit of. The most eligible spot at which the trocar can be introduced, is in the course of the coronal suture, about midway between the crista galli process of the ethmoid bone and the anterior fontanel, so that the danger of wounding the corpus striatum is avoided on the one hand, and the longitudinal sinus on the other. The instrument usually penetrates about two inches, and in most cases the serum has been colourless, but occasionally tinged with blood. In one instance, and that was in the last child operated on at St. Bartholomew's only a few weeks since, a large and alarming quantity of florid blood escaped; most likely from a branch of the meningeal artery. Sometimes on withdrawing the trocar the water will not flow until a probe has been passed along the canula to remove portions of cerebrum which block it up. After taking away all the fluid that can be removed consistently with safety, the head, which should always be steadily compressed by an assistant during the operation, may be strapped with adhesive plaister, that it may retain its diminished size, and that the fearful consequences of suddenly removing long-continued pressure from the brain may be averted.

I have now tapped in *nineteen* cases, and of these *ten* were living when last heard of. Several of the children, before the operation, were reduced to the most deplorable condition, having frequent convulsions, with loss of sight, emaciation, &c.; but the diminution or disappearance of these symptoms has been very remarkable. In some cases the results have been triumphantly successful; in others, from the reluctance of the patients to have the operation repeated, only temporary relief has been afforded; but none of these children died either during or immediately after the operation, and those which, in the subsequent list, are reported as dead, survived weeks or months after the fluid was withdrawn.

All the operations were performed in

the presence of many medical men, and most of them before large bodies of students at St. Bartholomew's Hospital, and their progress has been watched by gentlemen who have felt a deep interest in their termination; and although exclusive dependence has been placed on the withdrawal of the fluid without the auxiliary assistance of any pharmaceutical or other medical means, yet I consider much of the success to be attributable to the kind and able superintendence of medical friends.

Having long entertained a conviction that this deplorable disease ought not to be left without something being done for its relief and cure, and not discouraged by the want of success that had followed similar attempts, and considering "*anceps remedium melius quam nullum*," it was in the autumn of 1828 that—

The *first* operation was performed, on Catherine Seager, aged 20 months, whose head had been gradually enlarging during the previous half year. Not more than two ounces of serum flowed; but on a probe being passed into the ventricle (by Mr. Harvey, of Islington), when the child reached home, a considerable quantity of fluid escaped *stillitio*, so that during the night it was calculated that the saturated bandages and napkins could not contain less than two pints. Only one paroxysm of convulsions followed the operation, and some symptoms of meningeal irritation which supervened were speedily subdued by leeches and cold evaporating lotions. Two years and a half subsequently I had the high gratification, in company with some friends, to see this child, when the parents left England for America, and it was not only in perfect health of body, without the slightest evidence of its having been the subject of so formidable a disease, but in the full possession of its intellectual powers.

The *second* case was that of William Honey, aged 8 months. The enlargement of the head had been perceptible about six weeks, and on the 20th of November, 1829, I tapped him, at St. Bartholomew's Hospital, and withdrew twelve ounces of colourless serum from the right ventricle. On the 2d of December, twelve ounces more were withdrawn; and on the 16th an additional ten ounces and a half: making the total

quantity thirty-four ounces and a half. This child was progressing most satisfactorily when it became the subject of whooping-cough, to which intractable disease it fell a victim some months after the last operation.

The *third* operation was performed on William Wilmer, a boy now (March 1838) under a course of education in the Orphan Working School, City Road; nearly *eight years* having elapsed since twenty-four ounces of water were taken away by twice tapping him. The history of this interesting case has been given by Dr. Caldwell, of the City Dispensary, whose patient he was. That the account may be authenticated and impartial, the following statement is an extract from that communication:—"William Wilmer, aged 4 months, came under my care in the month of July 1830. His head was of an enormous size, and had been so from its birth: the forehead was large and prominent; his eyes heavy, and somewhat convulsed; frequent hiccup and vomiting, &c. Several gentlemen had seen the case, and they all gave it up as hopeless. In the beginning of August Dr. Conquest performed the operation upon this child, and immediately the fluid issued forth in a stream, at first clear, and afterwards a little tinged with blood. During the remainder of the day the child continued rather weakly, but was more lively than he had ever previously been, and for some time afterwards the intensity of all the former symptoms greatly diminished. When a month, however, had nearly elapsed, it was considered requisite to repeat the operation, and on the 3d of September Dr. Conquest again extracted a clear liquid, to the amount of twelve ounces more. The child sleeps well, eats heartily, is very lively, and in full possession of all its mental faculties.

"(Signed) H. S. CALDWELL, M.D.

"September 6, 1832."

Amongst other things mentioned in the paper from which this extract is taken, is the curious fact, that the head, which was so enormously large at the time of the operation, remained stationary, although the size and strength of the body has gradually increased in proportion to the age of the boy; and now that nearly eight years have elapsed, the head, although dispropor-

tionately large, remains at about the same dimensions.

The *fourth* case, that of Elizabeth Forster, is referred to with more than ordinary interest, not only because it is the one from which the largest quantity of water was extracted (no less than fifty-five ounces), but more particularly because I had lost sight of the child for years, and thought it was dead, until, in September last, I received the following most gratifying communication, which I transcribe entire, as it will convey all necessary information of the case:—

“Dear sir,—Being lately on a visit in Buckinghamshire, I was enabled, through the kindness of Mr. Cowley, of Winslow, to see the child, Elizabeth Forster, residing at Little Harwood, on whom you performed the operation for hydrocephalus about five years since. Her countenance and general appearance are healthy, her appetite good, and her rest at night undisturbed; she has been attending a school in the village, where her progress has been equal to that of the other children; she answered questions which I addressed to her on this and other subjects, with a shrewdness for which her governess says she is remarkable. The greatest circumference of the head is twenty-two inches; the ossification is complete, with the exception of the posterior fontanel, and two other openings of the same size, two inches apart, on either side of the medial line, in the course of the coronal suture. Her mother shewed a lively sense of gratitude for the benefit she has experienced under the treatment to which you had subjected her.

“Yours, &c.

“FRANCIS COOK, M.D.”

It would be useless at present to detail the particulars of the other fifteen cases, as all that is important to be known will be found in the summary given at the end of this communication, and from which it appears that, of the nineteen children operated upon, ten were living when last heard of, and nine are dead; but it is only fair to state, that as most of these children were amongst the lower classes of society, who are continually changing their residences, several have been lost

sight of, and may now very probably be dead, although when last seen, some time subsequently to their having been operated upon, they were living.

Of course these operations have been attended with different degrees of success. Unquestionably some are cases of perfect recovery; but in every instance there has been a very marked diminution of suffering, and prolongation of life, and in no one case has a fatal termination been accelerated.

Dr. B. G. Babington has analysed the fluid with great care, and states its specific gravity to be 1004. It does not coagulate by heat, acids, or alcohol, and consequently does not contain albumen. Tincture of galls produces no immediate precipitate, but after standing some hours a few brown flocculi subside, proving that it contains a very little gelatine. On evaporation, 1000 grains yield 10 grains of solid matter, chiefly chloride of soda, proved by precipitation with nitrate of silver. The liquid therefore contains, in 100 parts—

Water.....	99
Gelatine.....	1
Chloride of soda	845
Other salts and loss	055
	<hr/> 100.000

In no instance has clearly marked congenital disease been permanently benefited, and those cases have done best in which effusion manifestly resulted from inflammatory action, and in which cerebral excitement followed the operation. The number of cases tapped, with the quantity withdrawn, will be seen in the tabular summary which concludes this brief notice of the subject. [*vide next page.*]

I feel no ordinary pleasure in thus simply recording the progress of my investigation of this momentous and interesting subject, and shall be most happy to receive from my professional brethren any suggestions that may assist me in attempting to diminish this one source of human suffering and death.—I am, sir,

Your obedient servant,
J. T. CONQUEST, M.D.

Finsbury Square,
March 1, 1835.

No.	Name.	No. of times operated on.	Quantity withdrawn.	Living.	Dead.
1	Catherine Scager.....	1	3 xxxij.	1	0
2	William Honey	3	xxxivss.	0	1
3	William Wilmer.....	2	xxiv.	1	0
4	John Hall	5	lxviijs.	0	1
5	Alfred Parman	4	xlv.	0	1
6	Mary Ragon	3	xxvj.	1	0
7	Charles Discomb.....	2	xx.	0	1
8	John Ward	1	viiij.	0	1
9	John Clauditt.....	2	xxij.	0	1
10	Charles Clarke	2	xvij.	0	1
11	Elizabeth Forster	5	lv.	1	0
12	Jemima Evans	1	vijss.	0	1
13	Jane Brocken	1	xiiij.	1	0
14	Eleanor Mahoney	1	ix.	1	0
15	Francis Chiddy	4	xxxiiij.	0	1
16	Thomas Norman	1	vj.	1	0
17	Anne Armenio.....	3	xxxjss.	1	0
18	James Thomson	2	xiv.	1	0
19	John Pratt	1	ix.	1	0
19		41	451½	10	9

ON THE ANATOMY OF CERTAIN
STRUCTURES

IN

MYRIAPODA AND ARACHNIDA,

WHICH HAVE BEEN THOUGHT TO HAVE
BELONGED TO THE NERVOUS SYSTEM.

To the Editor of the Medical Gazette.

SIR,

THE communication of Mr. Lord, in the last number of the GAZETTE, respecting a structure observed in the centipede, *Scolopendra morsitans*, is one of particular interest, since the fact stated by him has immediate application to the controversy now pending between Dr. Grant and myself; this structure being that which Dr. Grant has hitherto described as the *true motor* tract, and which has also been considered by myself as analogous to the transverse nerves of lepidopterous insects. It now appears that the analogies between these different structures in Invertebrata, had not been correctly ascertained.

I had, however, long ago suspected that the structure in question in the centipede, and an analogous one in the

scorpion, did not in reality belong to the nervous system, and have stated my opinions, and shewed my preparations to many friends, but have been withheld from publishing them in consequence of my investigations of those structures forming part of a yet unfinished series of observations on the vascular system of invertebrated animals. I had not, however, traced the direct communication between the dorsal vessel in the centipede and the structure in question (*e*), which may be called the *supra-spinal vessel*, analogous to one in a similar situation in Crustacea, as described by Dr. Edwards. In addition to Mr. Lord's description of this vessel in the centipede, I must state that the central artery or continuation of the dorsal vessel on the upper surface of the œsophagus gives off a pair of branches almost immediately behind the brain, and these pass outwards behind the two lobes, while the middle artery is continued beneath the brain, as in insects and other Articulata. The branch described as originating at the side of the œsophagus from the *lateral communicating arches*, between the heart and supra-spinal vessel, passes forwards to the anterior part of the second segment, and then divides into two branches; the first

of which passes outwards, and enters the inner side of the base of the mandibles, while the other runs to the middle of the head, and passes beneath the muscles of the anterior part of the mouth and antennæ. From the posterior part of the communicating arches there appears to originate a branch, which I consider as the commencement of a venous lateral trunk; while the small branch which arises in the median line, on the anterior part at the junction of the arches, on the nervous cord, passes forwards to the first sub-œsophageal ganglion, and then divides into two branches, which proceed to the base of the antennæ. It was the course of these branches that first led me to suspect that this structure was not part of the nervous system.

I have now to state, that the structure which lies upon the nervous cord in the scorpion, *Scorpio afer* and *S. Europæus*, belongs also to the vascular system. I was first led to suspect this in the scorpion from this structure being always contracted irregularly in its course, being sometimes compressed from side to side, and sometimes flattened, and lying on the nervous cord, like a ribbon, and from its tearing, transversely, like a vessel, very easily; but was recently confirmed in this opinion on finding it was hollow, and often partially filled with granulated matter, and I have since more completely made out its anatomy and vascular nature.

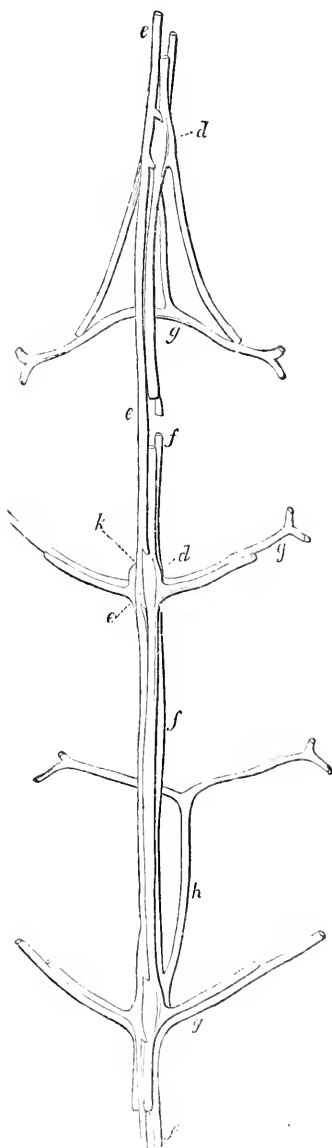
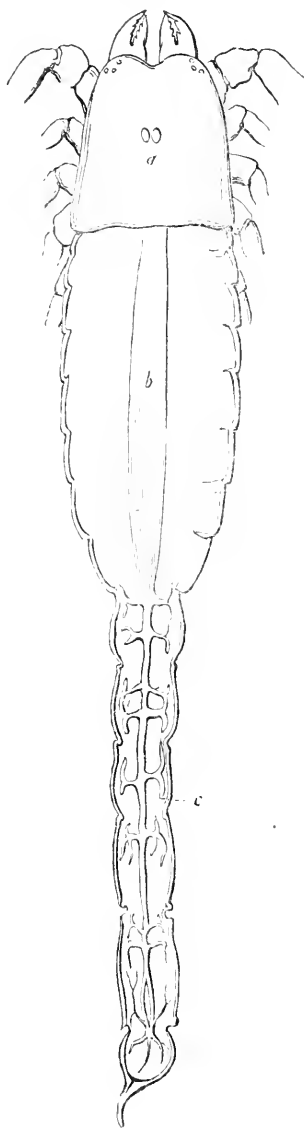
The dorsal vessel or heart (*b*) in the scorpion may be said to originate from the superior caudal vessel (*c*), which runs along the dorsal surface of the tail to the base, gradually enlarging in its course, and giving off in each caudal segment a pair of lateral vessels (*l*), which pass around the sides of the tail, and anastomose beneath each ganglion of the nervous system, with a vessel (*f*) beneath the cord (*d*), and which I shall presently describe as the *sub-spinal vessel*. The heart or true dorsal vessel consists of distinct chambers, furnished with valves*, and runs along the dorsal surface of the body as far as the posterior margin of the cephalo-thorax (*a*), where it descends very suddenly upon the œsophagus, and spreads out behind and above the great ganglion into three pairs of branches,

and one single central one, which becomes very small, and runs along the median line of the œsophagus to the anterior part of the cephalo-thorax. The other vessels are again divided and given to the surrounding limbs. The second pair of these vessels appears to pass around the œsophagus, and unite beneath it in a sinus just posterior to the great ganglion, and from this the supra-spinal vessel (*e*) apparently takes its origin. It then passes under a bony arch in the cephalo-thorax, unto which it is so strongly attached, that it was considered by Professor Müller as a ligament. After this it pursues its course, gradually decreasing in size, as noticed by Dr. Grant, along the upper surface of the cords, as far as the terminal caudal ganglion, and appears to end in two small branches, which seem to take the course of the terminal nerves of the cords. Throughout this course it sends down from its under surface, both anterior and posterior to each ganglion (*e k*), between the nervous cords, a branch, which unites beneath the cord with another vessel (*f*) the *subspinal*, but which I am not aware has hitherto been noticed in the scorpion, although a similar one was noticed by Duges in the leech, and called by him the *subnerval*. I have been unable to trace this vessel to its origin in the cephalo-thorax, where I first observed it, and from whence it is continued along the under surface of the cords to the caudal ganglion. Beneath the first abdominal ganglion it gives off a branch which passes backwards along the middle of the under surface of the abdomen until it meets and anastomoses with a vessel which runs transversely across the body (*g*), and pursuing the course of the nerves, from the first abdominal ganglion, is distributed on each side to the base of the respiratory sacs; a similar transverse vessel is given off from the subnerval beneath the second ganglion (*g*), in the course of the nerves to the respiratory sacs; and a similar one beneath the third ganglion, which is distributed in like manner; while another single vessel also originates from the subnerval beneath the third ganglion, and passing forwards into the integument of the abdomen, divides into two equal trunks, which, like the preceding vessels, are given to the respiratory sacs. In the tail of the scorpion the subnerval vessel also gives off a branch

* Not figured in the drawing.

beneath each ganglion, which unites, as above stated, with a branch from the superior caudal vessel (*f/c*), or origin of the heart. Judging from the peculiar distribution of the branches of the subspinal vessel to the respiratory sacs, I am inclined to regard it as the true portal or venous trunk, and the

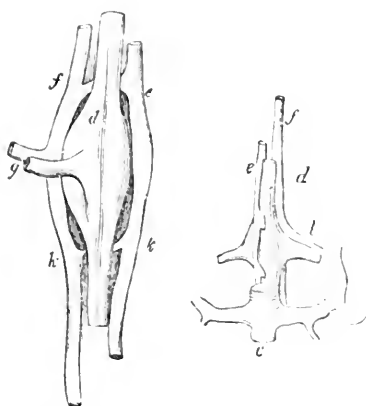
supra-spinal, to judge from its direction and direct continuity with the heart, as an aortal or arterial trunk. The blood, after being aerated in the sacs, is returned to the heart by four sets of vessels, which originate in the pulmonary sacs in small branches, and which, having formed distinct trunks, pass directly



ON THE ERUPTION ATTENDING
EPIDEMIC FEVER*.*To the Editor of the Medical Gazette.*

SIR,

SINCE the attention of the medical world has been directed to the exanthematic nature of what is called petechial eruption in continued or typhus fever, almost all the practitioners who have had the opportunity of observing such epidemics acknowledge a peculiar spotted appearance of the skin taking place in the first week of the disease, exhibiting spots of different colour, size, firm, and number, according to the different character of the epidemic, the age and constitution of the patient, the time which the spots themselves have existed, &c.; but having all the common character—that they disappear on pressure, or that they consist of a simple turgescence of the capillary vessels in the points in which they are seen. It is a matter of indifference whether we call them fever eruption, typhoid eruption, roseolæ, measles-like spots, and so on. I feel myself convinced, by a special attention which I have paid to the subject from 1833, when I first had the opportunity of observing an epidemic typhus, while attending the dispensary practice of the well-known clinical school in Halle, down to the present time—having attended the fever wards in Vienna for a few months, the Parisian hospitals for almost a year—having visited the fever wards of Birmingham, Manchester, and Liverpool, for a few days, attended carefully the Dublin hospitals for three months, and until this day the fever hospital of Glasgow (as often as my health allowed me) during two months—that, from the pale or rose-coloured spot (macula), which is called in France *tache typhode*, *éruption typhode*, to the papular spots, which have deceived even experienced practitioners at first sight, taken by them for measles, there is a regular gradation, that all degrees are observed, occasionally, in the same epidemic or endemic fever, but that there is no doubt, although, that either may be at one time the rule, and at another the exception.



from the sac to the inferior lateral surface of the heart. I suspect that there are also two lateral venous trunks; but these I have not satisfactorily traced.

These structures find analogies in insects and other Articulata. In perfect lepidopterous insects there is a large spinal vessel lying upon the abdominal portion of the cords. This was first noticed by Lyonet, but not as a vessel, in his posthumous publication, 1832, p. 595, (plate 52, fig. 18; and 54, fig. 2); and subsequently by myself, in the *Phil. Trans.* 1834, p. 395, plate xiv., fig. 9 (a). This is evidently the supra-spinal vessel. Duges, as above stated, shewed an infra-spinal vessel in the leech; but there exists also a supra-spinal, and I regard the like to exist in Myriapoda, although not distinctly traced.

These facts will prove that the supra-spinal structures in myriapods and arachnidans belong to the vascular and not to the nervous system, and controvert the opinion entertained by Dr. Grant, who regards them as the *motor* portion of the nervous system; while, I trust, they will tend to establish the opinion I have advanced, that the true motor structures in these classes consist, as described in my paper, 1834, of a series of fibres which form part of the longitudinal cords, and pass directly over the ganglia in all the classes.

I remain, sir,

Your obedient servant,

GEORGE NEWPORT.

10, University Street,
March 6, 1838.

* As there is some peculiarity of idiom in this paper, it may be proper to keep in mind that the intelligent author of it is a foreigner.—ED. GAZ.

† In an account of the epidemic typhus of 1833 in Halle (J. Staberoh, de typho exanthematico Halis epidemico: Halis, 1834), I was induced so to call the form, which I always observed.

In like manner the course which those spots follow differs in length, development, and termination. Sometimes they disappear in a few days; sometimes they become livid, and remain so until they disappear, or they become red again; sometimes they disappear while yet pink-coloured, and so on: but I must repeat that they continue to fade on pressure until they disappear.

The spots observed by Hildenbrand, Reuss, Bischoff, &c., and recently by Peebles, Perry, and many other medical authors, are described as following this course; and I may add, in the epidemic of 1833 above mentioned I never saw another termination.

What is called *Petechia vera* (*purpura*, Willan) are ecchymoses of different sizes in the rete mucosum, of which we have the best example in the *purpura hæmorrhagica* of Werlhof. We may compare them to flea-bites, because the resemblance is often very striking. Those spots were supposed until now, by a great many practitioners, to have characterized the typhus epidemics, which afflicted more or less, in the beginning of this century, all the countries which were the theatres of the French wars, and I refer to the very numerous accounts given of them in Germany (of which I have quoted in my thesis those alone who paid more or less attention to the existence of an exanthematous eruption). Many very accurate observers describe them as being petechial (*purpura*, Willan). Others, and I may refer to the most valuable paper of Dr. Peebles, in No. 125 of the *Edinburgh Medical and Surgical Journal*, suppose that those authors have been mistaken, and that they overlooked the circumstance that the true petechial spots (*purpura*, Willan) occasionally appeared among the exanthematous spots in the latter stages of the fever, depending upon the state of the skin, the general constitution, and the treatment, &c.* and appear to be convinced that those practitioners observed epidemics of continued fever similar to those which we see at this time. No person that my very learned and highly-esteemed friends in Glasgow, nor I, are aware of, mentioned a change of the first described exanthematous eruption into petechial spots—into spots which do not fade on pressure—a stage which I may

be permitted to call exanthematous petechiæ (ecchymotic state of the exanthematous spots). Dr. Peebles (*l. c.* p. 10), I understand, denies this change positively; and in the thesis above quoted, p. 30, I said, speaking of the roseola spots sometimes becoming livid—"minime vero, hic petechias me vidisse, quis suspicetur, quum macule hæcæ semper digito prementi cederent, ac circa eas rare apparerent, petechiæ veræ, colore ceterisque criteriis plane ab iis discrepantes."

But although I am satisfied that I was not mistaken with regard to those observations, which, however, were made in an epidemic of small extent, I shall be most happy to show to any man who will accompany me, for two or three days, into the wards of the Glasgow fever hospital, the above-mentioned change of the exanthematous spots into the petechial—into spots which do not fade on pressure—taking place in many cases of well-marked exanthematous eruption. While studying the fever in the Dublin hospitals I supposed this to be the case, and I think I mentioned it to my learned friend, Dr. Robert Law; but as I am not certain how far he approved of this opinion, I cannot avail myself of his authority: but in Glasgow, attending the same cases every day, and comparing the state of the eruption with great care, I found that petechial spots (which did not fade on pressure) were to be seen to-day in cases, were the day before exanthematous spots, fading on pressure, of the same form, and almost of the same colour, existed. This struck me, and I mentioned it to my very learned and very kind friends, Dr. Anderson (the physician of the fever hospital) and Dr. Cowan, who has favoured the medical world with the valuable pamphlet on "Statistics of Typhus Fever," partly the result of his attendance in the same hospital in 1836. Both acknowledged the possibility of the above-mentioned change, but they told me that they had never ascertained it. Hence I adopted, as well in their presence as in the presence of my kind companions, Mr. Carey and Mr. Stewart, clerks of this mentioned hospital, the following method to bring it to the proof. When I saw that, in a case of well-marked exanthematous eruption, some of the spots faded but slowly on pressure, while others disappeared easily (instantly); or

* I need not say that I observed also, repeatedly, this phenomenon.

when I found some of the spots not at all disappearing, but merely differing by a deeper shade of brownish red from the others, which still faded, I surrounded a certain number of these yet fading spots with a circle of dark thick ink, (better still with the nitrate of silver,) and I could, almost in every case, prognosticate with certainty that some of the marked spots would not disappear on pressure the next day, or the day after. It was always necessary to mark a number of spots, in which I supposed that the change would take place; as, very singularly, the change of one did not injure that of the others; and while I found that some of the marked spots became petechial, others continued to disappear on pressure, and followed the course formerly described, of a true exanthema, like measles and scarlatina. Yet, if I was not mistaken, I saw some of the spots, which became dark and faded but slowly to-day, regain a lighter colour, and fade promptly on pressure the next. Hence, by marking but two or three spots, I was very often disappointed, as the expected change did not take place in them, although the lividity or brown colour, &c. of the spots seemed to promise the success of the experiment. When the exanthema was very prominent, those pseudo-petechiæ preserved almost, I may say entirely, the same elevation above the skin*. But as to the relation of this change to the state of the patient, of the fever—as to the proportion between the different forms of exanthema—as to the relation of the exanthema in general to the epidemic at last, as to the comparison of the epidemic in Great Britain, Scotland, Ireland, France, and Germany; although I collected a great many striking facts, I'll not abuse the patience of my reader.

Professor Badham, before whom I had the honour to lay some of my observations, replied to me—"Well, but I hope you have not yet adopted any conclusion;" and certainly if there is any part of medical knowledge where the young practitioner gains, by cautious and repeated observation, it is in forming an opinion on essential (nervous) fever. I wish only to add, that the change of the eruption observed in that fever which I saw here (and I cannot speak, of course, of other epidemics with certainty until

I extend my observations,) strongly supports the idea, that the ancient authors, so remarkably well appreciated by Dr. Peebles, observed epidemics of continued fever of a character very similar to what we now see, both in this country and in Ireland. Further, that they may easily have overlooked the change of exanthematous spots into petechial, especially when the type of the epidemic caused it to take place in the earlier stages of the fever, as I think I saw it more than once the sixth or seventh day after the eruption of the spots; before attention was directed to the exanthematous nature of the spots by medical men, who observed epidemics, in which the appearance of petechial spots among the exanthema (or, perhaps, the change above mentioned also) was but the exception; as even until now it has not been proved by observers, in whom the medical world has the greatest confidence. I conclude these remarks with a few characters which could, perhaps, serve to distinguish the second (ecchymotic) stage of the exanthema (which may be partly a mere expansion of the capillary vessels), the exanthematous petechia, from the primary ecchymotic petechia (purpura) where they are found together; although I admit that, in many instances, only the complete attendance of the case, but particularly the experiment above mentioned, enabled me to distinguish them.

Purpura has its seat in the rete mucosum Malpighii; touches, at most, the level of the skin, but never rises over it: its colour has a well-marked blue shade in the red-brown, hence is the name purpura very striking. When examined with a powerful lens the colour is more or less uniform, although, especially some time after the appearance, paler towards the circumference. I may describe it as circumscribed by a well-marked edge*.

* I think I may ascertain it by very like casts taken by Dr. Paterson with his well-known dexterity.

* This condition of the purpura was most marked in a case of small-pox, in which the eruption receded a most entirely the third day. Purpura, as well as its larger variety, vibices, appeared throughout on the skin, and the conjunctiva appeared dark bluish red, purple, from extended ecchymoses, an appearance of which I have been favoured with a very good drawing by my friend, Mr. Anderson, clerk of the Royal Infirmary. The patient died the second day after the appearance of purpura, and on examining the body we found ecchymotic spots in the lungs, the stomach (my esteemed friend, Dr. Paterson, took a beautiful cast of it—the mucous coat appeared as well spotted as the skin in purpura hæmorrhagica).

The exanthematous petechia is always to be seen on the level of the epidermis. When the exanthema is very prominent, the corresponding petechial spot retains almost the same elevation—(further investigations are required); and on examining it with a lens, a granular appearance of the spots is observed, exhibiting an aggregation of more or less elevated brown red points on a paler ground. This dark red-brown colour has scarcely a shade of blue, and the edge of the spots is not so distinctly circumscribed as the purpura spots, which is easily ascertained by examining it with an armed eye.

JULIUS STABERON, M.D.
Of Berlin.

At present,
50, North Albion Street, Glasgow.

CASE OF HYSTERIA.

To the Editor of the Medical Gazette.

SIR,

FULLY conscious that the pages of your valuable journal are ever extended for the reception of aught that can benefit science or meliorate human suffering, I am induced to offer the subjoined curious case of Hysteria for publication in your columns, should your estimation of its merits entitle it to the favour.—I am, sir,

Your obedient servant,

JOHN JACOB,

Resident Medical Officer, Farringdon General Dispensary and Lying-in Charity.

March 12, 1833.

CASE.—On the 3d of September, 1834, I was requested by a gentleman to visit his daughter, a young lady, æt. 20, of rather pre-possessing appearance and

delicate constitution; the history of whose previous state of health I ascertained to be as follows:—She had been the subject of habitual constipation, to a great extent, two or three weeks having frequently elapsed without her having had any dejection from the bowels (an occasional head-ache, or nausea, being the only effects till then experienced). She evinced a somewhat capricious disposition, easily acted on by trivial causes. The catamenia had been little, if at all, interrupted; leucorrhœa had been more or less present for above a year past; her present suffering consisted (according to the notes I then took) of symptoms resembling those characteristic of colic—such as acute pain in the region of the left hypochondrium, extending towards the umbilicus; contractions of the abdominal muscles; costiveness; no febrile excitement; animal heat somewhat below par; tongue furred, but moist; pulse nearly natural; excretion from the bladder considerably diminished, &c. Notwithstanding these symptoms, I entertained some doubt as to the future progress of the case, and was therefore resolved to use much caution in its treatment. I merely, at first, cleared out the alimentary canal, which was not effected without some difficulty. The pain continuing for two days, and pressure on the abdomen inducing a degree of tenderness, I applied a few leeches, and advised the use of poppy fomentations. Meanwhile I prescribed a course of purgatives, anodynes, &c. A few days elapsed when the abdominal tenderness, &c. suddenly and completely vanished, and I found my patient suffering under considerable irregularity in the heart's action. It was now that my previous suspicions of the case appeared warranted, and I began to consider it a form of hysteria; of which opinion the subsequent detail will, I think, prove the accuracy. A state of considerable dyspnoea, identical in aspect with convulsive asthma, next engaged my attention. At some periods it resembled more a fit of whooping-cough, inasmuch as by frequently repeated convulsive expirations, the chest being exhausted of its contained air, and the respiration entirely arrested for the space of three or four minutes, the whole muscular system has been thrown into a state of rigid spasm, till a sense of the most dreadful suffocation being induced, the

rhagica Werlhofii), in the bowels, in the bladder, in the kidneys, and in the ureters. On examining the arm with a lens, while the light fell horizontally on the level of the skin, we saw at least a part of the epidermis unchanged above the dark purple colour of the spots. I could not ascertain whether, in the places where the papular elevations of the rising variola existed before, petechiæ of different kind from the purpura spots existed or not; but it would be interesting to examine whether, in small-pox, cases of this kind, as well as in measles and scarlatina, when petechial spots are observed, and, according to the general belief, all appear among the exanthematic spots, both exanthematous petechiæ and purpura spots are to be distinguished.

convulsion has abruptly terminated by a long deep inspiration, similar to the back draught in pertussis. The whole paroxysm has varied in duration, from half an hour to four hours; to which has succeeded a state of coma, that I have known continue, with little interruption, for two days. For three weeks these attacks of convulsive breathing recurred periodically, the intermission becoming more protracted as the general strength seemed to improve, their severity being reduced in frequency and degree: there nevertheless presented, at each recurrence, some new feature, indicative of the imitative powers of this singular affection; for scarcely a function in the whole body, whose interruption constitutes disease, has eluded the grasp of this mimic of diseases. I have observed its action on the brain, counterfeiting separately the various cerebral affections—as delirium, coma, mania, &c. I have noticed a spastic state of the muscles of one (generally the left) side of the body, including the extremities of the affected side. Here was a direct simulation of tetanus! The temporal and masseter muscles have been so rigidly contracted, constituting trismus, for the space of twelve hours or more, that all attempts to administer by the mouth were, even by the exercise of force, rendered perfectly impracticable.

Amaurosis of both eyes, to the effect of total blindness, forms the next item in this description. It has continued from twelve to twenty-four hours, the pupils remaining quite passive on exposure to a lighted candle. The most peculiar feature in this *mélange*, was a kind of hysterical somnambulism, under whose influence my patient has performed correctly on the pianoforte her favourite pieces; walked up stairs, undressed, and imperfectly fulfilled many accustomed domestic duties, unconscious of all around her; with no recollection, after the paroxysm, of what had transpired. Some portion of this detail may appear extravagant; I will, however, pledge myself to its accuracy. The irregularity in the heart's action was a general coexistent with almost every other symptom. The *globus hystericus* was altogether absent, and the secretion from the kidney considerably diminished; the convulsive efforts

made in respiration, have caused an augmentation in the capacity of the thorax to a very perceptible extent. Were I to give a more elaborate account of every symptom in such a train of morbid action as was exemplified here, I should be rather attempting a treatise on diseases generally, than the recital of an individual case.

I will now expose the plan of treatment pursued. My first care was to keep up a gentle and regular peristaltic action of the bowels, for which purpose I have used decoct. aloes comp. \mathfrak{ss} . pro dose. During the attack, when deglutition was practicable, I have exhibited combinations of aether, liquor opii sedativus, and camphor mixture; the stomach invariably rejected the preparations of *asafoetida*. I was induced, from the analogy existing between some symptoms in this case and the disordered respiration in pertussis, to make trial of the hydrocyanic acid, and must confess it afforded the temporary relief required. The tinct. lobel. inflat., in combination with valerian, was used with a view to obviate the dyspnoea, but failed in effect. I administered large quantities of the sulphate of quinine, thinking to check the periodic return of the paroxysms, with some advantage; but that medicine, from which the greatest benefit was derived, was the sesquioxide of iron, \mathfrak{ss} . bis terve quoti die, its administration being persisted in for some weeks. I recommended the use of the shower bath, but could not succeed in getting it employed; the cold bath was nevertheless substituted. It would be irksome for me to enumerate every remedy I have ventured upon: suffice it, that valerian, hyoseyamus, camphor, morphia, cubebs, arsenic, country residence on the coast of Kent, have all severally been had recourse to, with variable success; for, despite every measure ingenuity could suggest, the attacks, though mitigated, returned after a longer or shorter period of comparative convalescence. This detail comprehended a term of two years, at the end of which time circumstances transpired that opened a perspective of marriage to my patient. From maid she became wife, and a period not exceeding nine months made her mother. Sixteen months have now elapsed since she first became pregnant (which will be observed to have ensued immediately subsequent to marriage), during which

she has had no recurrence of any previous symptoms, and her general health has gradually improved to a natural standard. Her prolific powers are by no means inactive, she being at present five months advanced in her second pregnancy.

It appears from the examination of this, to me, unusual case, that distant local irritation was the cause of all the symptoms herein developed. The obstinate and long-continued constipation will appear ample grounds for remotely referring the whole derangement to the state of the alimentary canal; but admitting this as only a remote cause, and taking into consideration the contiguous relation of the uterus and its appendages with the lower bowels, the consequent continued pressure of a loaded intestine on the adjacent viscera, coupled with the constitutional irritation necessary to such a state, there is sufficient evidence to argue that the uterus, though secondarily affected, might itself become the source of the sympathetic disturbance indicated in distant organs. The facts of the disease being wholly arrested by marriage, the subsequent good health, and immunity from attacks, are sufficient to shew that disturbance in so important an organ as the uterus, partly dependent on the want of exercise of its natural function, is sufficient to induce a simulation of disease difficult to detect from actual disease by ordinary observation. I think that we are warranted in most, if not all, cases of hysteria, to consider it dependent on, or coexistent with, uterine disturbance, whether the local derangement be sufficient or not to attract our attention. We are all aware that disease may continue to exist for a length of time in an important viscus, without any considerable local disturbance resulting; it is therefore not unfair to conclude, that hysteria invariably, though not always evidently, depends on uterine derangement. An irritability of the nervous system peculiar to the female sex (without constituting the disease itself), may act as the medium through which the local irritation can develop itself with greater facility, in the same manner as the tubercular or strumous habit is more favourable to the development of phthisis than a robust constitution. I have met with ordinary cases of hysteria in public practice, in most of which I could de-

tect some deranged condition in the uterine function. In those few in whom I have failed to discover this, I have thought it right to imagine it to exist, though latent to my observation; I have therefore treated them in accordance with this view. We are too apt to think of alleviating the most conspicuous feature in a disease, without directing our attention to that which, though apparently insignificant, ultimately proves the actual source of all the mischief.

The foregoing remarks may appear redundant and intrusive, but the novelty of the above case will, I trust, exonerate me from a suspicion of impertinence.

CASE OF SUPPURATION IN THE VENTRICLES;

WITH

REMARKS BY SIR CHAS. SCUDAMORE.

To the Editor of the Medical Gazette.

SIR,

I FORWARD the enclosed case, thinking it to possess a sufficient degree of interest to merit publication.—I am, sir,

Your obedient servant,

JAMES PARRATT.

Mount Street, Grosvenor Square,
March 6, 1838.

Saturday, March 11, 1837. — Elizabeth Price, æt. 31, cook, suffering from severe pain in the head, referred more particularly to the forehead, which she describes as a constant throbbing and excruciating pain. Skin cool; pulse small and slow; tongue slightly furred; bowels regular; catamenia stated to have been always scanty; was in ordinary health until the evening of Wednesday last, November 8th, when, after much exertion in household work, she was seized with vomiting; at that instant came the severe headache, and it has continued to the present time.

States that for the last two years she has been subject to headache on exertion; is commonly very drowsy, and is accustomed to sleep for an hour after dinner. Has been in the habit of being bled twice in the year, in consequence of headache. Some years ago (possibly seven) she fell from a gate, and cut her head; but no particular notice was taken of it.

V. S. B. ad f. $\frac{3}{4}$ xx.; Hirudines, xvj. fronti et temporibus. Pil. Coloc. c. Calomel. iij. statim. Mist. Rosæ Aper. f. $\frac{3}{4}$ iv. sumet $\frac{3}{4}$ tiam partem horis duabus elapsis et repetat 4tis horis. donec alvus plene dejecerit.

12th.—Became very hot after V.S. and leeches; no relief from headache; did not sleep; bowels freely purged; motions dark; pulse 80; head hot.

Hirudines xvi. fronti et temporibus. Lotio Spirituosa, c. glacie capiti raso.

Evening.—Pain in the head as severe as before; has found no relief; eyes suffused; intolerance of light; pulse 88.

Cucurbitulæ Cruentæ Nuchæ ad f. $\frac{3}{4}$ xiv. Pil. Cal. gr. v. h. s. s. Haust. Sennæ crās, primo mane. Mist. Salin 4tis horis.

13th.—Throbbing pain relieved; complains now of intense pain over the whole head, as if it were being "riven asunder;" scalp hot; pupils natural; cannot bear the light; suffused state of eyes diminished; hearing morbidly acute; no sleep; pulse 88; tongue slightly white; urine scanty; bowels purged four times; motions dark.

R. Mist. Salin. 4tis addendo. Tinct. Opii $\frac{3}{4}$ ij. sing. dos.

Evening.—Has slept about half an hour since the morning. Headache intense. Other symptoms as before. Pain in the back; general soreness of the body and limbs.

Cucurbitulæ Cruentæ ad f. $\frac{3}{4}$ xij. temporibus; Empl. Cantharid. Nuchæ. Pil. Cal. c. Antim. 2 h. s. s. Haust. Sennæ primo mane. Perstet c. Lotion. Spirituosa et Mist. Salina.

14th.—Not relieved by cupping; continues to suffer same intense pain of head; groans a great deal; was slightly delirious in the night, for the first time; pulse soft, 100; bowels well purged; skin soft, but not apparently perspiring; much pain in the back, and general soreness.

Calomel. gr. i.; Pulv. Ipecac. co. gr. iij. M. 4tis horis. Mist. Salin. Aper. Efferves. c. Antim. Tart. gr. $\frac{1}{4}$, quartis horis. Fetus Papav. et Anth. pedibus, glacie capiti.

Evening.—Much the same.

Haust. Salin. c. Morph. Acet. gr. ss. hac nocte. Empl. Cantharid. cruri interno. Mist. Cathartic. f. $\frac{3}{4}$ vi. Sum. coch. iij. ampl. primo mane et repetat coch. ij. ampl. secund. quæque horâ donec alvus plene dejecerit.

15th.—Slept for about three hours. Vomited this morning, and suffered intense headache, which she continues to suffer; groans much on being moved; extremely nervous when spoken to; hearing acute as before; is not in any way delirious or insensible; pupils natural; bowels well purged; motions lighter; complains of back and limbs; universal tenderness; urine scanty, and said to be none except when the bowels act.

Haust. Salin. c. Syrup. Papav. Alb. $\frac{3}{4}$ j. 4tis horis. Calom. gr. i.; P. Ipecac. co. gr. xij.; Rhei. gr. xxiv. Ft. Pilulæ viij. c. Syrup. quarum sumat duas 4tis horis.

Evening.—Same distressing pain of head. No alleviation.

Cataplasm. Mier. Panis capiti loco glaciei. Liq. Amm. Acet. f. $\frac{3}{4}$ j.; Liq. Opii Sedativ. $\frac{3}{4}$ xlj.; Morphæ Acet. gr. i.; Mist. Camp. $\frac{3}{4}$ ij.; Syrup. Tolut. $\frac{3}{4}$ ij. M. sumat coch. iij. ampla statim et post horas duas repetat coch. ij. ampla. Postea repetat coch. j. ampl. omni hora.

16th.—Has taken all but one dose of the composing mixture; slept for about an hour or two; intense pain as before; no paralysis; pulse 108; tongue whitish; countenance indicative of cerebral oppression; pupils not so active on the approach or withdrawal of light; urine scanty; prefers the poultice to ice.

Perstet c. Mist. Opiat ejus sumat coch. j. ampl. omni hora si symptomata urgeant. Haust. Diureticus, 4tis horis. Pil. Cal. gr. iij.; P. Scillæ, gr. iij.; P. Digital. gr. vj.; Terebinth. Venet. q. s. ut fiant Pilulæ xij. Cap. ij. 4tis horis.

Evening.—Becoming comatose; groans much; pupils inactive, and somewhat dilated; very restless; sudden startings; no sleep; gums a little affected; bladder distended. About three pints of urine drawn off by the catheter.

Empl. Canth. capit. raso Ol. Terebinth. $\frac{3}{4}$ j.; Muc. G. Acaciæ $\frac{3}{4}$ ij.; Syrup. Tolut. $\frac{3}{4}$ j.; Aquæ $\frac{3}{4}$ j. M. sumat quartam partem 4tis horis. Perstet c. Pilulis ex Calomel. Scilla et Digital.

17th.—Coma increasing; groans much; in other respects the same. Lower parts of eyes injected; one pint of urine drawn off; pulse 108.

Perstet c. Mist. Terebinth. ut hieri. The blistered surface was dressed with Ung. Hydrag. Fort.

Evening.—Much the same.

R. Calomel; Scillæ aa. gr. j. Sacchar.
gr. iij. M. 3tia qq. horâ.

18th.—Perfectly insensible; much more quiet and composed; abdomen tense; bowels confined; urine escapes involuntarily; pulse steady, and some force.

Iodin. gr. j. Potass. Hydriod. gr. x.
Aquæ ʒiv. Syrup. Aurant. ʒij. M.
sumat. 4tam part. 'secundis horis.
Calomel, gr. v. Scammonia gr. ij.
Jalap ʒss. M. ft. Pulv. ss. Perstet
c. Pulv. ex Calomel. Scillæ et Sac-
charo.

Evening.—Perfectly insensible; pulse 108, regular and somewhat sharp; skin warm every where; bowels purged several times unconsciously; motions black; urine escapes as it is secreted; eyes glassy, pupils dilated, and scarcely affected by candle-light.

Perstet c. Mist. Iodin.

19th.—Mortua est.

Sectio Cadaveris.

On opening the head, the dura mater appeared every where exceedingly turgid. On its under surface it was in some places over both hemispheres firmly adherent near to the falx major, so as to require great care in detaching it, lest the substance of the brain be torn. The tunica arachnoidea covering both hemispheres was thickened, more on the left than on the right side. The substance of the brain was much injected, and exhibited innumerable bloody puncta on cutting into it: this was also greater in the left hemisphere. The left lateral ventricle contained from an ounce to an ounce and a half of serum; the right lateral ventricle contained from four to five drachms of pus, with flakes of coagulable lymph; gelatinous lymph was abundantly effused along the floor of that ventricle; on eleasing it, a patch the size of a shilling in the situation of the hippocampus major was altogether disorganized, presenting a brown-reddish colour, and exceedingly soft and pulpy. On opening the third ventricle, a small quantity of pus was seen, which is supposed to have escaped from the fourth ventricle at the time that the brain was being removed from the cranium; for, on opening the fourth ventricle, it was seen filled with pus, and the floor of it was disorganized as in the right lateral ventricle, but not

to the same extent; it was of the same reddish-brown colour, but not so soft, although much softer than natural.

The trunk not examined.

On Tuesday, the 14th, the advice of Sir Charles Scudamore was requested in consultation: he has favoured me with the following observations:—

"This case, unfortunate as it was in its result, affords much useful matter for reflection. It is unavoidable in the practice of physic that every now and then we are doomed to receive instruction from our failure instead of our success; so much is our art confined within certain limits. The pain of head which this patient suffered was remarkable for its severity, and she almost constantly exclaimed with agony as if she were undergoing a painful surgical operation. It appears singular that none of the bleedings yielded any relief from pain; the sense of beating was the only symptom alleviated; and hence it was, that on my first seeing the patient, and finding great evidence of exhaustion and debility, I did not recommend further depletion from the vessels, and thought it a safer course to employ medicines calculated to act consecutively on the liver, bowels, kidneys, skin, and nervous system; while, as auxiliary treatment, the sedative influence of cold was used to the head, and counter-irritation employed remotely. The cold failing, the relaxing power of heat was tried, as will be seen; but all the attentive treatment adopted in this case from first to last appeared to have scarcely the smallest useful result. The disease pursued its course with unrelenting severity. It was deep-seated *meningitis*; and it is highly probable that the sheath of the spinal cord was also affected, so remarkable was the pain down the back, the general soreness, the extreme restlessness, the inattention to a full state of the bladder, even before the 16th, when no urine had been passed for a considerable time, and three pints were drawn off by the catheter.

"Could any treatment have met with success in this case? None, I apprehend, unless at the very commencement a still freer abstraction of blood had been employed, carrying it each time *ad deliquium*, and with a view so to impress the heart and circulation that the vessels of the brain should have their morbidly increased action decidedly controlled; for, manifestly, it was purely inflam-

matory action terminating in *suppuration* which led to death. Such is our information derived from the post-mortem examination, but it must be at once admitted that Mr. Parratt's immediate treatment was active and quite appropriate. It is often difficult, perhaps impossible, to decide how far depletion from the blood-vessels shall be carried with propriety; for if our zeal with the lancet &c. be intemperate, we may rush from Scylla to Charybdis, and prove the immediate authors of destruction.

"But if there be one point of treatment more important than another in the most active inflammations, it is this—that our depletions from the vessels should be early as well as free, and quickly repeated, as far as may be necessary to control the heart and whole circulation in a very effectual manner. If such treatment be delayed till the disease has greatly advanced, it may not only prove ineffectual, but actually bring on the fatal termination.

"I have no doubt that the foundation of this disease had been laid very long. She had for a much longer time than usual omitted to be bled. It was remarked by her fellow-servants that for a considerable time past she had complained greatly of headache, and of oppressive sleepiness in the day-time. It was a chronic disease, all at once rendered perfectly acute."

DISEASE OF THE PLACENTA.

To the Editor of the Medical Gazette.

SIR,

HAVING read the case of inflammation and suppuration of the placenta, published in your last number, I am induced to forward a few particulars of a case of disease of that organ which occurred in my practice a few years since, in which the placenta contained a vast quantity of ossific matter nearly amounting to one-fourth of the whole substance. The symptoms were shortly these:—Considerable pain over the region of the uterus during the latter months of pregnancy, unattended by any other symptoms of inflammation. After the birth of the child, a fearful hæmorrhage supervened, rendering immediate manual interference necessary. The placenta was found firmly attached

almost throughout its entire surface to the fundus uteri, and could not be separated without very considerable force. When removed, it presented a very unusual appearance, being a firm, whitish, leathery-looking mass, interspersed with solid masses of bony matter. A preparation of this morbid placenta was made, and deposited in the King's College Museum, by the late lamented A. A. Cann, Esq., curator at that time. The patient did well, and has since been delivered under very similar circumstances; the placenta, however, this time did not contain so much ossific matter, owing, it was supposed, to the use of bleeding and purgatives in the latter months of pregnancy. If the foregoing brief account is thought worthy of a place in your journal, by publishing it you will oblige

Your obedient servant,

J. WETHERFIELD.

1, Henrietta Street, Covent Garden,
March 12th, 1838

MEDICAL GAZETTE.

Saturday, March 17, 1838.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tuarum; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

WHY IS QUACKERY SUCCESSFUL?

It has long been a moot point whether an unsparing enemy or an injudicious friend is most to be dreaded; for the support of the latter will too often ruin the best cause. We feel ourselves at present in this predicament, and are backed in this undesirable manner, by the writer of a pamphlet which is lying before us*. The writer tries to explain why quackery prospers, and how it should be put down, but we do not think he is very successful in either attempt; yet, as we agree with him in his object, it is with a natural reluctance that we shall endeavour to point out his omissions, and rectify his misstatements.

* Quackery: its Danger, Irrationality, and Injustice; the Causes of its Success; the best Means of its Suppression.

The causes of the success of quackery are very faintly and vaguely set forth by the Bath pamphleteer. He thinks that the success of charlatans may be explained by the "strong and absorbing feeling of the human mind—the love of life," to which their pretensions are addressed, and by the extravagant commendations of those who have derived benefit from quack medicines; moreover, patients "are ignorant of the structure and functions of their body, of the seat and nature of the great majority of the diseases to which it is liable, of the principles which should regulate their treatment, of whether they are capable of cure, *would get well of themselves*, or are inevitably fatal." With the exception of the few words we have underlined, these reasons seem to us singularly weak. The mere love of life, however strong it may be, however weak the person whom it afflicts, would never make him choose a quack, when he might have a Mead or a Heberden. Many people have a greater horror of shipwreck than of fever, yet this does not induce them, when they are about to undertake a voyage, to request the first schoolboy whom they see navigating a punt, to become the captain of their vessel. Quite the reverse; they make anxious inquiries whether the captain is experienced, the crew steady, and the ship A 1 in Lloyd's register.

Nor do we see why extravagant commendations are more likely to be lavished on a successful nostrum, than a successful remedy in regular practice; and as every sick man must have heard of fifty persons cured by educated practitioners, for one suffered to get well by a quack, the balance of commendation must be infinitely on the side of common sense and honesty.

The gist of the matter unquestionably lies in the few words we italicized in our quotation; the patient does not know whether or what diseases would get well of themselves, and, conse-

quently, if he recovers, ascribes all the merit to the Batavian Bolus, instead of the *vis medicatrix Naturæ*.

This, then, is the true reason why utter ignorance may long impose on the credulous public in physic rather than in other arts; most diseases have a natural tendency to get well. This tendency may be encouraged by judicious treatment, or thwarted by very bad; but the tendency is still there, and excepting, perhaps, in cases of malignant disease, this salutary effort is never absent. Hence we see why a painter turning physician, without any knowledge of physic, may make his 5,000*l.* a year by excoriating the chests of his employers; while if the converse were to happen, and a physician were to propose to live by painting, without knowing an atom of the art, he would be thought fit only for Bethlem. Hence, too, the analogy fails which the author institutes between a remedy absurdly extolled as curing every disease, and the supposed panacea of a *hygeian* watchmaker.

"What would be thought of the individual who advertised that he had discovered a something which, if put into a watch, would rectify all imaginary [imaginable] deficiencies; mend mainsprings, supply broken cogs, lost axles, renew worn-out wheels, and make the hands, on whatever cause their irregularity might depend, keep proper time?"

As in the former instance, the watchmaker has no help from any *vis medicatrix horologii*; and he who should attempt to make a watch go by poking something into it at random, would inevitably be set down by every customer as a fool or a madman. On the other hand, the watchmaker possesses the advantage of taking his machine to pieces, which the physician does not enjoy. Lastly, the watchmaker is the professor of a sort of exact science, while medicine is a conjectural art*.

* Est enim hæc ars conjecturalis. — *Celsus*, Lib. i. Præf.

A curious topic might be touched upon here, which is not even alluded to by the author of the pamphlet. Surgery has always been justly accounted the clearest part of the art of healing; its merits and effects are the most obvious*. The difference between a genuine surgeon, and an itinerant attempting to set bones by the light of nature, would be obvious, one might imagine, to the meanest capacity. Yet, even here, uneducated cataract-curers, rupture-doctors, and what not, have poached upon the medico-surgical preserves, and bagged some game.

Perhaps this may be attributed in part to the following reasons:—The entire domain of medicine and surgery is so vast in extent, that, until a very recent period, certain regions were hardly brought under regular culture, and lay like a kind of debateable land, between the limits of honest practice and charlatanism. Thus, not more than forty years ago, the first oculist in London used secret remedies; his prescriptions being drawn up in a way intelligible by one druggist only, in order to keep his lucrative mysteries as much as possible from rival eyes.

Now if the regulars did this, the public could not hope for better, nor experience much worse, at the hands of professional Cossacks and Lanzknechts. They would naturally say, if the tip-top oculist in London has no medicines but nostrums, why should we hesitate at taking them from our circumforaneous eye-doctors in Cornwall, or Cumberland?

This reason, however, will not apply to surgery proper, *quæ manu curat*; and here we are reduced to suppose either that those who employ the spurious surgeon are not within the reach of a genuine one, or that the genuine one

is a surgeon by diploma, not by skill, or that the patients have lost their wits.

This last supposition is the only method of explaining a curious advertisement for a quack, which appeared, not long since, in the *Leeds Mercury*. It seems that alarming accidents are of almost daily occurrence in the neighbourhood of Grassington, and therefore sundry benevolent persons, including three virtuallers, advertise—for what?—for a skilful surgeon? *Minimè gentium*—for a bone-setter! This is certainly not sustaining the character which the men of Yorkshire have acquired for acuteness in the common affairs of life. Even supposing (which we have no reason to do) that the neighbouring surgeons did not put up fractures with the most consummate skill, still this way will mend no broken bones.

It must be confessed, however, that as in surgery the superiority of the skilful over the pretender is boundless, and must be obvious to any one who is not utterly demented, so the mistakes of the half-educated will be painfully distinct to the patient, and will sometimes have the effect of throwing him within the grasp of the empiric. Thus, if we may be allowed to put a case by way of illustration, an Englishman sprains his ankle at Hamburgh, in the severest manner; in fact, it is a sub-luxation. He calls in an eminent surgeon, who uses the usual remedies, leeches and cold lotion, and bandages the suffering part with the dexterity of a practised hand, so that the patient is soon able to walk, the bandage not being disturbed by motion. He returns to England, and his foot still requires support; but living in a small market-town, he is no longer within reach of an eminent surgeon, but calls in his usual family attendant. A great mystery is now revealed to our astonished countryman: *it is not every one who understands all that he*

* Celsus, when speaking of the branch of medicine, *quæ manu curat*, says, "*estque ejus effectus inter omnes medicinarum partes evidentissimus.*" Lib. vij. Præf.

professes ; the foot bandaged by Dr. A , of Hamburgh, could walk up the Jungfernstieg without pain or stumbling ; the foot bandaged by Mr. B., of the small market-town, will not walk at all, for the bandage becomes loose in two minutes. Now the foot is the same foot ; it is therefore the surgeons that differ, and Mr. B. is no longer the cleverest man in Sussex. If a wandering professor of the art of bandaging should pass through Crowthorpe, we will not pledge ourselves that the disappointed patient will not employ him.

Again, as to the suppression of quackery, though we agree with the author of the pamphlet as to the end, we differ much as to the means.

One of his assertions concerning this branch of the subject is most startling ; he says :—

“ Quackery is suppressed in all the other countries of Europe. The French, German, Austrian, Prussian, and Italian systems of medical jurisprudence, all enforce the necessity of a comprehensive preliminary and medical education ; they protect the rights of medical practitioners, and prevent their *slightest infringement by illegal pretenders*.”

Assuredly, it is at least bold to say that quackery is suppressed in France. One has only to walk in the streets of Paris, and in spite of the regulation put up in letters a foot long, *il est défendu de déposer des ordures contre ce mur*, wherever there is a dead wall, it is covered with placards vaunting the *spécifique* of M. Un Tel, or the *Rob anti-syphilitique* of M. Chose. Those who have not seen Parisian dead walls may have read Parisian journals, which will bear witness to the same fact—to the flourishing state of quackery in the country where the pamphleteer supposes it to be suppressed. Thus, in the sitting of the Academy of Medicine on the 20th of February, the first article of the official correspondence consisted of a letter from the minister, sending a

secret remedy proposed by one Boyer ; and the fourth article was another letter from the same high source, with the receipt of a refreshing lemonade*. The Academy has the honourable office of investigating the merits of these nostrums, and reporting upon them to the government.

The same learned body, too, has been long occupied with attempts to suppress the sale of quack medicines ; they propose that patents shall not be given either for medicines or for cosmetics, and that the substances, whether simple or compound, which are employed under the name either of cosmetics or remedies, shall be subjected to the laws which relate to the compounding of medicine†.

These constant attempts to suppress quackery in France shew clearly that the previous ones have met with little success ; just as Hume says of the continual confirmations of Magna Charta, that they shew how little that great compact was observed. That the propositions of the Academy, if passed into a law, will be habitually eluded, no one can affect to doubt. At the sitting of the Academy where the article we have just quoted was confirmed, M. Desportes thought that the word *cosmétique* was not sufficiently clear, and wished to have it followed or preceded by an explanatory phrase ; on which a voice cried out—“ *C'est une affaire de dictionnaire*.” Like M. Desportes, however, we have our misgivings ; nay more, we go farther, for we fear that, in a free country, where thousands are willing to administer nostrums, and millions to swallow them, no definition, however logical, will prevent their sale. Nevertheless, we agree entirely with

* 1. Lettre ministérielle avec envoi d'un remède secret proposé par le sieur Boyer.

4. Lettre *idem* avec envoi de la recette d'une limonade rafraîchissante. — *Gaz. Méd.* Feb. 24, 1838.

† *Gaz. Méd.* March 3, 1838.

the Academy, and the author of the pamphlet before us, that it would be desirable to abolish the shameful practice of stamping quack medicines, and thus giving them a kind of royal sanction in the eyes of the ignorant.

DR. MARSHALL HALL AGAIN.

"*Ecce iterum!*"—We have received the following from Dr. Marshall Hall:—

To the Editor of the Medical Gazette.

SIR,

I am sorry I was not more explicit in my letter, and now send you a very brief note, which will supply this defect. The "misrepresentations" (the word is *borrowed**) in your two articles, are at least four:—

1. You represent my assistance as very insignificant. I have explained *what* it really was. I might have added that I had engaged also to supply the admission-fee and subscription, should the individual alluded to be elected an F.R.S.

2. You have represented me as trumpeting forth my assistance; whereas, in fact, I stated it in reply to a charge of "*suppression of facts*," this being the *only* fact which I had suppressed.

3. You have made a misrepresentation, arising out of the mystification which has been employed relative to the *subject* of discussion: that subject is the *third nervous column* in the articu-
lata, its discovery by Müller, in 1828, and that of its *motor* function by Professor Grant, as given in his lectures, in 1832.

4. You represent me as repeating the word *brutal*, in reference to the conduct of the Council of the Royal Society towards me. I did *not* repeat that word. But I *do* now repeat, that to refuse to witness my experiments, when I offered to give up five years of professional avocations and advantages, *was* brutal in the extreme.

To revert to the more recent subject of discussion, I beg your readers further to remember, that, in what I have

written, I was stimulated by unfounded accusations:—1. of "decided misrepresentations;" 2. of "suppression of facts;" 3. of "statements not in strict accordance with facts," &c., without one tittle of proof, by one whom I had befriended to the uttermost.—I am, sir,

Your obedient servant,

MARSHALL HALL.

14, Manchester Square,
March 12, 1873.

We are thus charged with four "misrepresentations;" and we shall answer them numerically, as set forth by our antagonist.

1. We did not represent the assistance as "very insignificant;" but we said, and we now reiterate the opinion, that the loan of 15*l.*, and recommending a pupil to Mr. Newport, did not warrant Dr. Marshall Hall in asserting that he had "fed, clothed, and housed," that gentleman.

2. We represented Dr. Hall as trumpeting forth his assistance, for this plain reason—that he did so; as every one must admit who took the trouble to read his letter published in a contemporary journal. As to his having been obliged to do this because he was charged with a "suppression of facts," the simple truth is, that the worthy Doctor was accused of suppressing certain facts regarding the nervous system; to which he replied by adducing certain facts relating to his account-current at Sir Claude Scott and Co.'s.

3. This relates to a matter of opinion, with regard to which we have made no "misrepresentation;" the most that can possibly be said is, that we are mistaken. We are perfectly satisfied, however, that the mistake lies with Dr. M. Hall.

4. The fourth paragraph shows the constitution of Dr. Marshall Hall's mind to be so utterly inaccurate and confused, as to render him incapable of conducting any discussion in which precision is requisite. We did *not* accuse him, as he erroneously states, of

* Possibly; but not from us.—ED. GAZ.

repeating the term "brutal," in reference to the Royal Society; our words were, "he does not *withdraw* the epithet of 'brutal' from the Council," &c.; and this assertion we made, and now reiterate, on the authority of Dr. M. Hall himself, in one of his published lectures. But farther, Dr. Marshall Hall, while indignantly accusing us of misrepresentation, in having said that he repeated the epithet brutal (which we never did say), makes use of these words:—"I do now repeat, that to refuse to witness my experiments, &c. *was* brutal in the extreme"!! It were a waste of words to argue with a man who seems to be so utterly regardless of all the courtesies of civilized life, and who thus contradicts himself at every turn.

Dr. M. Hall concludes by enumerating certain accusations brought against him by Mr. Newport; but he has omitted one rather important charge brought by that gentleman against him—namely, that of wilful falsehood. His words are, "the assertion is false, and Dr. Hall knows it to be so." When the Doctor next enumerates the charges which Mr. Newport has adduced, and which he has not found it convenient to notice, we advise him to add this to the list.

As we shall not be readily induced to recur to this subject, we may take the opportunity of stating, that Mr. Newport has addressed to us a letter, in which he assigns a reason for the pupil recommended by Dr. M. Hall having left him, which wholly exonerates Mr. Newport, and yet renders it impossible to give publicity to the circumstances without compromising a third party, towards whom the *exposé* would be an act of cruelty. This, however, we may state, that the separation of Mr. Newport and his pupil took place in May, 1834, and that in March, 1835(!), Dr. M. Hall penned the following certificate in favour of Mr. Newport:—

DR. HALL'S OPINION OF MR. NEWPORT.

"I have great satisfaction in bearing my testimony to the merits of Mr. Newport; his papers on Comparative Anatomy, published in the Philosophical Transactions, sufficiently attest his talent for this kind of investigation; but having known Mr. Newport during the course of his medical and surgical studies, I am enabled to say that he has been most assiduous in acquiring both the preliminary and the practical knowledge of our profession. On this ground I recommended, some time ago, a medical friend to place his son as a pupil with Mr. Newport; and I should think the young gentlemen who may in future have the advantage of Mr. Newport's assistance in their studies, will ever afterwards consider it as an important event in their lives.

"MARSHALL HALL, M.D.

F.R.S. Lond. and Edinb. &c. &c.

"14, Manchester Square,
March 20, 1835."

Now, if the pupil was removed in consequence of any disgraceful conduct on the part of Mr. Newport, as asserted by Dr. M. Hall, perhaps he will be good enough to explain how he came to grant that gentleman such a testimonial as the preceding, at a period subsequent to the perpetration of the alleged immorality? On comparing dates, we find that Dr. Hall's sense of Mr. Newport's delinquencies broke in upon him rather suddenly towards the end of 1836, when the Royal Society honoured that gentleman's paper with their medal; and, knowing that Dr. Hall has been a less successful candidate for similar distinction, the impression produced upon our minds we must confess to be, that the Doctor has subsequently viewed every thing connected with Mr. Newport through a medium which has perverted his feelings and blinded his judgment.

ON ANIMAL MAGNETISM,

No. III.

IN our last paper we considered the production of common sleep and of hysteri-

cal paroxysms, by the impressions made on the senses or the mind by the magnetic manipulations; we now propose to consider the several phenomena included under the name of magnetic somnambulism.

We showed that the production of common sleep depended on some common cause, as the *ennui* of a continued slight sensation, or imitation; and induced in this manner, in a person unacquainted with its real cause, but believing it to have been done by the will of the magnetiser, it will follow on each occasion with less difficulty, from the assistance which imagination, habit, and a kind of resigned expectation, will afford to its production. From the fact of its being more easily produced on each succeeding occasion, the magnetisers would deny the influence of imagination, which they say would act with less power each time, but it is evident (and we shall present in a future article several analogous cases proving it) that the reverse obtains; that the imagination once yielded to, acquires a stronger hold, and that its power over one whom it can once subdue is greatly increased by being exercised. We may see too the effect of habit in producing sleep rapidly every day. Most persons go to sleep on particular occasions without any regard to actual fatigue—as under the stimulus of a meal, or in the absence of stimuli on going to bed. It matters not how much or how little exertion of mind or body they may have previously undergone, the majority of persons in health go to sleep within five minutes of the application or removal of the usual stimuli.

Now the character of common sleep naturally produced varies infinitely in different persons—some never dream at all (at least as far as they know); some only occasionally; others have scenes, acts, and thoughts, presented before them with all the clearness of reality; some again seldom sleep without talking on the subjects of their dreams, on which, if another person can discover the clue, he may sometimes lead them into prolonged conversations: others, lastly (but these are far more rare), *walk*, as it is called, in their sleep, and are able during its continuance to perform all the acts of their ordinary occupation, or even others which they would not have dared to undertake, or would have been unable to effect, in their waking state. This

state (for the sake of distinction) we shall call natural somnambulism; its varieties are endless, and we regret that our space prevents us from offering to the reader some of the most remarkable cases which are recorded by authors on it; many, indeed, are, like those of magnetic somnambulism, far too marvellous, but in others, well authenticated, there have occurred phenomena which are of extreme interest.

Another state which we would request the reader to bear particularly in mind, is that of ecstasis, as first described by Sauvages, and of which a considerable number of cases may be found in different authors, under the names of Demonomania, Reverie, Cataphora, Catachus, Catoche, &c., appellations invented by nosologists in a vain endeavour to arrange the varied and anomalous symptoms which different cases have presented; some with lethargy, others with cataleptic rigidity, others with violent convulsions, others with raving delirium, &c. They are now, for the most part, merged into hysteria, for nearly all the subjects were girls, or women subject to that Protean disorder. We shall not attempt to lay down the most frequent characters of the cases we include under this head, but they will appear in the cases we shall allude to.

Other conditions which are occasionally met with, and which it will be well to remember in reading magnetic cases, are those of lethargy, trance, torpor, catalepsy, and day-sleep or day-mare, as described by Mr. Macnish in his *Philosophy of Sleep*. It is necessary to recollect the phenomena observed in cases related under these names, of which at least nine-tenths have occurred in hysterical patients, because we shall be able to show that there has never been a credible effect produced by animal magnetism which has not occurred in cases related under one or other of these names. And, if the exciting causes of these disorders are coincident with the supposed magnetic action, we repeat there is no reason for rejecting the former, whose influence is certain, for the latter, whose existence is altogether hypothetical.

The performance of the common acts of life during somnambulism is constantly observed, perhaps more commonly in this state when naturally than when artificially induced. We need quote no particular instances, for all our

readers must have met with such. It is to be observed, however, that if, before falling asleep, the somnambulist's mind has been much occupied by a plan which he is desirous of accomplishing, or if it have been much affected by some occurrence, then, like the imagination in common dreaming, the acts performed during sleep will take their direction from its previous train of thoughts. Hence in magnetic somnambulism, the patient's mind is filled with thoughts of the magnetiser and his manipulations, of her supposed or real disease, of her previous fits of convulsions, &c.; these will be the subjects of her conversation, and of all her acts. From this *direction* of the thoughts it is that, in natural somnambulism, the sleeper goes about the work that he has been anxiously engaged in or planning, and that in the magnetic state, the patient talks of pain in various parts of the body, imagines that she has obtained an insight into the seat of her own disease, and of those of others; in short, talks of and does all those things which she has heard or imagined to be the consequences of animal magnetism.

It is well known, too, that natural somnambulists have very often no perception of things which have not a reference to the subject of their dream. Thus, in the famous instance of the sleep-walker, Castelli, related by Soave, he heard nothing but what fell in with the train of his ideas, nor did he see any of the persons standing around him as he was reading, though if interrupted in his study he would answer and would carry on prolonged conversations on the subject of it. And so in the case quoted by Bertrand, as related by the Bishop of Bordeaux; in that of Negretti, as given by Muratori; in that told by Dr. Darwin, and in many others, the sight or hearing were awake to those impressions only which had some relation to the subject on which the somnambulist was actually engaged. In other cases one sense only seems to be awake; the somnambulists see every thing, but are insensible to impressions of hearing or common sensation. These phenomena of natural somnambulism are precisely similar to some of the most extraordinary of those stated to occur in the magnetic state—as the *rapport*, said to exist occasionally between the magnetiser and the patient, who will answer none but him, and perceives nothing but what is connected with

him, &c.; the magnetiser and magnetism are, of course, the subject of his dreams, and once dreaming, her thoughts may be led on in the same train. Hence, too, though sensible even to slight impressions which have reference to the magnetic condition, she is totally insensible to more powerful ones (though rarely to the most powerful) which have no such relation. If there be any credible cases of these phenomena, and in our belief there are a very few, they are thus explicable by their similarity with the above analogous ones, which are of more frequent and of natural occurrence.

Another occurrence said to be observed in magnetic somnambulism is that the patient remembers what occurred in the preceding fit, though it had been forgotten in the interval. This also is very generally the case in dreams and in common somnambulism. Such a case is quoted by Dr. Abercrombie, in his Treatise on the Intellectual Powers. The patient, a servant girl, used during the day to have somnambulant fits, in which she would continue her usual work with diligence and accuracy. She used to forget all the occurrences of each attack, but distinctly remembered in one of them the circumstance of having been taken to church during the preceding, which she had throughout the interval been quite unconscious of. The somnambulism ceased on the appearance of the *catamenia*, after six months. More remarkable cases still, perhaps, are those by Dr. Silliman, in the American Journal of Science, of three persons who were suddenly seized with delirium, from which, on their sudden recovery, after some time (in one case after two years), they recommenced the conversation that preceded the attack, or asked after the work in which they had been then engaged. One of them, who had several such paroxysms, used also to recommence at each attack of delirium the subject she had been talking on, and had suddenly broken off from, in the last—so that she appeared as a person might be supposed to do who had two souls, each occasionally dormant and occasionally active, and utterly ignorant of what the other was engaged in in the intermediate time. Hence Dr. Pritchard, who regards the cases as examples of reverie rather than any form of madness, proposes for them the name of *dipsychia*.

The same phenomenon was observed in cases related by Sauvages and Lorry, and is said by Andral to occur in epilepsy, so that in this again, which has been quoted as one of the most extraordinary circumstances in the magnetic somnambulism, we have nothing which does not occur, perhaps more frequently in that state, when produced without the apparent assistance of magnetism.

The insensibility to other impressions than those which have reference to the train of ideas forming the dream, may be carried to such an extent, that means which in the waking state would cause intense pain, may be employed without disturbing sleep. In the commoner cases of somnambulism, whether natural or magnetic, an impression of moderate violence is sufficient to arouse the patient, but in the severer cases of reverie, and still more in those of trance, the most powerful stimuli are insufficient. Thus in cases given by Dr. Darwin, in his *Zoonomia*, vols. 1 and 3: in one of these a girl was attacked by hysteria, in which the convulsions were followed by a reverie, in which she talked aloud with imaginary persons, sang, repeated poetry, and conversed sometimes sensibly on the subject of her dream, yet no notice was taken by her of even the strongest impressions on the ear which did not accord with that subject. So also in Lorry's well-known case of ecstasy, (in the *Comment. Acad. Paris, Mem. des Etrangers*, t. 3) the patient, though she conversed freely with those around her on the subject of her dream, could not be excited by severe pinching and pricking; and in that by Sauvages (*Acta Acad. Upsal.* 742) of Magdalena Valletta, the patient was even more vivacious than when awake in conversation, and in all the acts of her dream, singing, whistling, joking, and dancing, and yet she gave no sign whatever of sensation; a smart blow on the face, a finger moved rapidly towards her eyes, a lighted candle brought so near her open eyes as to burn her eye-lashes, striking hard on the bedstead with a stone, brandy and sal ammoniac placed under her eyes and put into her mouth, tickling the eye, snuff blown into the nostrils, pricking with pins, twisting the fingers,—all these, and many others of a like kind, were quite incapable of arousing her. A similar case is related in Blumenbach, *Bibl.* i. 376. We need only allude to those cases (now nume-

rous) of trance, in which persons have been buried alive, or awakened by the dissecting knife, or have suffered the excruciating pain of pulling off the nails, without awaking—these every one must know by tradition, if not by observation. They are, however, of great importance, for the insensibility, as evidenced by external signs, is perfect, although sometimes there is a consciousness of what is going on, without the power of resisting it. The immobility of the limbs alone seems to distinguish it from the cases above quoted, and from those of *complete* magnetic insensibility; a condition, by the way, far more rare than magnetisers would have us believe.

Such cases are fully sufficient, in our opinion, to counterbalance the most important, because the best authenticated, of all the magnetic wonders; but lest there should not be proof enough of the analogy of the two sets of cases, we will place one of each together. The magnetic one is, we need not say, that in which M. Jules Cloquet removed a scirrhous breast without any evidence of pain on the part of the patient, who had been put into a state of trance by a magnetizer. We need not detail the history of this oft-told case; suffice it to say, the patient was by habit easily put to sleep by manipulations, and was not aware of what was intended to be done. Our case is this; it occurred some few years since, and, for obvious reasons, the names are suppressed:—A young lady, subject to hysterical fits, fell down in one of them, and struck her forehead; she was taken up quite insensible, with her features fixed, her pulse regular, her respiration easy; in short, in a state of deep sleep—of trance. There was a puffy tumor over the situation of the blow, and under the idea that injury might have resulted to the brain or its membranes from the violence, she was trephined after having been insensible for two days. The whole of this painful operation elicited from her *no expression of sensibility*; every thing was found healthy, and the wound was closed again: she remained for two days more in the same state, and then awoke in her usual health. The wound did well, and, with the exception of one or two more similar fits, she experienced no further inconvenience than before the accident, her hysteria being soon after completely cured. On awaking, she detailed all the proceedings of the ope-

ration of which she had been *painlessly sensible*. In this particular alone did the case differ, as regarded sensibility, from M. Cloquet's, and from those before related of Sauvages, Lorry, &c., as well as from those of the fanatics of St. Medard, whose histories we shall take occasion again to refer to at length, who endured apparently without pain, and even called for, the most violent blows from heavy weapons on the most sensitive parts; and Bodin, in his *Demonomanæ*, p. 164, gives insensibility of parts of the body as one of the most frequent signs of *possession*.

Before proceeding to other phenomena, we must observe that if we have not hitherto exercised the privilege to which we have proved our right, of disbelieving very many of the statements made by so many magnetic authors, it has been simply that we have not yet had to deal with any that have not been reconcilable to common sense and probability. Not that we believe more than half the cases that have been told of the occurrence of the phenomena we have already considered, but finding fair evidence of their having been met with occasionally, and produced in appearance by magnetic influence, we are anxious to point out the causes to which they may be more properly referred; and this more particularly for the benefit of those who may hereafter witness or hear of similar cases. The statements we are now coming to, however, will need far more freedom of scepticism.

Among them are those of the occurrence of exalted sensibility. This is seldom noticed, but may have occurred; for it is a common occurrence in hysteria and other nervous disorders. It was present, in a very marked degree, in Sauvages' patient, Helena Renault, (*Observ. Med. Pract.* p. 248, and *Nosology*, vol. iii.) who could perceive the smell of weak ammonia ten feet off her, and went into her ecstatic fits when forced to smell it more strongly. This symptom we have often observed ourselves in hysterical girls, or in persons convalescent from acute diseases; and indeed most antipathies are founded on excessively acute sensibilities of particular organs, or towards particular stimuli. Exalted sensibility to some stimuli has also been observed in many cases of fanatic extasy, and both in them and in magnetic somnambles; and this is of some importance, as it may explain

what small portion is true of the tales about transferred senses.

Increased intellectual powers are said to have been exhibited during magnetic somnambulism, especially in M. Puysegur's cases. We are not disposed to think that much of this was dependent on any thing but the disproportion between the magnetizer's own intellect and his imagination; but there are analogous non-magnetic cases related on as good authority. Two of the fanatic convulsionnaires of Loudun (of whom more hereafter) were known during their fits of ecstasis to converse in Latin with fluency—a language of which, in the intervals, they knew scarcely more than the rudiments. This resulted, however, from the perfection which the memory sometimes acquires in cerebral affections; no new language was ever acquired. The fanatics of Cevennes, for example, are said to have talked polite French in their fits, and *Patois* in the intervals, but they never talked German; the new languages they spoke were like the unknown tongues of our own day, unintelligible—a mere babbling, which the hearers supposed it was their misfortune not to understand. The most remarkable case of perfection of memory by cerebral disorder, occurred some years since at St. Thomas's Hospital. A man fell on his head, and remained for some time insensible; on recovering, he spoke a language unknown to all around him, and could not in any way make himself understood, till a Welch milkwoman, coming into the ward, recognized her own dialect. It appeared now that he had left Wales, where he was born, at three years old, and had totally forgotten the language till he met with the accident; the blow which had knocked out all his English, restored him his Welch. When he recovered, however, he again lost the latter, and regained the former. Now had this man been magnetised, one might indeed have said "*Quel prodige!*" Similar though less remarkable cases of improved memory are sometimes met with in common delirium. And in dreams, during the absence of every thing that might distract the attention, not only are forms and ideas recalled to the mind with unusual accuracy, but the imagination is exalted so as to attain extraordinary power. We need only allude to the composition of the well-known sonata by Tartini, to which he imagined himself in his dream to be

instigated by the devil, and to the many instances in which thoughts of peculiar beauty have suggested themselves to imaginative minds during sleep. Had they occurred in magnetic somnambules they would have been deemed peculiar to it.

Other extraordinary intellectual powers, shewn by somnambules, have been, an insight into the diseases of others, and a discovery of their remedies; these were especially enjoyed by the patients of the philanthropic Puysegur. As he lived, however, during the popular prevalence of humoral doctrines, they saw every thing, as it were, through humoral spectacles; and they diagnosed actions, acrid matters, and other things, whose existence is about as evident as that of the magnetic principle itself. The remedies, too, which they ordered, were of the simple and efficacious character generally known in the class in which the patient lived, as vegetable purges, &c., on which the patients of course sometimes recovered. When Broussais was in the ascendant, nothing but gastro-enterite was seen—the intestines of every patient were red or ulcerated, and leeches and gum-water were the only prescriptions. We would venture privately to predict what will be the discoveries and remedies of the first physician-somnambule that is met with in London. This medical power, however, is but an imitation of that long ago enjoyed by the *possessed*, and various fanatics; and it is singular that the words in which Corré de Montgeron describes it in the *convulsionnaire* of St. Medard are almost identical with those in which it was first announced as enjoyed by somnambules. It needs no explanation.

ROYAL COLLEGE OF PHYSICIANS.

Monday, March 5, 1835.

THIS was the second meeting of the season, and was numerously attended. A paper, containing many interesting facts on the subject of Heat, by Sir Charles Scudamore, was read. We regret that we have not room for it this week, but we shall, if possible, give it in our next number.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

March 12, 1838.

THE PRESIDENT IN THE CHAIR.

READ: a paper by Dr. Travers Cox, of Yarmouth, descriptive of

A Cyst found at the under Surface of the Liver,

on dissection of a patient of whose case the following history is given:—

The patient, a muscular man, of 32 years of age, came under the observation of the author, in 1832, while suffering from ascites, for which he was tapped, when twenty one pints of fluid, resembling ordinary bile in consistence and colour, were drawn off. The patient afterwards took iodine and diuretics, and no fresh accumulation took place. He continued at work, as a coal heaver, from this time till April 1836, when pulmonary disease manifested itself, and he died of hæmoptysis in November last. On examination of the body after death, a large distended cyst was discovered at the under surface of the liver, attached by a small portion of its surface to the right of the gall-bladder, and in front of the transverse fissure. The cyst a preparation of which was exhibited, was about four times the size of the gall-bladder, contracted in its centre, where it was cartilaginous, and was found to contain a gelatinous mass composed of concentric layers capable of being unrolled from each other. The outermost of these were nearly transparent, those in the centre being opaque. Between these fibrous layers lay here and there small pieces of a substance resembling wax and vermillion, prepared for injecting arteries in the dissecting-room, both in consistence and colour.

Read also,

Observations on the Dislocation of Phalanges of the Fingers and Toes. By JOHN P. VINCENT, Esq. Surgeon to St. Bartholomew's Hospital.

The object of the author, in this paper, is two-fold: first, to point out the principle upon which the reduction of the phalanges of the fingers and toes should be conducted; and secondly, to call the attention of surgeons to the manner in which the force of that portion of the muscles which remain uninjured may be made to co-operate with their efforts in the reduction of dislocations in general. With reference to the former point, the author

insists strongly on the propriety of extending the dislocated phalanx, in a direction at right angles to the bone from which it has been dislocated, instead of employing the force in the axis of the bone. These observations are illustrated by a diagram, in which it is shown that the lateral ligaments, supposing them to remain entire, oppose an insurmountable obstacle to the reduction in the latter direction.

With regard to the second part of the inquiry, the author adduces reasons for believing that, in the reduction of other dislocations, the operator can do no more than bring the head of the bone down to the acetabulum or glenoid cavity; after which, its replacement is effected by the agency of the muscles which are commonly employed in moving the bone upon its axis. The paper concludes with some observations on the effect of position of the body in keeping fractured bones in apposition, and relates an instance of recent occurrence, in St. Bartholomew's Hospital, of fracture of both clavicles, where no bandage at all was employed in the treatment, and the patient was discharged in a month with the bones well united.

Read also, a paper on

The Perforation of the Third Lobe of the Prostate Gland. By R. A. STAFFORD, Esq. Surgeon to the St. Marylebone Infirmary.

The author recommends this practice to be adopted in cases of enlarged third lobe of the prostate, considering it quite free from danger. He relates an instance in which he had performed the operation in a man of 82 years of age, in whom it appeared to him to produce no bad effect. The patient died shortly afterwards of apoplexy, when the author had an opportunity of examining the bladder; a preparation of which was exhibited.

PERFORATION OF THE ACETABULUM, CAUSED BY A FALL ON THE TROCHANTER.

M. GAMA has given a case of the above-mentioned accident. It occurred to a man æt. 30, who had fallen from a height of eighteen feet, and who had suffered a severe contusion in the region of the right trochanter. There was neither shortening nor deformity, but there was very severe pain at every movement of the limb. Some days afterwards the patient walked in the garden on crutches, and said that he felt very little pain. On the fourth day symptoms of violent peritonitis, with inflammatory swelling of the entire limb, came

on. On the dissection, infiltration of pus was found in the immensely distended subcuticular cellular tissue, from the hip to the calf of the leg. In the right iliac fossa a conical swelling arose, and extended nearly to the kidney; this was an abscess containing pus and torn pieces of the psoas muscle, and at the bottom of it the head of the femur was to be seen where it had burst through the acetabulum. The acetabulum was broken in three pieces; the smallest of these was placed with the round ligament upon it, and unbroken on the head of the bone; the second was the horizontal ramus of the pubis which was separated from the symphysis pubis and ischium; and the third, the ischium, which was no longer connected with the ilium.—*Gazette Medicale, and Dublin Journal.*

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, March 15, 1838.

Thomas Birt, Stroudwater.—Loraine Weaver, Liverpool.—George Ebenezer Ely, Rochester.—Thomas Cam, Bath.—Charles John Robert Cook.

Mr. W. R. Rogers passed his examination on the 1st of this month. The name was, by some accident, omitted in the printed list.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, Mar. 13, 1838.

Abscess	1	Hæmorrhage	1
Age and Debility	36	Whooping Cough	8
Apoplexy	8	Inflammation	19
Asthma	13	Bowels & Stomach	2
Childbirth	5	Brain	1
Consumption	36	Lungs and Pleura	8
Constipation of the		Insanity	1
Bowels	1	Liver, diseased	1
Convulsions	22	Measles	7
Croup	1	Mortification	6
Dentition or Teething	8	Paralysis	7
Diarrhœa	1	Small-pox	10
Dropsy	6	Sore Throat and	
Dropsy in the Brain	6	Quinsey	2
Dropsy in the Chest	1	Veneræal	1
Epilepsy	1	Unknown Causes	25
Fever	15		
Fever, Scarlet	1	Casualties	5
Fever, Typhus	2		

Decrease of Burials, as compared with }
the preceding week } 390

NOTICES.

The communications of Dr. Borrett, Dr. Thompson, Mr. Greenhow, "Z," Dr. Ward, and Mr. L. Parker, have been received.

The letters relative to Mr. Dermott and his pupils, can only appear as advertisements.

WILSON & SON, Printers, 57, Skinner-st., London.

THE LONDON MEDICAL GAZETTE,

BEING A
WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MARCH 24, 1838.

LECTURES ON TUMORS,

Delivered at St. George's Hospital,

BY CESAR HAWKINS, ESQ.

LECTURE V.

III. SARCOMATOUS TUMORS (*continued.*)

On the Nature and Properties of Malignant Diseases.

9. *Verrucous Tumors of Cicatrices.*

10. *Carcinomatous Tumors.*

Definition.—Various Forms of Carcinoma. Period of Life,—Hereditary?—Exciting Causes.

History of a Cancerous Tumor.—Treatment, General and Local.—Operation for.

WE have now discussed eight different genera of the third order of the class of tumors; namely, the adipose, fibrous, neuromatous, conglomerate, conglobate, scrofulous, cystic, and tuberculous cystic tumors, and we have found that they are all innocent, *i.e.* nonmalignant; not possessed of any contaminating influence, even of the lowest kind, so that they are all local diseases in this respect; unless it be that the conglobate and the tuberculated conglobate tumors are malignant; but I am rather inclined to call even those tumors incurable affections of the absorbent glands, than really malignant diseases. I do not think there is sufficient evidence that either of these tumors, nor the cystic, nor tuberculous cystic tumors, possess the power of causing the same structure as their own to be formed by their influence in the surrounding tissues, nor in the absorbent glands, nor in the whole system. But, at the same time, we have found that they are not innocent in their effects on the patient; on the contrary, they may be

fatal to life itself, by irritation, by ulceration, sloughing, and bleeding, unless they are removed by operation, which may be done with safety with all of these tumors, except sometimes the tubercular conglobate tumor, if the operation is done early, and the tumor is accessible to the knife.

But you must not forget that all tumors may become malignant, although they are not so originally; and perhaps they are in themselves a source of malignancy; that is to say, if the constitution of an individual is disposed to malignant action, and happen to have a tumor in any part of his body, that part will be selected for the development of the constitutional taint; just as, if a patient deranges his system so as to dispose it to form ulceration somewhere, the new structure of a cicatrix will be the first, in all probability, to give way. Thus a mole, or a fatty tumor, may become malignant, and go through all the changes of a malignant tumor: a man was admitted into the hospital, for instance, under Mr. Babington's care, with a fungous tumor of the size of the fist, which, for forty years, had been a small mole unchanged in size and nature. A new action is suddenly developed, and a new character assumed by the part, and it now gains the property of contaminating the structures around, and making them like themselves in appearance, and of contaminating also the glands, and of poisoning the whole body. I need hardly say, then, that this observation is a sufficient reason for your taking away even innocent tumors, if the patient can be induced to submit to their removal, lest they should become a cause for the development of malignant action, which the patient might have escaped if no morbid structure existed.

I have used the expression malignant condition of the whole system; now, what are you to understand by this? You will recollect what was formerly told you as to

the different degrees in which malignant action is exhibited; first, it may be apparently entirely local, so that forty years may leave a patient with only one part affected. Secondly, the glands to which the absorbents come from a malignant tumor may be enlarged in consequence of their connexion with that tumor, and yet the general health may still remain pretty good; but, thirdly, the health may give way nearly simultaneously with, or even before there is any evidence of local disease at all, shewing, in the latter case, that the constitution must have been tainted before the formation of the tumor. But now comes the important question, whether any malignant disease whatever is really local, or whether the constitution is always affected first, as it unquestionably is sometimes?

It was said, I believe, first by Mr. Wardrop, with regard to fungus hæmatodes, and the opinion is elaborately supported by that zealous pathologist Dr. Carswell, as to all malignant tumors, that the actual matter of the disease circulates in the blood, and the arguments in favour of the opinion are threefold:—first, because the new substance is deposited by the capillary vessels, nutrition and interstitial absorption going on together, so that the malignant matter simply occupies the place, and assumes the form of the original materials of the body; an argument that goes for very little, as it would apply equally to cartilage or urine, though nobody goes further with regard to such substances than to affirm that their elements are contained in the blood, and separated by the capillaries in a new form from that fluid.

Secondly, because the matter of malignant disease is found in the blood-vessels; you will see in these preparations the trunk of the *vena portæ*, and all its branches in the liver, completely filled with solid substance, so that the circulation must wholly have been interrupted through the vessels. But this also is no proof that the blood has contained the peculiar substance in that shape. *a.* In the vessels, as well as elsewhere, the malignant structure is deposited, in all probability, by the capillary branches of those vessels, as in ordinary nutrition, and it is therefore generally organised and adherent to the sides of the vessels, by means of organised communication. *b.* It gets into the vessels by ulceration through their coats, and then spreads rapidly, from its being subjected to less pressure: you may see here a little portion of tumor just ulcerating into, and hanging loose in the *vena cava superior*; another portion is ulcerating through the pericardium, and the gentleman from whom I took the preparation, who was a

patient of Dr. Scymour's, died suddenly while at the closet-stool, by hæmorrhage into the pericardium. *c.* Sometimes, again, malignant matter gets into the vessels by absorption; I have seen it, in the absorbent vessels, coming from a tumor, in its passage to the glands, and I believe it is found in the same way in the small veins; but this is *after* it has been secreted by the capillaries, just as pus is found in the same vessels, and thus you have one mode of contamination of the system.

Thirdly, the only probable proof that the malignant matter exists in the first instance in the blood, without having been previously formed, like every other part, by the vital action of the capillary vessels, is the circumstance of an effusion being observed in a soft condition, in some places consisting of a mixture of blood and malignant matter. But I have not seen this satisfactorily made out. I have often, indeed, seen a mixture of fluid malignant matter mixed with blood, but only when it appeared likely to have been formed previously to the rupture, either in the cellular substance or on the surface of a membrane,—in the same way as you may often see a mixture of purulent matter and blood, without adopting the supposition that the pus had been mixed with the blood, so closely combined as to be dropped from one another, as it were. The pus and the malignant materials are, surely, alike formed out of the elements of the blood, by the capillaries in some part of the body, before these respective substances become mixed, as such, with the blood in the circulation.

See, now, what an important question this is in practice: if the whole system be so poisoned, as actually to contain perfect malignant matter in the circulation, before it is deposited anywhere, either in the molecular structure, or as a secretion, there can be no such thing as a local malignant disease; no operation can consequently be performed before the whole system is tainted; the blood always circulates the poison. I trust, however, that there is often a predisposition only, or tendency, to malignant disease, just as there often is to scrofulous action, before any serofulous matter is formed; I trust, therefore, that a tumor composed of malignant substance may sometimes be local, aye, and for several years, during which time the blood does not contain any formed malignant matter; but, at last, if a further influence is exerted on the system, or if the matter already deposited is absorbed and mixed with the blood, so as to deteriorate that fluid, the patient has now passed the boundary within which an operation affords any security against a return of the malady. Sometimes, on the

other hand, there is not only a slight predisposition, but the blood has so strong a tendency to separate those elements which, when united, constitute malignant structure, that the whole system may be said to be in a malignant condition, before the vessels have yet formed any local tumor; or, if a tumor has been formed, although no malignant matter has yet been absorbed, so as to be mixed with the blood, yet an operation is useless, or worse than useless, and may hasten the formation of the same structure elsewhere in the body.

Now, there are several forms of malignant disease which are, for a greater or less period of time, merely local, especially in the skin; one such form I shall hereafter speak of, with the other diseases of the face. There is also another, which is so peculiar in several respects, that I shall speak of it, a little out of its place perhaps, as another genus of sarcomatous tumors, under the name of

III. 9. *The Verrucous Tumor of Cicatrices.*

I think it deserves separate consideration, because it has not been well described in surgical works, a circumstance which induced me to publish a series of cases which I had seen of the disease in the 19th vol. of the *Med.-Chir. Transactions*, to which I may refer you for a more minute account of it than our time will at present allow.

It belongs to those cases which are called, sometimes, semimalignant; but, for my own part, as I think it is essentially carcinomatous in its nature, I prefer the term locally malignant, by which I mean that it is a kind of cancer which contaminates only the neighbouring textures, and does not affect the glands; at least, I have not yet seen it do so. I have never seen it, except in the cicatrices of old injuries, and I think it probable, therefore, that the peculiar texture of the new skin may influence its growth, making it occur where the constitutional tendency is less than would be sufficient to develop malignant disease in sound parts, and causing it also, when formed, to differ from the malignant tumors of original and sound skin. The injured parts may remain quite well for many years, or may frequently thicken with common inflammation, or ulcerate from the usual causes, and with the common appearances, before the peculiar changes in question evince themselves.

The warty tumor of cicatrices presents three successive stages to your notice:—1. Here is a tumor which formed in the scar left by a flogging, and was removed by Sir Benjamin Brodie, from a man who had been sentenced, eleven years before, to

receive 1,000 lashes. It was vascular, warty in appearance on the surface, scarcely ulcerated, but discharging an offensive watery secretion; in this particular case there was very little pain, but the pain is sometimes considerable. You may see, also, little elevations of the same character around the larger portion of the tumor. The section of any portion of the tumor in this condition shews you a firm hard structure, of a dense white colour, rising in a fibrous manner, perpendicularly, from the substance of the cutis.

In the second stage, the tumor becomes less warty in appearance, and is composed of a mass of rounded fungous elevations, still very solid, but less fibrous, when cut into, than before. You may see it in this preparation, which was removed from a man's back by Mr. Jeffreys, and which also followed a flogging received 27 years previously. Or you can see the nature of the disease still better, in this plate and preparation, from a young woman of 28 years of age, who was under my care. This is the earliest age at which I have seen it, and it followed a burn received when she was a child, which had healed, and left a scar from the toes to the knee. It had broken out into an ulcer, six months before her admission into the hospital, which had generated a luxuriant mass of fungous warts, accompanied with the most intolerable pain, and a very offensive discharge, under which her health seemed rapidly sinking. She refused the amputation which I had proposed to her: but one day, on coming to the hospital, I found, from the house surgeon, that she had told him if I would perform the operation directly she would submit to it, but that she would not have it done on any future occasion. You see a large mass of fungous growth, two inches in thickness, and nine in diameter, and the section still shews, in some parts, very well the fibrous warty nature of the disease; you may also perceive that although of this large size, and attached to the periosteum, the morbid growth had produced no alteration, except a little inflammation in the bone below. This woman remained quite well two years afterwards, and had no return of disease in a part of the cicatrix, which I had left, in order to amputate the limb below the knee.

In the third stage, the fungous growth ulcerates, and leaves a foul ulcer on the level of the skin, and spreads by the edge around, when the skin goes on forming warty elevations of the same character. Here is a very beautiful preparation of the disease in this state, which I took from a man whose limb was removed by Mr. Gunning, when I was house surgeon, and

in whom it formed in consequence of a gun shot wound. A patient was under my care for a very extensive ulcer in the back, following a burn, which had been healed eleven years; it was too late for operation, being ten or twelve inches in diameter, and the man fell a victim to the intolerable pain it produced. Even then, however, it had not extended below the fascia, and there was no sign of morbid structure in any part of the body.

I have seen various methods tried to destroy the morbid growth, many of them on the supposition of the disease being only an unhealthy ulcer, but without avail. Looking upon it as a malignant disease, it is evident that we ought not to expect any measure to succeed which does not insure the entire destruction of the new growth, which can, of course, only be effected by strong caustics, or by the knife. Of the caustics, the best is the chloride of zinc, but I much prefer the excision of the tumor, if that is practicable, by the knife, or the amputation of the limb, when the disease is too extensive for excision. The patient remains, I believe, free from a return of the disease, as he ought to do if the disease is only locally malignant, and as he has done, as far as I know, where I have seen the removal of the disease practised.

Separating, then, for convenience sake, the verrucous tumor of cicatrices from cancer, of which it is probably only a variety, the two remaining genera of tumors are malignant in the highest degree, affecting not only the surrounding textures, but the absorbent glands also, and poisoning the whole system; and most of the remarks I have to make upon them, apply equally to the disease, whether there be, strictly speaking a tumor, or whether the new structure be only an interstitial deposit, as in the cancelli of the bones, or the textures of the internal organs, without increase of their bulk.

III. 10. *Carcinomatous, or Cancerous Tumors.*

Surgical writers have often divided this disease into two stages:—1st, Scirrhus; 2d, Cancer; terms which are synonymous with the expressions, occult and open (or ulcerated) cancer. Scirrhus is a bad name, however, to retain, since it is used by the older authors to designate any hardness which is the result of inflammation or disease. I should rather say the older English authors, for it is still very often used by foreign writers, without any definite idea of the disease as an affection of the system; some of the French, for instance, often speak of scirrhus and cancer from syphilis, a sequel of the latter disease purely imaginary. Unfortunately, these efforts of the

imagination are not confined to the mind, but are carried into surgical practice, and nothing is more common than to hear of a great number of operations for cancer of the rectum, in which that gut (sphincter and all), has been cut out for simple ulceration, or of a great number of cases of excision of the cervix uteri, in young women of eighteen, for supposed cancer of that part. Some, indeed, instead of dying of cancer, have survived the operation, and have borne children afterwards, grateful, doubtless, to their operative friends for the preservation of their lives! such cases, no doubt, having been instances of hardening and enlargement of the os uteri from simple inflammation, for which some persons might prescribe caustic, and some matrimony; and either remedy, I dare say, would be pleasanter than excision, and quite as efficacious.

By *carcinoma*, then, I mean the generic name applicable to all forms and stages of the disease, whether there be a distinct tumor or not, and whether the part be recently formed, or extensively ulcerated. Almost every structure in the body is liable to carcinoma, but some textures are more disposed than others to the disease; it is very common in glandular organs, such as the breast and liver, which you may here see; and, where mucous follicles abound, as in these preparations of the cervix uteri, the cardia of the stomach, the œsophagus, the colon, the rectum: you may see it here in the lungs; here in the skin, and at the junction of the mucous membranes with the skin, as in the penis and labia; in this preparation you perceive it in the serous membrane, the pleura: and in this in the bones. And, in whatever tissue it originates, the morbid structure affects, by degrees, all the contiguous textures: you see in this plate, for instance, and in this preparation, that it implicates distinctly all the coats of the stomach, after having begun, as it would appear, in the mucous membrane.

Cancerous structure affects several different forms, and varies much in its appearance. *a.* The most usual form of an external tumor is that of a tolerably circumscribed tumor, which is generally firm and hard in texture, something like cartilage, sounding under the knife when cut: running through this you can distinguish a number of bands, or rays, of a white or brownish white colour, from which the disease derives its name of cancer; these bands extend to the neighbouring structures, however, from the tumor, and are the result of carcinomatous deposit in the cellular tissue, which is much disposed to the disease, so that you will occasionally see the skin connected to the tumor by these cancerous bands, while the interme-

diate fat remains healthy. It seldom, therefore, happens that a tumor is really as circumscribed as it appears to be; it is not confined by a cyst, but these bands are sufficient to propagate the disease after the tumor itself has been removed. In the interstices of these membranous bands a cancerous tumor is less compact, and you can squeeze out a milky fluid, or a transparent liquid like glue. *b.* At another time the tumor is less solid and has few bands, and resembles what was called, by Mr. Abernethy, the udder-like or mammary tumor. *c.* Sometimes the tumor has a number of cysts within it of small size, and containing mucus, or a bloody fluid, or a liquid like chocolate in appearance, and in a large cell fungous excreescences of cancerous structure are sometimes found. *d.* In another instance, especially in the hollow viscera, as in these instances, it is soft and transparent, constituting gelatiniform cancer, or cancer areolaire, as it is called by Cruveilhier, or the *matiere colloide* of Laennec. *e.* In other cases, the cellular bands are almost entirely wanting, and there is a larger quantity of unorganized greasy secretion in the interstices of the cellular membrane, making the lardaceous tumor, such as you here see in the lung and liver. *f.* In other cases, any of these structures will affect a distinct tubercular form, with a surrounding cyst more completely insulating the diseased substance than usual, as here in the liver or lung, or in the form of a number of little tubercles around the original tumor, without any apparent connexion with it. *g.* Or, lastly, you will find millions of little hard rounded or flattened tubercles in the serous membranes, especially the pleura, as in this instance, projecting into the serous cavity. But there is no essential distinction in the disease from these variations of character, and you will meet with all these forms of structure in the same person in different organs or tissues, and several of them at once, in separate parts of even the same tumor.

A characteristic distinction of cancer from other malignant tumors, is, that it is a disease of the middle period of life, from about 35 to 55; it is very seldom met with under 30 years of age: I have only once seen it in a young woman under that period; but no rule is without exception, and Sir Everard Home met with an instance of a girl who, when 15 years old, received a blow on the breast, which left a hardness in the injured part, and at 20 this lump began to enlarge, with the usual signs of cancer, and was subsequently removed by operation. So also cancer sometimes shews itself after 60 or 70 years of age, but seldom, except in the skin, at such an advanced age.

Cancer appears much more frequently in females than in men; it is said, in the proportion of ten to one; and in females (in whom it takes place chiefly in the uterus and mamma), the disease occurs very often about the time that menstruation begins to cease, and when these organs are no longer useful. It is said, too, by many persons, that cancer of these parts occurs more frequently in unmarried females than in those who have borne children; as if these organs, when their natural functions are not called into action, are like children, who cannot remain idle, and are therefore always getting into mischief, if not properly employed. I am, myself, however, very doubtful if this observation be well founded, or whether even the reverse may not really be the case.

It seems not improbable that cancer is, to a certain extent, hereditary, so that you will often find the parents and children in a family, successively, have the disease, or several sisters will each die, at a certain age, of the disease. Of course, from what I have before said, you are not to understand that cancer is born with them, or that cancerous materials exist in their blood from birth, but only that a certain peculiarity of constitution is hereditary, so that when any exciting cause is applied, cancer is more likely to shew itself in them than in persons who have not this peculiarity: just as persons with an hereditary predisposition to scrofula cannot expose themselves to cold with impunity; though no one imagines that scrofulous matter is circulating in the vessels.

The cause of the development of a cancerous tumor is, then, the existence of such a state of system as that just described; but the situation chosen is frequently attributable to some external injury, or other exciting cause. It frequently takes place in the hardness left by inflammation, as by a blow on the breast; it often arises from the local irritation of soot upon the scrotum, or of a tobacco-pipe upon the lip. Sir Everard Home met with this remarkable case: a sailor contrived to get his penis squeezed by a piece of wood, and flattened like a half-crown, an accident which, as you may imagine, made the man faint from pain, and produced inflammation of the penis and testes; he recovered, however, except having a small pimple, which, six months afterwards, ulcerated in the form of cancer of the penis; this was followed by a tumor in each groin, which ulcerated also, and in four years he died in this hospital, from frequent bleeding from the ulcerated parts; and, after death, a number of glands were affected in the lumbar region in the abdomen, and in the mesentery, liver, and other parts. Sir Everard Home went

so far as to believe that cancer never was developed unless in a part which had been injured, or diseased in some manner or other, previously, in which case, the morbid alteration of a simple kind, the pimple or wart, or whatever it might be, became afterwards cancerous; but he was, no doubt, wrong in thus supposing that it did not occur in healthy parts, and you will find cases, not unfrequently, in which, with all their fondness for finding local causes, your patients can assign no reason why any particular locality is attacked.

Cancer, as it is modified by texture and situation, will be more particularly described to you hereafter, with the diseases of the breast, the scrotum, the lip, the eye, and so on, and in your medical lectures; but let us now trace the progress of a cancerous tumor, in any part of the body, in its common form below the skin.

A cancerous tumor, in its early stage, feels like a firm, hard mass, circumscribed and distinct from the organ in which it is situated, but yet generally giving you the sensation of its being intimately attached to other textures; sometimes moving freely below the skin, and upon the deeper parts; but this circumstance also is doubtful even in the early progress of the tumor: the tumor is generally irregular on the surface, with somewhat angular projections, but in many cases where the interstitial fluid is in considerable quantity, so as to make the tumor softer, its exterior feels quite smooth and uniform. It has grown to a certain size without being observed, and without pain, but as it increases further, it is attended with a good deal of pain, which is described as acute, lancinating, stabbing, darting, and so on, expressing its irregularity in degree and kind; the tumor is not very tender in general, but the pain is much increased afterwards by handling; sometimes the pain is most at one part, and this occasionally from a nerve stretched over the tumor.

The pain is considerably aggravated as the second or ulcerative stage comes on. The tumor now becomes attached to the skin, which is generally puckered and contracted at the point; then it becomes red and ulcerates; or a part of the tumor grows more rapidly than the rest and projects, and becomes of a dark colour; it gets thin, and gives way by being stretched as it were; it does not suppurate, but simply ulcerates and wastes away. The discharge is thin and watery, or an imperfect pus mixed with mucus, or it is ichorous, and has a peculiar foetid odour. The ulcer is flat, and the edges thin and superficial, and increases gradually, as represented in this cast; or it has sloughs formed upon it of a dark colour, with fre-

quent hæmorrhage, as in this cast, and the edges are sometimes thick, raised, and everted, as is shewn in this cast. Sometimes ulceration is established in the interior of the tumor, instead of on its surface, and suppurates imperfectly. I attended a patient, with Mr. Peregrine, in whom a considerable sized cavity was thus formed, with such an extent of hæmorrhage from the inner surface that the cavity was obliged to be freely laid open to control the bleeding; then the disease became quiet and nearly dormant for above a year, but it has since been fatal.

Now commences the third stage perhaps. Hitherto the general health may have been not much affected, but after the ulceration the patient begins to droop, looks thin, emaciated, haggard, with a dulness of eye, and relaxation of muscle, and has a peculiar sallow, or leaden hue in her countenance, which is well depicted in this plate of Alibert's, or in this cast, and which communicates an expression which the experienced eye of the surgeon recognizes as indicative of a malignant condition of the system, perhaps before much complaint is made by the patient herself. If seen there is now no hope of cure from surgical operation, and the unfortunate sufferer is condemned to drag on a miserable existence, sensible, too, of the disgust created in the feelings of her attendants, by the appearance and smell of this horrible complaint. The condition and state of mind in a cancer ward is well depicted by Milton:—

“Despair

’Tended the sick, busiest from couch to couch,
And over them triumphant Death his dart
Shook, but delayed to strike, tho’ oft invoked.”

The time which elapses before a fatal result varies very much; it is influenced by the texture in which it arises—by the organ in which it is situated; it depends on the kind of carcinomatous formation in each case, and various modifications of the disease will be described to you by and by, which will influence your practice, especially in the breast. For the most part, about three or four years will be occupied from the first discovery of the tumor. The last scene is sometimes very rapid: a woman, for instance, came under my care with a tumor, which she discovered a year before, and which was just beginning to ulcerate, and in three months from this time she died with most extensive disease in both breasts, and in the skin and glands, and in the internal viscera: and sometimes your patient will die in a few months from the appearance of a tumor, when there is no evidence of internal disease to hasten the event. On the other hand, cancer sometimes proceeds very slowly: a woman was

three weeks under my care for some other affection, suffering so little, that she allowed this time to pass before she even mentioned to me that she had had a cancerous tumor in one breast, which had been ulcerated eleven years, and had existed twenty; she had also a tumor of the same kind in the other breast, of four or five years' standing, and had a little cough, that appeared, perhaps, to be connected with disease in the chest.

Cancer runs its course, I think, quicker in young persons than in old; I knew a lady who had had a tumor forty years, ulcerated several years before she finally died of hydrothorax, at an advanced period of life. It is more rapid, perhaps, in fat and bloated individuals, than in those of less ample dimensions. In short, there are great varieties in different cases of cancer.

I need hardly say, after this account of cancer, that we possess no cure for it; the removal of the diseased part, before the system has become affected by it, affords us the only chance of safety from the complaint. But even when the operation has been considered improper, or has been ineffectually performed, we can still do much, both locally and constitutionally, to alleviate the patient's sufferings, and to retard its final result.

Unquestionably, whatever debilitates the body, or irritates the mind, accelerates the appearance of the complaint, and hastens its progress. Anxiety of mind, such as that induced by poverty, or the loss of children, or other dear relations, is often the immediate exciting cause; and it runs its course quicker in persons of irritable constitutions, and occasions much more suffering.

Mr. Pearson, who is a high authority on some medical subjects, recommends a starving diet, and depletion by purging, and so on, to retard the progress of cancer. More modern experience, however, has fully established the reverse, and I advise you to recommend a moderately nourishing diet. Whatever stimulates does harm, such as wine and bark, if any heat or fever are caused by them, but, on the other hand, whatever lowers the tone of the system accelerates the rapidity of the disease. A young woman, 29 years of age, was under my care, as an out-patient of this hospital, with a cancerous tumor of the breast, and enlarged glands of the axilla, and the state of the parts altogether induced me to advise her not to have an operation performed: after some time she changed her residence, so as to make it more convenient to her to attend at another hospital, where she was kept on low diet, and had a great number of leeches applied during

six weeks, at the end of which time the pain was intensely increased, and she was emaciated, and apparently dying. I immediately put her again on a more generous diet, and gave her a course of sarsaparilla, and in a short time she had regained her flesh and strength, and nearly lost the pain in the tumor. Subsequently, of course, the tumor ulcerated, and she fell a victim to the disorder, but her life was certainly prolonged considerably, and her condition rendered much more supportable.

The medicine I have just mentioned is, on the whole, the best, I think, given in considerable quantity—a pint of the comp. decoction, with a dram of the extract daily, if the patient bears it, and continued for several weeks, whenever the health appears to be sinking, before the irritative fever of the last stage comes on. If the countenance is assuming the peculiar aspect I have alluded to, about $\frac{3}{4}$ of a grain of the oxymuriate of mercury, or five grains of Plummer's pill, or $1\frac{1}{2}$ grains of calomel and a grain of opium, is of service with the sarsaparilla. Sometimes other tonics are of more service, such as gentian, or calumba, which may be joined with soda, or potassa and rhubarb, where the appetite is beginning to fail. Sometimes small quantities of carbonate of iron, or the comp. steel pill, will agree, but not so often as the less heating and stimulant tonics. I have seen the malignant state of the system repeatedly disappear, for a time, under this plan; I have even seen the tumor become much smaller, so as to excite delusive hopes in the patient's mind of final recovery.

Such a method of treatment as I have now recommended is of service to the patient under several different circumstances, though it will not cure the disease: it is useful before an operation, enabling the patient to bear it better; it is of service after an operation, by delaying, at least, the recurrence of the disease in some other situation; and when an operation is altogether improper, the case I narrated to you shews what benefit it may confer upon the sufferer, by retarding the progress of the complaint. Almost the only thing you can do besides, is to soothe the patient's sufferings as much as possible by opium and conium, and other narcotics, with a little coloeynth, or other purgative, to prevent the constipation and diminution of the secretions, which attends their use when employed singly.

Locally, also, you can do much good. When slightly inflamed and hot, a weak spirituous and gently stimulating lotion, used cold, or tepid under an oiled silk, according to circumstances, will agree much better than poultices and fomenta-

tions; warmth generally increasing the pain. Occasionally, a few leeches are of service applied around the tumor, not upon it, especially when the skin is thin and inflamed, otherwise the ulcerative process is hastened by the leech-bites; and you must take care, at the same time, that they are not applied so often, or in such numbers, as to weaken the patient by loss of blood.

In the early stage of the tumor, when not inflamed, a warm plaster spread on leather, and placed over and around the whole tumor, gives frequently great relief; it may be the mercurial, or opium, or soap, or ammoniacum plaster; or, when the pain is severe, a plaster consisting of one-third part of extract of belladonna, with two-thirds of soap plaster. The opium and belladonna are both, however, offensive to some patients, and induce headache; and the latter, in large quantity, will dilate the pupil and cause headache and other symptoms of its narcotic influence. I have seen the pain, when of three years' duration in a tumor in this stage, kept quietly within moderate bounds, merely by these applications.

Pressure has been recommended as a means of cure, by the late Mr. Young, but I believe it only lessens the tumor by rendering it more solid, when it would otherwise have had fluid within it, and by causing the absorption, not of the morbid structure, but only of the ordinary effects of inflammation.

When the tumor is ulcerated, the fœtor is subdued and the sore made more healthy by the chlorides of lime or soda in solution, and they check the sloughing process; a charcoal poultice, or the yeast poultice, will do the same. The pain of the sore is much relieved by a poultice of fresh hemlock leaves, if you can procure them, or by a solution of the extract: $\frac{1}{2}$ a drachm to $\frac{1}{2}$ a pint of water, may be used as a lotion, or in a poultice. The woman from whom this cast was taken derived more relief from the following liniment, than from any other application, which you may rub down and spread on lint:—

R. Liq. Plumbi Subacet. ʒiss; Tr. Opii, ʒiss; Conserva Rosæ Gall. ʒj. M. fiat Linimentum.

In another case, most relief from pain is given by prussic acid.

R. Acid Hydrocyanici, ʒss.; Aquæ distill. ʒviij. M. fiat Lotio.

When the sloughing ulcer bleeds much, as it often does, apply one of these applications:—the muriated tincture of iron, Ruspini's styptic, the decoction of tormentilla, or of galls, or the subcarbonate of iron in powder or mixed with honey, so as to spread on lint.

Sometimes, under some plan or other of these kinds, with judicious constitutional treatment, the sore will become more healthy; it does not granulate indeed, but is healthy enough to cicatrise completely though not soundly, a thin shining dark-coloured skin only being formed; or perhaps just as it is completed ulceration is again established, and goes on to its fatal termination.

The great point, however, with all cancerous complaints, is to get rid of the local disease, whenever this is practicable and desirable. One method, which is often resorted to by quacks, is to destroy the disease by caustics, of which the most common is arsenic. If used to an ulcer, it may be employed in three ways:—as a paste, by mixing up equal parts, or a third of white arsenic and two-thirds of sulphur, which destroys the whole at once; or in a solution on lint, from five to ten grains in an ounce of lime water, the effect of which is more gradual and less painful and severe; or, as recommended by Dupuytren, in powder, with calomel, but this is the least useful of all the forms. Be very much on your guard, however, whenever you use arsenic, and never employ it at all with a large sore; for many a patient has probably been destroyed by it, as it is easily absorbed. A better caustic, from its not being attended with this risk, is the chloride of zinc, mixed with two-thirds of sulphate of lime in paste, and with a small diseased part, and a superficial ulcer, it may be used with advantage sometimes, but not if the tumor is large or thick. And caustics are always to be considered inferior to the knife, if the patient is willing to have it used, and the situation of the disease renders it easily accessible.

The removal of a cancerous tumor by the knife forms a very important subject for consideration. It is not, indeed, very often that any operation will effect permanent good; the disease, in a large majority of instances, will return again, and often in a very short time after the removal, where circumstances appear at the time not very unfavourable; but still some few persons escape without any return, and their lives are doubtless preserved by the operation. It is right, too, to perform it sometimes in cases in which a complete cure is perfectly hopeless, in order to afford a respite to the patient's fate, and remove a source of local suffering. A man, 75 years of age, had a cancer of the penis; it was removed because his health was good, and the disease very painful, although a gland was enlarged in the groin. A woman was under my care with a large cancerous tumor of the breast, with enlarged glands in the axilla, the tumor being full six inches high, and sloughing, and irritating

her so much that she could scarcely have lived three weeks. I removed the tumor, with a great part of the pectoral and intercostal muscles, in which a number of little cancerous tubercles existed, and the woman said she would gladly have gone through the operation for less than a week's freedom from her previous sufferings; she got well in health, and enjoyed life for eight months afterwards, when she finally fell a victim to the disease. Your patient may again, even in an advanced stage, get rid of a loathsome malady, which she exchanges sometimes for one of equal bodily suffering perhaps, but in an internal viscus, the lungs, or the liver, it may be, but removed from sight, and causing therefore less mental agony.

But in other cases the operation may only hasten your patient's death; sometimes it returns with greater activity in the part itself; sometimes the operation affects the constitution so as to hasten the development of disease elsewhere; and sometimes there are dangerous and even fatal results from the operation itself.

The question of an operation depends a good deal on the kind of cancer in each case, or the organ in which it is situated, and some circumstances of this sort will be explained to you, with the separate diseases of the breast and other parts; much however depends also on the state of the parts affected, and the state of system wherever the disease is found, and I will briefly allude to some particulars which are to guide you.

We have seen that the disease is propagated in several ways; let us examine how each of these affects the chance of removing the whole disease:—first, cancer spreads to the neighbouring parts, where these are even so little affected as to be apparently healthy, yet when cut into, the disease may quickly return in the cicatrix, or the wound never heals entirely. You may see the skin sometimes altered, firmer and thicker when you pinch it up, presenting a number of spots and black specks, from enlarged sebaceous glands, when compared to the skin around; and I have alluded to the curious fact, that the skin may be thus contaminated, while the fat between the skin and the tumor remains healthy. Sometimes you find, perhaps unexpectedly, in the operation, a number of little cancerous tubercles in the muscles and in the fat around, perfectly distinct from the chief tumor, varying in size from a pin's head to a pea; or you may distinguish them with the finger moving below the skin; or little flat or roundish tubercles are found and seen, as well as felt, in the skin itself. I have seen such a disposition to this tuberculation that all the marks left by the cupping,

scarificator, and by leeches, have become cancerous tubercles. Now, whenever the skin is thus diseased, or tubercles exist anywhere around the tumor, you have no chance of removing the whole disease, and the operation will generally do harm by its effect on the system. For the same reason, when the skin is ulcerated, an operation is seldom admissible, but I do not think a state of ulceration so bad as the thickening and tuberculation of the skin.—Secondly, cancer is propagated by the poison being conveyed by the absorbent vessels; the evidence of which is a hardness in their course, or an enlargement in the next series of absorbent glands—in the neck, the axilla, the groin, the loins, and so on. Now, whenever glands are thus enlarged, the operation becomes either altogether improper, or affords you very little chance of permanent good, even if you can remove, at the same time with the tumor, all the glands that appear diseased. Sometimes, indeed, especially with cancer of the skin, the glands are irritated and swelled, as they might be from any disease not of a malignant nature, and in such case the glands may subside after the removal of the irritating cause. You endeavour to distinguish whether this be the case by the hardness or softness—the inflammatory redness and throbbing pain, or lancinating pain, of the two cases. But, except to remove a cause of great suffering, as in the instance of my own, before described to you, you should seldom operate if the glands are altered in structure by cancer.—Thirdly, the whole system becomes affected, either previous to the appearance of, or by absorption of morbid matter from the local tumor, according to the circumstances of the case. You are to look carefully, therefore, for evidence of this state, in the sallowness, emaciation, dulness, and languor of the patient. Sometimes, cough, hæmoptysis, or hydrothorax, will show you that the lungs or pleura are affected; sometimes an examination will show you the uterus affected, when there is also an external tumor; and the whole body must thus be investigated before determining on an operation on any part.

Something depends on the age of your patient. A comparatively young person may encounter more risk than an old one, from the greater number of years that may be gained if the operation be successful; at least she may do so if the tumor is insulated, and unattached, and not growing rapidly: otherwise, the very fact of cancer appearing in a young person is an argument in favour of the system being probably already poisoned, or at least very strongly predisposed to the disease. In very old persons all operations are formidable, and you will probably accelerate their

dissolution by performing one, for the disease is slower in its progress than it is in earlier life. You may be guided by the natural constitution of your patient, whether she is fat and bloated, or has a more healthy constitution; and much also by the temper of your patient: you should seldom, indeed, press the operation very strongly on a reluctant person, for this very state of mind may cause a fatal result, in a case where a person of a calm cheerful disposition would recover, and have no return.

LECTURE VI.

III. SARCOMATOUS TUMORS, *concluded*.

II. *Fungus Tumors.*

- a. *Fungus Hæmatodes.*
- b. *Encephaloid or Medullary Tumor.*
- c. *Melanosis.*

II. *Fungus Tum vs.*

THE last genus into which I have divided the solid tumors, is, like the carcinomatous, of a malignant character; and it may be divided into three species; viz. (a.) *The Fungus Hæmatodes Tumors*; (b.) *The Medullary Tumors*, and, (c.) *The Melanotic Tumors*. The most striking peculiarities attending these tumors, besides a more rapid increase than the last genus, are, the growth of an irregularly-shaped fungus when the tumor is not subjected to pressure, or when it is exposed by ulceration, and generally a disposition to erosion of the vessels of the tumor, occasioning hæmorrhage, sometimes in considerable quantity, from the exposed substance, or into its interior, and we will call it the genus *Fungus Tumors*.

These tumors have received, however, a variety of different names, derived from their different appearances, sometimes only at different stages of the same structure, which occasion modifications of colour, and various alterations in their course, without materially affecting their essential characteristics. They used formerly, and not very improperly, to be called soft cancer, to distinguish them from the last genus of carcinomatous tumors. Sir Everard Home imagined that the disease was cancer in muscular structure; but, in reality, cancer is as distinct in the muscles as it is in other textures. They began to be described nearly at the same time, in this country, by Mr. Hey, under the name of *Fungus Hæmatodes*—by Mr. Abernethy, under that of *Medullary Sarcoma*—by Mr. Burns the term *Spongoid Inflammation* was used (a very bad term, however, since inflammation is not essential to their formation)—by Dr. Baillie the appellation of *Pulpy Testicle* was used to describe the appearance of the

fungous tumor in that organ, and Dr. Monro adopted the name of *Milk-like Tumor*, for another form of the same complaint. Soon afterwards great attention was paid by Laennec, and other French pathologists, to the same form of the disease which Abernethy called *Medullary Sarcoma*, and they used the corresponding terms, *Encephaloid*, or *Cerebriform Tumor*, to describe its appearance. It is singular, however, that while they have much improved our knowledge of the subject, and have discovered another form, the *Melanotic Tumor*, they should, many of them, have remained ignorant of the real nature of the disease so well described by English authors, and should still class together the vascular nævus and the real fungus hæmatodes. Sir Astley Cooper, again, has used the term *fungoid disease* for all varieties of the complaint; but as the peculiarity alluded to is not like a fungus, but is actually a fungous growth, I prefer the name placed in the table behind me, *Fungous Tumors*.

One of the latest and best of our authorities upon the subject of malignant diseases, Dr. Carswell, has placed them all under one head, with the title of *Carcinoma*, which is subdivided into heads:—

1. *Scirrhus*, under which name he includes scirrhus, or cancer, in its ordinary form, and in that of a lardaceous, and a gelatiniform mass, and also the pancreatic tumor, or conglomerate tumor, of our division.

2. *Cephaloma*, placing in this division the medullary or encephaloid, and the hæmatoid varieties of fungous diseases, and also the mammary, or mastoid, tumor of Mr. Abernethy, which I mentioned as a variety of cancer (which is not the mammary tumor of Sir A. Cooper, you will recollect), and also what Mr. Abernethy called common vascular sarcoma; but what that is I do not profess to understand.

Now we shall find, presently, that there can be no doubt whatever that cancer and fungous diseases are very much allied. For instance, here is a preparation of cancer of the breast removed by Sir Benjamin Brodie, which was followed by fungus hæmatodes of the rib; but I am not quite pleased with the generic name of carcinoma, as including both these forms of disease, since it is almost universally, in medical and surgical writings, confined to one of them, viz. the true cancer; and it seems very doubtful to me, whether some of the diseases included by him, especially the conglomerate tumors, have any title to be placed among the malignant diseases. And then, again, fungus hæmatodes is not like brain, as the term cephaloma implies; and exactly the same reasoning which connects together the carcinomatous and

medullary, and hæmatodes tumors, will join to them also melanosis, which Dr. Carswell excludes from the class; because just as cancer is often joined with a fungous structure in the same tumor, or they mutually succeed each other in the same part, or in different organs of the same individual, so also does a melanotic tumor join itself to each of the others. All of them are allied, while each possesses some peculiarities, and melanosis is, perhaps, most allied to the fungus hæmatodes in the rapidity of its growth and the extent to which it affects the system.

They may then all, I think, be included in one great division of malignant diseases, of which we noticed the carcinomatous or cancerous variety in the last lecture, and we now have the encephaloid, hæmatoid, and melanoid varieties, to take into our consideration. The origin of all these is often the same exciting cause, and the external characters are so much alike, that we can often not tell what tumor our patient has till it has undergone some farther changes, and sometimes even when removed we cannot, in the present state of our knowledge, decide of what character the tumor is.

Still the division between them is real, in many respects, and it is useful, because our reasoning upon the proper treatment to be adopted, especially with regard to the important subject of operation, very often depends, not upon the general characters in which they all agree, but upon those specific differences, or upon the changes undergone by the tumor at different stages, in which its characters are sometimes, to a certain degree, merged in those of another variety.

The Fungous Tumors, in their more ordinary form, the encephaloid and hæmatoid varieties, are, on the whole, softer than carcinomatous tumors during all their growth, and often feel quite of a pulpy consistence: a quantity of soft substance may be washed out of them, leaving a flocculent membranous structure pervaded by numerous vessels. In colour they vary much; sometimes there is a soft white matter, exactly like brain; sometimes a yellowish or brownish hue predominates, and there is a mixture of effused blood in some parts, as in this example, in minute specks, or in larger masses; sometimes there are a number of cells filled with different fluids, and mixed with solid fungous matter in the intervals between the cells, and these cells are often half broken down, or more or less filled with coagulated blood, as in these preparations; which fact is probably explained by supposing that the soft coats of the blood-vessels have been ruptured by un-

equal pressure. The consistence of fungous tumors varies, however, considerably, from a milky semi-fluid substance, which will not retain its form, when you cut across it, to a solid firm structure, almost like scirrhus: even the hardest, however, want the peculiar radiated and fibrous appearance of cancer, and the peculiar stony hardness, like cartilage, of that kind of morbid structure.

Contrast the two genera in these internal viscera. In the lung there are, in this case, the solid, hard, round, well-defined tubercles of scirrhus; here are the large, soft, yellowish white (not insulated) masses of cerebriiform disease, and here the soft, red, brittle, vascular tubercles of the hæmatoid fungous tumors: the former seated on the surface under the pleura, and projecting into the cavity of that membrane; the latter intimately, and almost inseparably, united with the parenchyma of every part of the organ. Look again at this portion of a liver containing large diffused tubera of fungous disease, with all the branches of the vena porta filled with the same substance (as I explained in the last lecture, or as you may see in this plate of Cruveilhier), and compare it with this mass of carcinomatous substance, with the centre harder and depressed, puckered in, as it were, while the rest of the morbid growth expands around—exactly, in fact, as the nipple is drawn in and retracted in cancer of the breast, with prominent enlargement of the substance around it. Such, then, is the kind of structure you will ordinarily find in this genus of tumors in its two species of medullary and hæmatoid materials, which I have for the present blended together, as they run insensibly into each other; and the peculiarities I have described are sometimes as distinctly marked in a tumor not larger than a pea as they are in one of twenty pounds weight, and differ materially from the forms of cancer.

I told you in the last lecture, that scirrhus affected particular organs more than others, especially the female breast and uterus; so also does a fungous tumor; but it is frequent in parts which are not usually the seat of scirrhus. Fungus hæmatodes is very common in the testis, while cancer of that gland is almost unknown. Fungus hæmatodes is frequent in the globe of the eye, and in the orbit, while cancer is rare in those parts; but cancer is, on the other hand, often found in the eyelids or lachrymal gland. So indisposed, in fact, is the globe of the eye to cancerous ulceration, although so readily affected by the other forms of malignant disease, that you may see it quite surrounded and insulated, and hanging loose in a large and

deep cancerous sore, without being itself tainted. Fungus hæmatodes is rare in the coats of the alimentary canal, which are frequently the seat of gelatiniform cancer. Still no structure, probably, is exempt from either of the diseases, though, perhaps, the contamination of fungous disease is less resisted by most structures than scirrhus may be.

Fungous tumors, like cancer, frequently arise from local causes. Here is a preparation, which was removed, when I was house-surgeon, from the breast of a groom, thirty years of age, five months after he ruptured some fibres of the pectoral muscle, in catching a run-away horse; the tumor began a fortnight after the accident, and was of the size of an egg in five weeks; then it grew more rapidly, and in four months from the accident it ulcerated, and a fungus as large as an orange, of which this is a section, grew from it, and the whole mass of the tumor was seven or eight pounds weight. A tight shoe has been known to occasion fungous disease of the foot. A gentleman, a pupil of this hospital, three or four years since, pricked his finger with a needle, and fungus hæmatodes arose from the injury, which was speedily fatal.

But certainly fungous tumors are less frequently local in their effects than cancerous tumors. Either the system is more affected before any local disease is formed, or the secretions of the part are more easily absorbed, so as to contaminate the system, and the poison is less frequently arrested by the absorbent glands. This fact involves the important consequence, that you have much less chance of a successful operation than for some forms of cancer, and the disease generally reappears in a greater number of places. For instance, Mr. Keate removed a fungous tumor of the breast, in January, from a woman whose general health was so little disturbed that the disease was at first opened as for an abscess. Before she died in the following April, that is, in three months, twenty or thirty external tumors had formed in different parts of the body, besides extensive morbid deposits in several viscera; and these tumors seemed to present every variety of form, both of cancerous and fungous structures.

There is, doubtless, an original difference of constitution, which has something to do with the kind of malignant structure which is developed when a malignant diathesis is engendered. The patients, both with cancer and fungous complaints, become emaciated and unhealthy as the disease proceeds; but cancer chiefly appears in persons whose countenance is pale and bloodless when the system is affected, with sunken eyes, and a leaden com-

plexion. Fungus hæmatodes, on the contrary, frequently appears, and grows to the size of a person's head, perhaps, in young people, stout, and with a fair skin and florid complexion, and apparently healthy constitution, for a considerable time. Mr. Travers expresses this circumstance by giving it as his opinion, that fungus hæmatodes is cancer in a scrofulous system; the delicacy of the skin and full development of the cutaneous circulation being, in such cases, like what may be observed in scrofulous individuals not immediately labouring under that complaint. At last, however, in all the forms of malignant disease, whether cancerous or fungous, there occurs that half-jaundiced complexion, and peculiar aspect, which the experienced eye detects without much difficulty.

Another circumstance which strongly demonstrates the different condition of the system, is the age at which the two genera of tumors respectively occur. Cancer, as we have seen, is a disease of the middle and latter periods of life, very seldom taking place under thirty years of age. Fungous disease, on the contrary, may appear at any age, but it is most frequent in young persons, and is seldom met with in mature years. Here is a plate of fungus hæmatodes of the eye in a child of eight months, and I have seen it in much younger infants even than this. Still fungus hæmatodes may take place at any age, and you are not to regard the two genera of tumors as the same disease modified by different ages; for if an encephaloid or fungus hæmatodes tumor take place late in life, the disease is just as characteristic as in young persons; except, indeed, as we might expect from the greater activity of their circulation, that the fungous disease is generally more rapid in its progress in younger persons than in adults.

Another circumstance in which the cancerous and fungous tumors are contrasted with one another, is, that the fungous tumor may be handled in general without its causing much pain, while the examination of the scirrhus tumor is sure to be followed by increased pain, even when you do so gently. During the progress of the fungous tumor, it is, in its first stage, much less attached than scirrhus to the surrounding parts, and it is insulated and confined by a cyst of cellular membrane, more or less condensed, allowing it to be freely moved, and more easily removed. The skin also is seldom fixed to the tumor till ulceration begins, and is seldom affected by this genus of morbid growths, while it is easily contaminated by scirrhus. These tumors have also large tortuous veins on their surface more frequently than other kinds of tumors, although this

appearance is not to be looked upon as necessarily implying the malignancy of a tumor.

A striking difference between cancer and fungous tumors is afforded by the comparative rapidity of their growth. Scirrhus generally increases steadily for several years, and seldom attains any great magnitude, nor are the fungous growths from its ulcer very large and vascular. Fungus hæmatodes, on the contrary, may, in a few weeks or months, become as large as a person's head; or, having grown slowly for a certain time, will suddenly increase with frightful rapidity from some internal change in the tumor, or from the removal of pressure by ulceration, or other circumstances. A girl was, not long since, in the hospital, under Mr. Cutler's care, with a fungous tumor, of a mixed medullary and hæmatoid character, below the hip, a part of which by degrees protruded upwards under Poupert's ligament, and immediately, from the little pressure it was subjected to in the abdomen, increased with additional rapidity, and in about three weeks, I think, from its first being perceived, this part, which had only a narrow connexion by vessels to the mass of the tumor below the pelvis, grew to the size of more than a foot in diameter. If the pressure is taken off by ulceration, an immense fungous mass is sometimes exposed, which bleeds most profusely from erosion of the vessels.

One reason of the rapidity of the growth of fungous tumors is the cells which are often intermixed with the solid part of the tumor, the fluid contents of which are more readily secreted to distend them, than the solid matter can be deposited and organised. Look, for instance, at these two testes which I amputated. In a tumor of the breast, of encephaloid character, I emptied some of the cysts occasionally, from one of which, at a time, as much as six or eight ounces of fluid sometimes flowed. In a patient of Dr. Seymour's, lately, I gave great relief, for a time, by evacuating about four ounces from a cyst in a fungous growth in the abdomen, I know not of what organ.

Recollect, then, in examining tumors, the existence of cysts in those of a fungous character, and endeavour to ascertain the nature of the solid portions of the tumor, if there be any at the same time. But further, there is a frequent source of error in diagnosis, from the cyst-like division of a fungous tumor, when there is really no fluid; the semi-organized deposits in these separate portions feel pulpy, especially in the medullary kind, and are very often mistaken for abscesses. The distinction is chiefly this: the tumor feels firm and elastic, like fluid, when you press upon it,

but does not fluctuate if you tap it on one side, and press your finger on the other; and there is a difference in degree of solidity in the several parts of the tumor, one part feeling just as if there was fluid below, while another part is more solid and resisting. The testis affected with the disease will look and feel at first like a hydrocele, but a careful examination will show that it is somewhat irregular and tuberculated in its shape and consistence. If you are in doubt as to the nature of a tumor, you may puncture it, and then, if it be fungous, you will see nothing but a little blood, or serum, or gelatinous fluid, escape, and the resistance will be that of a body more or less solid. If you do puncture the tumor, and I know no harm from doing it, it should be with a grooved exploring needle, not with a lancet; the wound made by the latter instrument is liable to ulcerate, and the tumor inflames, and a fungus rapidly arises, which is not the case when the needle is used.

Another peculiarity in fungous tumors is of great consequence; they are sometimes, especially in the hæmatoid variety, very vascular, and very large vessels circulate within them. This is an important circumstance in the diagnosis, for the tumor may then pulsate exactly like an aneurism. There is in the museum, I believe, but I could not find it for you, a preparation of a tumor which was situated in the thigh of a woman, which pulsed everywhere like an aneurism, the pulsation being controlled by pressure on the common femoral artery. The surgeon whose patient she was, however, felt doubts upon the subject, and punctured it, on which a stream of arterial blood jetted out to some height; still he had doubts, and on other occasions punctured it again, with the same result. I was present when the artery was tied, as for aneurism; and on the patient dying a few days afterwards, it was found to have been a large hæmatoid tumor of the thigh.

A very instructive and interesting case has been published by Mr. Guthrie, in the fourteenth volume of the *MEDICAL GAZETTE*, in which he tied the common iliac artery; and it is one of the few instances in which the operation has been performed with success. A lady had a tumor of the size of the head on the buttock, which was seen by several distinguished surgeons, of whom Mr. Keate was the only one who expressed any doubts of its being an aneurism of the gluteal artery. The operation having been done in August, the tumor diminished considerably, and the patient was supposed to have been cured of her aneurism. The tumor, however, returned, and she died in the following April, with an immense tumor of the me-

dullary and fungus hamatodes species mixed, which originated in the os innominatum.

It is fortunate when surgeons of distinguished reputation have the candour to publish their mistakes; and all must make mistakes. Such cases, indeed, are not intended to justify carelessness on the part of those who are of less eminence, but they should teach caution to all in the exercise of their judgment, in practice as well as prognosis, especially to the young, who are prone to think that they know every thing; they should inculcate a lesson of charity, also, when mistakes are committed by others.

But this is not all, for you may be led into exactly the opposite error. Here is an enormous aneurism of the thigh, which looked like a malignant tumor, as it had no pulsation, except where the femoral and popliteal artery passed, not through it, but over its surface. I did not myself examine it very narrowly before it was amputated by Mr. Keate, for as mortification was beginning in the toes from its pressure on the blood-vessels, the operation was called for, whatever the nature of the tumor might be. A man had a tumor in the thigh, where the femoral artery enters the sheath of the triceps, into which an opening was made with the intention of dissecting out the fungous tumor that it was supposed to be; but a coagulum was found, through which the arterial blood of an aneurism made its appearance, on the discovery of which I made pressure over the wound, while the artery was tied above. The patient afterwards got well. I believe I was the only one who suspected that the tumor was aneurismal; and I mention the circumstance, that I may tell you why I formed the opinion, in order to guide you in similar cases. I believe that a fungous tumor pulsates like an aneurism, and you can alter its size by pressure; but the diminution never seems to be in one part only, and on the removal of your pressure the blood never comes into the tumor again with the peculiar thrill of an aneurism: in this case, on the contrary, I distinctly felt a kind of central softer depression after I had pressed some blood out of the cavity, and there was a thrill on its regaining its former size, which thrill you may distinguish with the stethoscope, or the ear, when you cannot by the hand. Almost all aneurisms, in fact, even when most solid, have some fluid within them, which you can thus press out, unless the aneurism is under the process of natural cure. Even this, however, is not an universal rule, for Mr. Lawrence has published a case in which he amputated the thigh, as for a solid tumor, but which proved to have been an aneurism, which

had completely lost all pulsation, and could not be made to vary its size at all.

Thus, then, it appears that, in its first stage, a fungous tumor may occasion some doubts in your minds regarding its nature; not so, however, when it reaches its second stage, that of local ulceration. When this begins, your patient will complain of more pain than he had hitherto experienced; the skin becomes dark-coloured and more vascular, and then purple, and ulcerates and discharges a thin watery fluid; then there arises a fungous projection, at first of the surface of the cyst inclosing the tumor; then a slough forms, a dark yellow slough, perhaps, as in this preparation, or of a brown colour. The edges of the skin, perhaps, look thick, and welted, and everted, as is well seen here, but are still distinct from and unattached to the surface of the tumor; while, on the contrary, no division can be perceived when cancer is thus ulcerated; and although the discharging surface is fetid, the factor is rather that of a gangrenous ulcer than the peculiar sickening odour which emanates from cancer.

A third stage commences as these sloughs separate, by which process the large vessels of the tumor are opened, and bleed considerably, and irritative fever comes on. You see here the large tumor I have alluded to in the breast, in its second sloughy and fungous state; in this preparation you see a tumor in the same situation in its third stage, which illustrates the mode in which a patient frequently dies of the disease. The wound had nearly healed, when a little tumor formed by the side of the cicatrix about three months after the operation, and in three months more it had formed a large bleeding and sloughing tumor, with prominent florid masses of granulations alternating with the sloughs, and he died worn out with irritation, and pain, and loss of blood, and cough, and hectic fever. You may see all that had not sloughed away adherent to and implicating the upper ribs, and projecting into the cavity of the chest, with a portion of lung adherent to it; and the pleura was also full of serum, which compressed the lung. Hydrothorax is, in fact, not an unfrequent result of either cancerous or fungous tumors, and its existence must be well investigated before an operation is performed on any tumor on the chest. I have repeatedly seen a fungous tumor commencing in the rib itself, and care must be taken lest an operation be undertaken on the supposition of the case being only a tumor external to, and only attached to the rib, when it really, perhaps, projects almost as far within the chest as it does externally. I recollect a case in which a large soft swelling existed

on the side of the chest, in a state of incipient suppuration, and with an impulse communicated to it by coughing, as in a case of empyema. When Mr. Keate punctured the tumor, a quantity of broken and dead pieces of bone, mixed with some pus, were felt by the finger, and the case proved to be really an instance of fungus hæmatodes of the ribs.

It is not only by hectic and irritative fever, or serous effusion in the chest or abdomen, that a fungous tumor causes death; witness many of the preparations on the table. In this case a man was admitted into the hospital under Mr. Keate, and you may see that the fracture is across a tubercle of medullary substance in the bone; indeed, it is not unfrequent in the osseous texture. A few days afterwards he died suddenly of vomiting of blood, and you may perceive extensive disease of the same kind in the lungs and œsophagus, with a large ulcerated opening in the latter tube, from which the fatal hæmorrhage had taken place.

Here is a case of fungus hæmatodes in the bladder and prostate gland, for the hæmorrhage of which disease the high operation has more than once been performed, though unnecessarily as I conceive, since the blood can be dissolved and washed away.—Hæmorrhage again took place in this instance into the pericardium; and I have seen the same thing in the other internal cavities.—This medullary tumor was removed by Mr. Ewbank from the head of a young man, while I was house-surgeon; but the disease returned, and proved fatal by stupor and other symptoms of pressure on the brain.—The same thing, or apoplexy, or watery effusion, will be the termination of such a tumor as this within the brain itself; and it is singular that the gentleman from whom I removed this hæmatoid disease had lost two others of his family with a similar disease, nearly in the same situation in the cerebellum.

When an external fungous tumor is situated on the thigh or trunk of the body, where it can easily gain skin from the neighbouring surface, it seldom ulcerates till it has attained a great size, and thus it frequently happens that the patient is carried off by some co-existing internal disease of the same kind, where its pressure occasions fatal results, or where it ulcerates more readily than the external tumor; a happy circumstance, very often, for the patient, who is perhaps spared the dread of hourly impending death from a bleeding surface, together with the annoyance and pain and suffering of a sloughing fœtid fungus.

The hæmorrhage, by which so many are exhausted in the progress of fungous

tumors, is not merely the result of their great vascularity, especially in the hæmatoid variety, but is the consequence, as it would seem, of some peculiarity in the texture of the coats of the vessels, so that in an operation your patient may almost die of hæmorrhage under your hands, if you cut across a part of the tumor, while he will hardly lose any blood if the incisions are made just beyond the limits of the tumor, through the same vessels, of course, which transmitted the blood in the former case, but which have a contractile property, which those in the tumors are destitute of; the vessels in this respect resemble those of the vascular naevi.

And this peculiarity of the texture of the vessels influences also the hæmorrhage frequently observed in the interior of the tumor; the vessels, when congested, are easily ruptured, so as to cause effusion into the cells and cellular texture of the tumor. At the same time, I do not wish you to understand that the effusion of blood in the texture of a tumor is a complete proof of its malignancy; the same circumstance may doubtless occur in other tumors, if there is a similar brittleness of texture in their interior. So, also, the strangulation of a portion of the tumor during its irregular growth, and the consequent mortification of that portion, may occur in several kinds of tumors, perhaps, in the manner suggested by Dr. Hodgkin, which I explained to you formerly. But, whenever you see in a tumor of doubtful nature much disposition to internal effusion of blood, and to partial sloughing of portions of the interior, you should strongly suspect that it possesses a malignant character.

There is yet one other circumstance in the history of fungous tumors to be mentioned to you. The sloughing process occasionally pervades the whole tumor, and it dies. The patient is, perhaps, near dying too, but recovers from the dangerous constitutional symptoms he has laboured under, and the disease is cured, at least in that part. A man was under my care with a fungous tumor in the ham, originating in the femur; this ulcerated and then sloughed, after he had refused amputation, and was almost dying with the jaundiced appearance of malignant diseases, and he left the hospital, expecting to die each hour; but having as great horror of being examined after death, as he had of the knife while living, and when it might have saved him. I saw him, afterwards, however, and continued the small doses of calomel and opium, with sarsaparilla, which I was giving him, and in six weeks the cavity in which both hands might have been put, filled up with granulations. He became stout and well enough

to resume his occupation of a groom. Only half the tumor, however, had thus sloughed, the other part of the bone was affected, and six months afterwards he was again looking thin and haggard, and I dare say he did not long survive.

The history I have thus given you of the progress of a fungous tumor, applies chiefly to those in which the vascular hæmatoid species predominates; the well-marked *cerebriform* species is less vascular in its texture, and hence, perhaps, is less seen in very young persons than the hæmatoid; and its lesser vascularity influences its progress to a certain extent, so that it is, on the whole, rather less rapid in its growth; there is less hæmorrhage and sloughing in its interior, a less disposition to form cells of various fluids, and less bleeding from its surface when ulcerated, and from its structure when cut or torn in operation. It has been sometimes said, from its great resemblance to brain in appearance, that this form of fungous tumor originated in the nervous matter; but the opinion is quite erroneous, and the medullary tumor really originates in every texture that the other forms affect, and the difference must arise from something in its growth which is not yet understood.

Fungus hæmatodes, and medullary tumors, are, however, often mixed in different parts of the same tumor, and a medullary tumor often changes its character as it proceeds, and a part of it becomes redder and more organized, and like the hæmatoid species. Here is a large tumor connected with the ligamentum patellæ chiefly, the surface of which became fungated, and bled enormously. I amputated the limb, and the patient died in a few days afterwards with hæmorrhage from the bowels and by the mouth. You may see that the base of the tumor is exactly like brain, while the projecting fungus alone is like fungus hæmatodes.

Still the medullary and hæmatoid species are not, as some have imagined, merely the first and second stages of the same tumor; each is often distinct and well marked at its commencement, and preserves its specific difference to its termination. The medullary form is not unfrequent in the glands; and I recollect examining a patient of Mr. Merriman's, at Kensington, with an immense lobulated tumor, composed of a great number of glands, reaching from the upper part of the head into the mediastinum, which had destroyed a large portion of the parietal, temporal, and maxillary bones, with which it was in contact, by suppurating under their periosteum; and yet, when of this extent, every lobule was perfectly white

and brainlike, and no portion had in the least assumed the characters of fungous hæmatodes.

The cerebriform species of fungous tumor is no less fatal and no less deeply rooted in the constitution, and no less universal in its appearance, than the fungus hæmatodes species; and I may observe, that if the tuberculated conglobate tumor is really a form of fungous tumors, rather than a separate species, it is to the medullary tumor of the glands, such as the case I have just now alluded to, that it is most allied.

Melanosis is a species of fungous disease which derives its name from the peculiar dark colour of its masses, which are black, or purple, or dark brown; and it was first described, I believe, by Laennec, as a separate affection from the encephaloid and hæmatoid species. This colour arises from the secretion of the peculiar coloured matter into the cells or cellular texture of the tumor or organ affected, and it varies in solidity from a semifluid consistence to a completely solid substance. *Melanosis* generally consists of a number of globular portions united together, as in these preparations from the eye. The first case which I ever saw was a tumor of considerable size on the dorsum of the foot, which, from this form, and from its colour, looked exactly like a bunch of dark purple grapes, besides several other single tubercles which she had elsewhere. I well recollect the case, as I accompanied Mr. Ewbank, whose patient this woman had been, twenty miles into the country, on an exceedingly wet night, and having our ardour in the pursuit of science rewarded with a positive retraction of the promise which had been given for us to examine the body.

Sometimes the whole surface of the body, as in this plate of Alibert's, or as in a patient who was a year ago in the hospital, under my care, whom some of you may remember, is studded with an immense number of little blue tubercles, like seeds in size. You may see the same appearance in this portion of the liver: sometimes it is in larger masses, as in this plate of Cruveilhier's; at another time, though not so frequently, it is seen in the form of a number of distinct round tubercles, as in this beautiful preparation of the liver, from a patient of Mr. Keate's. Here is the eye of the same patient, which was removed some time before her death, where the melanotic matter is mixed with other morbid structures of a different appearance. The disease in the liver showed itself not long after the operation.

Melanosis is very frequent in the eye,

and it has been imagined that the pigment of the choroid coat might influence the colour and growth of the tumor, while the retina was supposed by the same persons to determine the medullary form of tumor. In reality, however, melanotic tumors are found in as many structures as the other varieties of fungous disease, and cannot be attributed to any peculiarity in the tissue affected. The disease has been observed not unfrequently in white horses; and Dr. Carswell has supposed that this circumstance may arise from the absence of coloured matter in the skin and its other proper tissues; but then, why should we see the disease in every colour of the human species, and not in albinos only?—and why is it not confined to the lungs, or to the eye, or rete mucosum? In fact, however, the nature of the colouring materials of melanotic tumors has not been well ascertained. Chemical examination shows us in them carbon, with some phosphate of lime and iron, but affords us no reason for the peculiar deposit, nor for the constitutional condition on which the formation of melanosis, as well as of the malignant growths, depends.

The structure of melanotic tumors is most like that of the cystic and other kinds of fungus hæmatodes; so that it seldom feels very firm, but I have sometimes seen the tumors very hard and solid when examined through the skin. Melanotic tumors have not the same high degree of organization that the hæmatoid tumors exhibit, there being a good deal of the secretion in the tumor, but the cellular texture is tolerably vascular.

Melanosis is equally fatal with either of the other species of fungous tumors; but perhaps its less organised condition is one reason why the tumors less frequently ulcerate and generate a bleeding and sloughing fungus than the others: another reason, however, appears to me to be the fact of the still more uniform malignant condition of the system, so that, pervading more structures at the same time, the constitution sinks under it at an earlier stage of any one tumor than even in the other forms of fungous disease. Melanosis is, on the whole, a disease of later life than the hæmatoid variety, in which respect it resembles the cerebriform tumor and cancer. It ultimately occasions the same emaciation and gradual sinking as other malignant diseases, with a fatal result from the affection of some internal organ,—sudden peritonitis or pleurisy, or ascites, or hydrothorax, or universal anasarca, as is beautifully shewn in this picture of the disease in Alibert's work. A melanotic tumor is sometimes observed a longer time than a hæmatoid tumor before

the final result, but is in general quite as rapid as that disease.

All the malignant diseases, however, are often mixed up together—melanosis with cancer—and both with the encephaloid and hæmatoid tumors. The last patient I myself had in the hospital came under my care for a tumor in each axilla, which tumors were of the joint characters of fungus hæmatodes and medullary tumor; and in a short time melanosis made its appearance most extensively, both in the integuments and in various internal organs.

The term melanosis is sometimes very improperly applied, even by pathologists as distinguished as Andral and Cruveilhier, to the collection of carbon often found in the glands of the bronchi, and to the change of colour produced in the blood by chemical agents. Of the latter kind is the blackness of the stagnant blood in the vessels of the intestines and peritoneum, from the contact of their contents, either before or after death; such, also, is the black vomit from effused blood, from cancer, or other ulcers, which erode the vessels of the stomach. Dr. Carswell attributes this to the action of the gastric juice and other acid secretions of the alimentary canal, whence the change is only observed in those situations, and proposes the use of the designation "spurious melanosis" for such appearances. You will recollect, however, when you see the appellation, that these forms of black appearance have nothing whatever to do with the disease melanosis, as a fungous disease, to which, therefore, to avoid confusion, the term ought to be restricted.

Such, then, is an account of these curious malignant affections, as ample as our time will allow, in their several forms—fungus hæmatodes, eucephaloid tumor, and melanosis. There are also some other trifling differences in the appearance of some tumors, which, at present at least, are not sufficiently distinguished to require notice as separate species. They are all, we find, allied, and, like carcinomia, are all incurable; and an operation for their removal alone offers any chance of permanent success. They may temporarily diminish under pressure, and iodine will also sometimes considerably lessen their size. Here, for instance, is a case of fungus hæmatodes of the rib, which appeared like a tumor of the breast, from its pushing that gland forwards; this was seen by Sir B. Brodie three or four years before the patient's death, and for a time its size was materially diminished. Relief is sometimes obtained by evacuating the fluid of the larger cells, by a grooved needle, as I have before mentioned.

Caustics afford the same means of destroying these tumors that they do in cancer; they imitate, in fact, the natural cure by sloughing. They are dangerous, however, and have even less power than in cancer of destroying the whole tumor, their greater activity of circulation giving them, perhaps, more power to resist the action of caustic: hence the knife alone is a safe means of extirpation.

You may adopt the same remedies to retard the growth of the disease, and make the operation more successful, which I before recommended for cancer. Sarsaparilla and other tonics, sometimes with small quantities of bichloride of mercury, or, if the countenance is yellow and sunk, small doses of calomel and opium, a half or quarter of a grain of the former to half a grain of opium, night and morning; and I have mentioned one case, in which, under this plan with sarsaparilla, a patient of mine rallied from the sloughing process, and got stout and well in health. Narcotics and gentle evacuants are also called for, as in all malignant affections.

Neither shall I detain you with speaking of local remedies, which are also to be of similar qualities with those which I spoke of in the last lecture as useful in cancer; but you can do much less towards stopping the hæmorrhage and sloughing of fungous tumors than you can do in cancer.

In the last place, with regard to the propriety of operations for the removal of fungous tumors, somewhat similar rules are to guide your determination, since the disease is propagated nearly in the same way; but still there are some modifications of your practice to be adopted.

1st. There is less contamination of the surrounding textures than there is in cancer; there is often a complete cyst around the morbid structure; and the textures of the skin are seldom adherent and diseased: consequently, there is much greater facility in removing the whole tumor. Fewer circumstances have to be inquired into with regard to the local condition of the disease, and the morbid structure seldom returns in the cicatrix, as it is so prone to do after operations for cancer; unless, indeed, the ulceration is extensive, as in this tumor of the breast, in which, however, there is probably some admixture of cancer with fungus hæmatodes.

2d. The disease is propagated, as in cancer, by the absorbent vessels. Here, for instance, is a fungous disease of the testis, which I removed by operation, and I found distinctly the same white medullary fluid in some of the absorbent vessels of the spermatic cord, which existed in parts of the testis, and which was perceived still more remarkably in a large mass of fungous

cerebriform tumor in the lumbar glands, from the effects of which the patient died soon after the operation. In one case, indeed, Sir Astley Cooper informs us that he found the receptaculum chyli and thoracic duct completely filled with the same morbid matter that the testis of the patient contained, so that there was no more passage for chyle through it than there is in this preparation for blood to flow through the vena portæ; the respective fluids must in each case have got into the circulation by some circuitous route. I have also seen the absorbent vessels filled in the same manner by morbid matter from malignant disease of the bowel. But although the disease is propagated in this way, and the abdominal glands are very often affected by disease of the testis, still I think there is considerably less affection of the glands by fungous tumors in other situations, than there is in the cancerous tumors of the same parts; and you will often find the next absorbents unaffected by a tumor, while the malignant state of the system is shewn by the occurrence of other tumors of the same or analogous character elsewhere. If they are enlarged, even when you have no evidence of disease in other situations, the operation is generally useless; but remember, moreover, that when the glands are perfectly safe, the chances are still great that you will find malignant disease in the viscera.

3d. You should, therefore, devote more especial attention to the state of the general system, in order to detect, if possible, the simultaneous morbid condition of the lungs or liver, or some other part. Certainly, I think, considering the whole history of the disease, that an operation is less frequently successful than one for cancer, bad, indeed, as is the prospect even there. Mr. Wardrop, in his observations on fungus hæmatodes, which well deserve your perusal, has collected a great number of cases, in not one of which was the patient exempt from a return of the disease a year after an operation; but then these cases are almost entirely instances of fungous disease in the eye, on which organ the operations are notoriously unfavourable. Certainly in other parts of the body, even when the tumor has been growing a longer time, it would appear that the life of the patient is occasionally prolonged. I have notes of a case which I well recollect, where the testis was removed by Sir Benjamin Brodie, and the section of the disease was considered by every one, at the time, to be a remarkably fine specimen of fungus hæmatodes; yet I know that this man was alive and well nearly five years afterwards. I know not how long he has subsequently lived.

A few such cases are a sufficient en-

couragement to us to operate, where there are no forbidding circumstances, though with but a feeble hope of ultimate success. Even when the disease already exists in more than one part, yet if one tumor is ulcerated, or affects the system much by the irritation it excites, it may be right sometimes to get rid of this part of the malady. We thus retard that fatal result which we cannot wholly prevent. A gentleman, of whom I formerly knew something, had a tumor on the thigh, which was so malignant in appearance that amputation was refused by several surgeons, although he was greatly emaciated, and dying of this tumor, while there were several others in other parts of the body not in so advanced a state. Sir William Blizard, however, performed the operation; and twelve years afterwards, when I last heard of him, he was still alive, even although two or three of the other tumors subsequently enlarged so much as to require removal at different times. These latter operations were performed by Mr. Lawrence, who has published the case as an encouragement to operate in cases of the tuberculated sarcoma of Mr. Abernethy, of which disease Mr. Lawrence considered the case an example, though it appears to me, from all I have heard of it, to have been more probably a modified case of fungous tumor.

It appears to me, however, as well from this case as from other facts in particular instances of fungous tumors, that there is probably some variety in the degree in which different cases are malignant, as there undoubtedly is in cancer, as it affects particular localities, and is modified by texture. Future investigations therefore, I hope, may determine more precisely the several species and varieties of these malignant affections than our present knowledge enables us to do; and by greater accuracy in laying down the appearances of the tumors, and the states of system attending each variety, to guide us in resolving in what cases an operation is proper to be performed, as affording a tolerable chance of success; and under what circumstances, on the contrary, it is to be avoided, as likely to aggravate the morbid condition of the system. In the skin especially, I have seen a few tumors in the skin, (in which texture cancer is less virulent than in most other textures,) of the nature of which I could not feel certain, but which I have suspected to have been some rarer forms of fungous tumor of this texture. Here, for instance, is one like a mushroom in shape, which was attached to the skin of the thigh by a narrow pedicle, the surface of which having ulcerated, bled repeatedly, to the ex-

tent of above half a pint at a time, though the vessels which I divided in dissecting it off were not remarkably large; but its texture is not distinct.

ON THE INTRODUCTION OF AIR INTO THE VEINS.

By M. VELPEAU.

[Concluded from p. 959.]

Estimation of the Cases which have occurred in Human Life.

EXCLUDING the cases which occurred in animals, I may divide those belonging to man into four classes: the first containing the unimportant cases; the second, those in which death did not supervene; the third, containing fatal cases, in which there was no examination; and the fourth, those where there was an examination.

FIRST CLASS. Cases to be rejected.—These are the cases which are built upon mere hearsay. They are four in number, being those attributed to MM. Gräfe, Lodge, Duportail, and Sir Astley Cooper. It seems certain, indeed, that these are not real facts, and that they have been referred to merely through some mistake.

SECOND CLASS. Cases not followed by death.—Fifteen of the cases above analysed, in which it is mentioned that the patients came to themselves again, seem, at any rate, to prove that they did not die under the influence of the first symptoms. One of these cases belongs to Mr. Mott, two to M. Clemot, one to Mr. Barlow, one to Mr. Warren, one to M. Roux, one to Mr. Mirault, one to M. Rigaud, one to M. Delaporte, one to M. Dubourg, one to M. Malgaigne, one to M. Begin, one to M. Toulmouche, one to M. Amussat, and one to myself.

These fifteen cases are of very different values. Those of M. Rigaud, of M. Malgaigne, and Mr. Mott, only indicate a wound of the external jugular vein. Those of MM. Amussat and Toulmouche merely relate to the mammary veins. In Mr. Barlow's case, and in one of M. Clemot's, it also seems that neither the axillary nor the internal jugular vein was wounded. There remain, then, only those of MM. Roux, Warren, Begin, Delaporte, Dubourg, and my own, with one of M. Clemot's, which allow us to regard the fact as possible, considering the region, and the vein wounded. The case taken by M. Guérétin, from M. Mirault's practice, leaves us in doubt as to the vein opened.

THIRD CLASS. Cases followed by death, but without post-mortem examination.—These

are six in number, and are attested by the name of Warren, Clemot, Barlow, Goulard, Klein, and Maugeis.

In M. Clemot's case the vein opened is not well determined; in M. Barlow's it was probably the internal jugular; in Klein's, it was the thyroid plexus; and a subscapular branch in Mr. Warren's. M. Duplat asserts, positively, that it was the axillary in the case which he attributes to M. Goulard. The median vein at the bend of the arm was the only one opened in M. Maugeis' case. This simple abstract sufficiently shows how vague must be our ideas, when such facts are presented to us.

FOURTH CLASS. *Cases followed by death, where there was a post-mortem examination.*—These are seven in number, and belong to MM. Piedagnel, Dupuytren, Delpech, Castara, Ulrich, Roux, and Putegnat. M. Putegnat's case being given without any particulars, and on the word of a third person, must, I think, be put aside. M. Piedagnel says it was the external jugular vein which was wounded in Bauchène's patient. He has also informed me, that in Dupuytren's case the tumor was on the right posterior and lateral region of the neck; consequently it is not possible that the internal jugular vein, or the subclavian, could have been wounded. In M. Roux's and M. Delpech's cases, the operation was amputation at the shoulder-joint, and the only veins wounded when the bad symptoms came on, were unconnected with the axillary. M. Saucerotte says, that in M. Castara's patient it was a subscapular vein which had been opened to the extent of less than a line, which was also the case in Mr. Warren's. Hence M. Ulrich's case is the only one which affects the region where, according to our experiments, the introduction of air into the veins appears possible and dangerous.

If we now take up the question again at a higher point, we are in some sort compelled to admit the following conclusion—Either the experiments hitherto performed on living animals are incomplete and deceptive, or else these cases concerning the introduction of air into the veins of man are not conclusive.

Looking upon the question, I see, in fact—First, by direct experiment, that it requires a large quantity of air to kill a dog; that air does not enter the heart spontaneously, excepting by very large apertures in the jugular, subclavian, or axillary vein; and that the right cavities are then always distended by a red frothy matter, evidently formed by the intimate mixture of blood and air. Secondly, in many of the cases in which human beings were concerned, the only veins wounded were the external jugular, or the veins of the

breast, shoulder, or face. In the others the aperture in the vein was small, and but little air could enter; and, lastly, cadaveric examination never showed, in any case, the appearances which have been established by direct experiment.

It is not necessary here to refute the language of those who, no doubt through inadvertence, have always kept beyond the points at issue, both in the course of our experiments, and in the discussions at the Academy; and who have always reasoned as if I and others had denied the possibility and the dangers of the entrance of air into the veins. Without seeking for the motives which may have induced some of my colleagues to fall so continually into this unfortunate error, it is sufficient to refer to the bulletin of the Academy, to show the falseness of the supposition. A point which I must take up, as it might otherwise lead to error, relates to the manner in which some have thought to refute the interpretation which I had put upon certain cases. M. Roux, for example, who, in relating several cases, which, though otherwise interesting, were completely foreign to the subject, thought it very strange that I endeavoured to conclude from animals to man; that I accused observers of not having given all the requisite details of their cases; and that while rejecting one explanation, I gave no other; M. Roux, I say, whose discourse seemed to make an impression on the assembly, yet appears to me, I must confess, not to have thrown light upon any point of the question.

To assert that, at the instant of accidents like these, the surgeon has not sufficient presence of mind, and is too disquieted and too grieved, to think of any thing but the danger which threatens his patient, and that he cannot give an exact and circumstantial account of what has happened, and that in the midst of such occurrences it would be unjust to reproach operators with having published cases destitute of sufficient details, or badly drawn up, is, indeed, a sufficient justification of those who have been placed in these unfortunate circumstances; but does it, in the eye of science, improve the facts, or make them more conclusive? Is it not, on the contrary, confessing at the outset that these facts are necessarily incomplete, and that in so arduous a question it is impossible to bestow our complete confidence upon them?

M. Roux, while allowing that the phenomena observed in animals are far from resembling those which have occurred in human beings, exclaims that this proves nothing; for since symptoms offer numerous varieties, even in different individuals

of the same species, or of different species, in direct experiments, we may well believe, he says, that matters will not be the same in men as in dogs. I might answer, that I myself mentioned this supposition to the Academy, and proposed to take it into account long before M. Roux had uttered it. But he must allow, in his turn, that it is only a supposition; and that the differences between what has been said of human beings and what has been seen in animals, are so striking, that until the contrary has been proved it is impossible not to retain some doubts upon the subject. Besides, M. Roux seems to have forgotten that reasoning of this kind would authorise us, at the outside, to presume the fact in question, to regard it as probable, or as very probable, but that it cannot be demonstrated in this manner. But what is the question between us? Not to know if it is *possible*, *presumable*, or even *very probable*; but whether it is *demonstrated*, irresistibly, that in the cases referred to, death took place from the introduction of air into the veins. Thus we remain in doubt, which, in my opinion, is the real state of the question.

As to reproaching us with not explaining why the patients died, if we do not allow that they died from the introduction of air into the veins, I think that M. Roux must have done this through inadvertence; for if we must always consider those facts as demonstrated or incontestable of which we cannot give any other explanation, or must admit them, because in denying the explanation of a phenomenon we cannot give any other more satisfactory one, it would certainly not be long before science was overwhelmed with an infinity of dangerous errors. Nevertheless, I must say, that all the cases which have occurred in the human species have something strange, and altogether unusual, about them. Though it is true that patients may die of syncope, hemorrhage, alarm, or exhaustion, during great operations, it is also true that in such cases life is extinguished with a different train of symptoms from those which have been related. If we erase from the list the cases of Klein, Dupontail, Lodge, Dubourg, Mauzeis, and Sir Astley Cooper, which are either quite unimportant, or which may be easily explained without the supposition of air having entered the veins, it is difficult not to return to this way of explaining the others. If we suppose that in the cases related by MM. Rigaud, Clemot, Begin, Malgaigne, and myself, the gush of a small artery against some organic layers, or the entrance of air into the sinuities of a *cul-de-sac*, may have produced the hissing, the bubbling, or the sound of emptying a bottle, mentioned by authors,

it is almost impossible not to admit something more in the cases of MM. Pielagnal, Dupuytren, Castara, Delpech, Ulrich, Barlow, Warren, and Goulard.

In this state of uncertainty, might one not ask whether, at the moment of the operation in human beings, veins *enlarged* by the nature of the tumor itself, or by the handling to which they are exposed, may not be (for the moment) in the state which is the natural one of the veins at the apex of the chest in living animals. With the help of this explanation, however, the only cases which can be brought into the class of *very probable* ones are those of MM. Goulard, Predagael, Dupuytren, Castara, Delpech, Warren, and Mirault. It appears to me clear that nothing had caused this transformation in the cases of MM. Rigaud, Amussat, Toulmonche, Mott, and Malgaigne.

Can we attribute it, then, to the previous debility of the patients? Every thing tends to show (although our direct experiments still allow some doubts upon this point) that a great loss of blood must make the entrance of air more dangerous. But on analysing the cases, we see that the patients were still strong, with the exception of those belonging to M. Piedagnel, M. Roux, and Klein; that most of them were in perfect health; and that they had not lost more than a few ounces of blood when the symptoms came on.

If practitioners are unwilling to abandon this comparison, our last resource is to admit, that either physically, or physiologically, or pathologically, the conditions accompanying the entrance of air into the veins are remarkably different in the human species and in animals. These differences may be regarded as possible, when we reflect that air in the veins kills a horse sooner than a dog—that death is more speedy when the animal is held in one position than in another—that by blowing in air from the mouth, we kill with the rapidity of lightning, while it requires a pretty long time to produce the same effect if a syringe is employed.

I am ready to allow, however, that no one of these reasons is conclusive; and nothing of what I have said is sufficient, at present, to place beyond the limits of doubt what has been advanced in favour of the introduction of air into the veins of man.

The most reasonable thing to rely upon, to aid us against all these difficulties, is the want of care with which the cases have been drawn up. There is no doubt, that if writers had had a more accurate knowledge of what had been already done, they would have entered into minute particulars as to what concerns human beings. There is no doubt, too, but that a number

of important circumstances have been either omitted, or inaccurately related, or wrongly explained, by those who have published these cases. I say this without blaming observers for it, because in phenomena which rapidly pass away, and are at the same time complex, and difficult to apprehend, persons do not take every thing into account, unless the question implicated in the phenomena has already engaged their attention, and unless they are well acquainted, beforehand, with all the details which it is possible to collect.

Summary.—Meanwhile, to give my candid opinion, I consider the introduction of air into the veins as *probable* in the cases of MM. Bégis, Malgaigne, Mirault, Warren, Barlow, Delaporte, one of M. Clemot's, the first pointed out by M. Roux, and my own. There is no proof that this accident took place in the cases of MM. Pottinmouche, Mott, M. Clemot's second and third cases, or in those of MM. Rigaud, Dubourg, Maugeis, and Amussat. It appears to me *extremely probable* in those of Delpech and Ulrich, and *almost certain* in the cases of Dupuytren, Castara, and Goulard. But after all, this can only be given as my personal belief; for I allow that no one of these facts can be exactly compared with the results of direct experiment, and that, in a scientific point of view, there is not one that carries absolute conviction with it.

Thus my readers will see, that at the end of this discussion I find myself brought back, by force, to repeat what I said on the introduction of air into the veins, in 1832 (*Méd. Opér.* t. ii. p. 194)—“Without denying the possibility of this phenomenon, when the veins form open canals extending to the thorax, I think that new experiments are required to decide the question.”

I will add, as I said in 1833 (*Anat. Chir.* t. ii. pp. 104, 457, 471, 2nd edit.) “That if the fact is really so in human beings, we must look for a different physical explanation from the one given by MM. Poiseuille and Bérard; for it is clear that phenomena resembling those which seem to denote the introduction of air into veins, have been observed in other parts besides the neck and the axilla.”

The reader will see, at a glance, how much the question has been cleared up by late experiments, if he compares its present state with what I said of it at the first sitting of the Academy. I then summed up in the following manner:—

“The introduction of air into the veins raises several important questions. There are already about twenty instances (I ought to have said thirty, at least), where serious symptoms, which have supervened during surgical operations, have been attributed to it. Of this number there are

several, certainly, which are not conclusive. Bichat thought that it was enough to inject a few air-bubbles into the veins of an animal to kill it, and that death then took place through the brain. Nysten showed, on the contrary, that it required a great quantity of air to kill a dog, and that death then took place by stoppage of the circulation. Nysten's experiments have been since repeated by a host of persons, among others every year by Magendie, and have always been followed by the same results.

“By showing that the large veins of the root of the neck are always in a state of tension, M. Bérard has proved that air may pass through them when they are open; while M. Poiseuille maintains that this is not possible at more than a few centimetres from the first rib. But in most of the cases several of these conditions are wanting. Thus, Dupuytren's patient had a tumor on the shoulder, and twenty-four hours afterwards only a few air-bubbles were found in his heart. In others so little air seems to have entered, that, to judge of what we see in animals, they could not have died of it. Lastly, in others the operation was performed upon the face, chest, &c.; and physiological experiments do not allow us to grant, that death can occur by the introduction of air into those parts. Hence the question is still open for examination.”

Therapeutic Means.

Treatment.—In scientific questions there is a rock which we ought to make every effort to avoid; I mean the error of forming an opinion too soon, and wishing to pronounce a positive decision, in spite of the absence of proofs. Although the facts quoted in favour of the introduction of air into the veins are as yet incomplete, it would be wrong to conclude that this accident has never taken place. For my own part, I repeat that, in my opinion, it has happened several times; only I feel that, until more conclusive proofs are alleged, this can only be a personal belief, and that there is nothing at present in science that can change this belief into a general conviction. With this phenomenon before us, we are like magistrates in whose presence there stands a criminal against whom there are no witnesses.

Holding these opinions, I have reflected, like many others, on the means of warding off the dangers attributed to the introduction of air into the veins. These means must be divided into two kinds, the preventive and the curative.

Preventive means.—There is a difficulty at the outset which will long throw doubt upon the efficacy of the attempts which may be made to prevent air entering the

veins during operations. The difficulty is, that no one can tell beforehand if the phenomenon will take place or not, supposing that such or such a vein is opened. Thus, suppose some particular precaution is taken in the case of three, six, ten, or fifteen patients, in whom tumors are extirpated near the apex of the chest, and that the air does not enter the veins in any one of them. Those who put their trust in the given precaution will not fail to conclude, that if nothing had been done, some of the patients would have sunk. Yet this conclusion, which would seem very natural, might be altogether erroneous; for the cases quoted of the introduction of air into the veins are very rare, compared with the number of operations of a kind which would seem to favour it. I have myself more than fifty times extirpated submaxillary, parotid, axillary, supraclavicular, or supra-sternal tumors, obliging me to go very near the great veins of those regions, and often even to open them; yet the case which I related above, and one of a young boy, are the only ones which alarmed me, even for an instant, as to the introduction of air into the veins. I will add, that when assisting M. Roux, I saw him make a large incision in the upper part of the subclavian vein in one case, in the upper part of the axillary vein in another, and in the lower part of the internal jugular in a third; and yet nothing which could be referred to the introduction of air ever resulted. When we reflect on the number of amputations which have been performed at the shoulder-joint—on the frequency of operations in the axilla for cancerous or lymphatic tumors—on the numerous instances in which arteries have been tied above the clavicle, and in the carotid regions, without any results comparable to what takes place when air enters the heart—we are forced to admit, at least, that the accident in question is hard to happen. How, then, can we be certain that, if it does not take place in a given instance, we are to attribute this to our precautions, rather than to the natural resistance of the frame? Hence we see that, to settle the question of preventive means, requires, like the rest of the subject, numerous experiments. Let us, however, examine those which seem to have attracted attention up to the present time.

Compression of the thorax during the whole of the operation seems to me to be suitable neither in theory, nor according to the experiments already tried. If the compression were sufficiently strong to prevent any raising of the ribs, the patient would obviously suffer a constraint so considerable as to be insupportable. Moreover, the

chest would be equally extended in its vertical diameter by the depression of the diaphragm; nor has it yet been shewn that the dilatation of the heart has no share in the absorption of the air.

M. Poiseuille, reasoning as if inspiration alone could draw air into the heart (*Gaz. Méd.* 1837, p. 671), thought that it would be sufficient to prevent this accident, if we could make the patient carefully avoid deep inspiration. This recommendation seems to me to be connected with the same error which suggested compression to M. Gerdy. In fact, experience has long shewn surgeons that, during serious operations, the chest of the patient is generally contracted spasmodically—that respiration becomes slow, and is performed by small movement—and that one has rather to excite patient's to breathe deeply, than to prevent them from doing so, if we wish the venous circulation not to be too much clogged.

Compression of the veins between the heart and the incision.—When the idea of the introduction of air into the heart first arose among practitioners, the first remedy which necessarily presented itself was this kind of compression; it seems so natural and so certain. Thus Larrey says that, when bleeding in the neck, we ought to compress the vein below the incision until the bandage has been put on, if we wish to prevent air from reaching the heart (*Clin. Chir.* t. i. p. 357.) Dupuytren does not omit to point out the same means when narrating his case (*Arch. Gén. de Méd.* t. v. p. 438), which he had previously communicated to the Academy. A similar recommendation is to be found in Mr. Barlow's memoir (*Gaz. Méd.* 1831, p. 355.) M. Putegnat likewise gives it in his thesis. Mr. Warren states, at page 266 of his treatise on Tumors, that the surgeon who operates in the vicinity of the jugular, subclavian, axillary, or iliac veins, or even of the saphena, when it is dilated, ought to leave to the last the separation of the pedicle of the tumors on that side, in order to be better able to compress the veins before opening them. He adds that, if possible, one ought to compress the veins between the incisions and the heart. I have myself thought it necessary to insist upon this recommendation, and to follow it in some cases of extirpation of parotid and submaxillary tumors; but it must be confessed that the remedy is a weak one. If, like the first observers, we admitted the possibility of air being inspired at a very great distance from the heart, the remedy would deserve our serious consideration. In case of the internal saphena, or femoral veins, the iliacs, the veins of the arm, the face, and the external jugular, nothing

would be so easy as to put it into practice; but if it is true that the introduction of air is impossible in all these regions, compression is useless as a preventive. On the other hand, who does not see that, in the region above the clavicle, the upper part of the axilla, and the upper hyoidian region, where it would be useful, it is inapplicable? The subclavian vein, concealed under the clavicle or the sternum, and separated from the ribs by the root of the scapula, lies in such a manner that its calibre cannot be obliterated through the skin. It is the same with the internal jugular vein below the larynx. To which we must add, that compressing this vein, when we are operating near the parotid region, puffs up the face, and increases the size of all the veins, which we are in danger of wounding.

For these various reasons we must admit that compression of the venous trunk between the wound and the heart is a resource of small importance, and rarely applicable.

Hence it follows that, in practice, we cannot count upon the efficacy of any of the preventive means which have been hitherto mentioned, and that all which is in the surgeon's power may be reduced to these three points:—

1st. To avoid with the utmost care wounding the internal jugular or subclavian veins in operations.

2ly. In cases where he is obliged to go near these vessels, he is not to separate the pedicle of the tumor without previously compressing it between his fingers on the side of the heart, or passing a strong ligature around it.

3ly. He must avoid as much as possible all stretching, pulling, and separating the parts, lifting the arm, or throwing back the shoulder or the neck, when the bistoury is near the great veins at the apex of the chest.

Curative means.—If the art of healing possesses nothing capable of preventing the air from entering an open vein, we must confess with sorrow that it is still poorer in any method of expelling air from the heart. I greatly doubt that it can be of much use to place the patient on his side, as recommended by M. Forget (*Trans. Méd. t. x. p. 75*).

Compression of the thorax—the jerking compression by which Nysten thought he could expel the air from the auricle through the incision in the vein—is evidently useless, at any rate in human beings. Even in dogs, the air contained in the right ventricle cannot be thus driven out, and the right auricle gives up but very little of it. Who does not see that, in human beings, the thorax, which is far

less flexible than in dogs, can never be flattened to such a degree as to react efficaciously upon the heart?

The most obvious method of all, that is to say, closing the incision in the vein, which has been already frequently adopted, offers, perhaps, as much danger on one hand as advantage on the other. While it puts a stop to the introduction of air into the wounded vessel, unfortunately it also obstructs the exit of the air which the contractions of the heart tend to expel; so that it cannot succeed unless, at the moment when it is applied, the air is not sufficient in quantity to cause death.

Drawing out the air by applying the mouth to the aperture of the vein can hardly ever be applicable. The least reflexion, too, would suffice to shew that it could not be of advantage.

Artificial respiration, either by means of tracheotomy, or by introducing a tube into the natural passage, were means tried by Mr. Warren, but seem to me to have no object.

The introduction of a saline fluid into another vein, which he also recommends, would only be adding to the danger of the patient.

There remains the plan of drawing out the air by a tube or syringe, introduced as far as the heart. But this suggestion, which arose from experiments on animals, should be rigorously proscribed in human beings, if I am not mistaken. It is plain that it could be adopted only when the internal jugular was wounded; and it is equally clear that, in order to introduce the tube, the vessel must be placed in the precise condition most favourable to the introduction of air. Moreover, during a serious operation, it would be requisite first to inquire whether the symptoms which had supervened really depended on the phenomenon in question; then to look for the aperture in the vein; then to ask the assistants for the tube prepared for this purpose; then to introduce it, and apply the syringe or the mouth to it. Now all this would necessarily require more time than intervenes before the death of the patient, if the accounts of these cases are correct. In short, we have really nothing to oppose to this occurrence.

This is a painful avowal, no doubt, but it is the truth. We are reduced, therefore, to the general remedies against syncope, unless there is any efficacy in bleeding, which was formerly proposed, and is still extolled, by MM. Boulay, Leblanc, &c. To avoid extending the veins in the neighbourhood of the chest during operations; to apply the finger to the wound, and to suspend the operation for a short time, when the supposed characteristic sound is

heard; to keep the patient in the horizontal position, and stimulate him with the vapours of ammonia, alcohol, or vinegar; to use friction to his body, and to throw water upon his face—such are, at present, the only means which reason and experience allow us to use without apprehension.—*Gazette Médicale*, Feb. 24, 1838.

STRICTURES
UPON
DOCTOR ARNOTT'S TREATISE
UPON
WARMING AND VENTILATING.

By JULIUS JEFFREYS, Esq.

[Continued from page 967.]

I TAKE the liberty of mentioning a case, remarkably illustrative of the fact, that freshness of air in a house lessens, instead of increases, the liability of the inmates to take cold; although I am aware, while apparently necessary, it was a bold experiment which could not always be followed, inasmuch as it was unmitigated air which was so freely admitted.

In her father's house a lady had been accustomed to rooms in which the ventilation was greatly lessened by close fittings and felt. Yet, since open fires were used, there could not fail, as will be shown hereafter, to be more air admitted than by the use of Dr. Arnett's stove in the common way. In her own house, accordingly, she followed up this plan. The house was made very secure, and the children, though allowed plenty of exercise, were kept as snugly as possible from every draught; nevertheless they were always catching colds, and the more she checked draughts the more they ailed, catching also every epidemic that prevailed. Living in the country, she was led to contrast with her own the healthy children of the farmers around; and, observing their ill-fitted casements and doors, and open fires, with large mouthed chimneys, resolved boldly to copy what she saw. Having removed to an old-fashioned house, with doors and windows fitting as badly as she could desire, she allowed them to remain as they were, and the children to run constantly, even in cold weather, in and out of the parlour door, which opened into the garden. Such has been her course for some years. Her children now know not what it is to take cold;

and although one has a constitutional tendency to an affection in the head, which grew alarming under a system of ventilation, more liberal even than the thermometer stove alone can ensure, they are now the pictures of health.

I trust I shall be excused for having occupied my reader's time with this case, since it appears to me an instructive one; not because with a profusion of fresh air the family grew so healthy, for there are farm-houses in every part of the country presenting the same aspect, but because this, which was the extreme of what Doctor Arnett pronounces to be the catarrh and pleurisy-exciting system, did actually deliver them from the catarrhs, which, under a different system, they were constantly suffering from. Once more I would repeat, that this extreme course is not held up for general imitation; that it would not answer in the case of delicate constitutions; and that for these the air should be rendered mild, but its salutary copiousness should, if possible, be retained.

Having brought himself and his reader gradually over from the advocacy (if strong language and illustrations mean any thing) of a system of more copious ventilation than is usual, to one of much more confined ventilation, it seems necessary, lest there should be any misgivings in the reader's mind—any wonted longings after fresher air—that his thoughts should be familiarized with a Russian state of things, where almost no ventilation exists, and in such a manner, that penny-a-day ventilation may be associated together, in his mind, with long life and rubicundity. The author, therefore, continues, "To these persons," namely, all the people who are advocates for open fires, "it must appear wonderful, that in Russia where, all through the winter, there are only close stoves, and double windows carefully closed, and no provision made for ventilation, beyond accidental crevices, the people are very healthy; and more individuals attain a very advanced age than in almost any other country in Europe." On reading this Russian argument, one is led to exclaim—What has become of the ease of the poor Buckinghamshire lace makers, and where is now its contrast, the delicious trout stream of the author's imagination? Alas, he has left it far off upon the opposite side, and has taken up his

abode in a land of suffocation! As this argument, however, is not without plausibility, and as it may have weight with many readers in favour of what, I believe, would be a dangerous revolution in popular feeling, it is necessary to request of my reader an attentive examination of it.

The squalid appearance of the inhabitants of very northern regions, after their six-months' inhumation, travellers have often noticed. I had supposed the appearance of the Russian poor, who were much at home, must have partaken to some extent of such effects of close confinement; and that although, as in most simpler states of society, individual cases of longevity might be more common there than in England, England had greatly the advantage over Russia as to the chances, or average duration, of human life. The case, however, with which we have to deal is not at all affected by the author's argument. The Russian might be able to live for ever without air, like a toad in a stone, and it would be no proof that the system would answer in England, where all our experience, as formerly shown by the author himself, is against it. The Buckinghamshire lace-makers, for instance, have been trying the experiment for years, even in a more moderate degree, for they *have the opening of the chimney*, and it has miserably failed, disease and early death being the effects of a stifling system, practised in a greater degree with impunity, if not with vigour, by the Russian poor. The reason of this there will be no difficulty in seeing.

The quantity of ventilation necessary for man decreases in some very high degree with the temperature of the climate. It may not be easy to explain upon what physical causes this depends—how the animal system is so modified by climate, as to require the presence of so disproportionate quantities of air in different climates; but the fact is an unquestionable one in regard to the several climates with which Englishmen are most familiar, and our author's very argument establishes it in Russia. In tropical countries—in India, for instance—we find it necessary to build houses with gigantic doors and windows, to the English eye, at first, out of all proportion large and numerous. This is not done for the sake of coolness only; for the mean temperature, during

the hot seasons, is much lower in houses which have massive walls, and few and small doors and windows. It is chiefly for the sake of fresh air. And although these large and numerous doors and windows, ill-fitted as they are, allow of a leakage, when closed, manifold greater than our similar ventilation in England, it is still necessary to throw them all open for several hours every night, even when the outer air, as is oftentimes the case, is much hotter than the inner. In the western provinces of India especially, from the month of March to the middle of July, during the whole twenty-four hours, the air out of doors is hotter than that within. Nevertheless, although air passes freely through the house all the day, not by crevices only, but through doorways, before which are wetted surfaces, which, while they cool the air, give free passage to it, so that it might be supposed more ventilation could not possibly be needed, it does still prove necessary to throw aside, some time after sunset, every window and door for several hours. Any family neglecting this practice, soon declines in health. Here is a case, where, in an oppressively hot climate, a great sacrifice of coolness has to be made to freshness. It will not be said that all this air is wanted merely to carry off the increased perspiration in a tropical climate, for the quantity is ten thousand times more than would be abundant for that purpose. Compared with this ventilation, any in England is as nothing. Is it not plain, then, that, as a ventilation to be tolerable in India must be a hundred fold what will suffice in England, so a ventilation to be tolerable in England must be manifold greater than what may do in Russia? Hence, the Buckinghamshire women failed deplorably when they experimented with Russian ventilation, or rather non-ventilation, here, just as would any family in India which should try English ventilation in that country. The author's Russian argument, therefore, though a dangerous one, must, if rightly understood, go for nothing. The closing sentence of the paragraph remains to be noticed:—"In a room of twelve feet in all its dimensions, and containing, therefore, 1728 cubical feet of air, there is, without any ventilation whatever, an allowance of two feet a minute for one person for fourteen hours." The author, though not mean-

ing it, of course, speaks, in this place, as if the air could be used in distinct parcels until the whole was consumed, each parcel being put aside as it was done with, like a heap of waste paper, so as not to contaminate the rest. Whereas (as he has himself said elsewhere) every breath mixes with all the rest: the whole air is soon a little tainted, and ought then, without delay, to be renewed.

The time has now come for us to notice a grand omission of the author. He has, throughout all his arguments, spoken only of the demand of the lungs for oxygen, and of the presence of the carbonic acid they give off. He has said nothing of the animal impurities, of a much more pernicious kind, thrown off profusely both by the lungs and by the skin. The air of an assembly, of which so little has been used by the lungs that the chemist cannot detect any diminution in its oxygen, nor any of the carbonic acid they have added to it, may in the meantime have become very oppressive on account of animal impurities of the other kind so freely discharged into it; and, if he were to lock up the assembly, until he could discover, with all his skill, the presence of any considerable quantity of carbonic acid, he would have sealed the fate of most of them, by forcing them to imbibe their own poison—matter, though less offensive, perhaps, yet as truly animal off-scourings—as thoroughly excrementitious—as any that goes forth into the draught. I make no apology for using expressions which, under other circumstances, would be unpardonably coarse. It would be a mistaken affectation which should hesitate to do so upon the present, a question of vital importance. The above is a fact, which no physiologist will venture to deny. It is one which cannot be too generally known by the public. Moreover, the confined habit of body of a large portion of our city population, especially of the sedentary classes, is such, that the skin and the lungs, in addition to these their natural duties, have to throw off, in vaporous discharges, much that ought to pass off in another way. So deleterious are all these matters to the health, that, as the author himself has shown in his Introduction, pestilential diseases have decreased in our land, in proportion as our streets and our houses have be-

come wider and more airy. What now must be thought of objecting to the bare ventilation which our houses, with their open fires, commonly afford us; nay, to the indulgence in any quantity of air which we can severally afford to warm?

Such is the beneficial influence of fresh air over the body, that, as experience proves, with all the trying vicissitudes of weather opposing him, the more nearly a person can live in the open air the better, for the most part, will his health be. The ploughman enjoys more vigorous health than the equally hard-working mechanic; and the coachman seated on his box, than the accountant at his desk. It cannot be said that exercise is the chief agent in effecting this difference, for a joiner in his workshop, even with much more air flowing into it than the quantity the author has set as a maximum, does not in general present the hearty aspect of a coachman or a guard, although the work of the former throws all his muscles into the most useful exercise, and the occupation of the latter has the defect of being sedentary; showing that exercise, even confessedly beneficial as it is, cannot make up the difference between the effect, not of a very confined, but of a fairly ventilated place and the open air. With regard to exercise, it is of importance to remark how subservient is its influence to that of the open air. It loses greatly of its beneficial powers, nay, often proves injurious, when deprived of fresh air to give effect to it. It is very common to hear warehousemen and mechanics complaining that the work is too much for their health; work less laborious, in general, than that with which the hedger-and-ditcher, or the lighterman, is familiar. The effect of the exercise of the former persons appears often to fall unequally upon the system, and therefore too heavily upon some one part; while upon those who work abroad, it would seem to be more equally diffused. Determinations to the head, the heart, the lungs, &c. being, I believe, much more common effects of labour within doors than of labour without.

If the presence of the air of heaven around the body, *without measure*, is unquestionably beneficial—the benefit increasing, if the weather is mild, with the speed with which it passes over the

body, a windy being more invigorating than a calm day, and a seat outside of a coach than one even with the windows open inside—are there any properties in brick and mortar which empower them to subvert this order of things within doors? It is true that the body at rest cannot endure cold or draughts. These, of course, must be afforded the fullest consideration; and it will then be obvious that the only limit to that quantity of fresh air which is desirable—that quantity which is to exercise the most salutary influence on the human frame—is to be found at the point where the current excited in the air commences to be too strong, or the expense of warming it too considerable. What limit is there, then, to the quantity of fresh air a person should be allowed within his house, if he can introduce it without draughts, and if he can afford to warm it? Nay, if such a person should be philosophical, and should have ascertained that one-sixth of a cubic foot of air per minute will do for the breath, and should prudently have allowed two cubic feet, on account of the constant mixing of the damaged with the fresh air, and should liberally, as he thought, have apportioned to himself two or three cubic feet of air per minute, and should be satisfying himself that the air of his apartment ought, therefore, to be warmed for less than a penny a-day; would it not be the duty of any friend, upon whose mind the previous and a multitude of other evidence which might be cited to the same effect, were exercising their proper influence, to contend against economy such as this, as of a very erroneous kind? Might he not say—I will not dispute your philosophy, whether I can admit your measurements or not; but I affirm, that, unless miserably poor, you ought to be seeking fresh air in quantities compared with which all that is indispensable for mere existence is but an indefinitely small fraction. You have before you irrefragable, irresistible proof, that, to air flowing over them in boundless quantities, myriads of your fellow men do chiefly owe their hale and vigorous health. It is but a small part of this which, sedentary, and within doors, you can command; but do not reduce this quantity, limited as it must be, five hundred-fold more. Inquire not upon how trifling a sum you can manage to warm air for your rooms, care-

fully meted out in cubic feet; but, of the two, rather inquire what is the utmost sum you can afford towards warming and introducing it in unmeasured quantities into your house. If you will alter your supply, increase its quantity by all means, but on no account think of diminishing it. Behold the hale looks of your neighbour, whose occupation keeps him always abroad, and ask yourself, if any, or if all of your luxuries together, are capable of doing for your health what boundless fresh air is doing for his, and be guided by your own reply. You will then give your luxuries up, one and all, rather than part with any of the little air compared with his, which you already have.

To the poor man this argument may with great, if lessened, force be applied. To him it may be said, that no person ever yet could prove the *habitual* use of any quantity of beer, or spirits, to be necessary, nay even to be beneficial, to the health; that they may be a luxury, but not certainly a necessary; whereas, no one can deny the very beneficial effects of abundant fresh air. Give up, then, your useless beer and pernicious spirits, and devote a part of the saving to the warming of more air for your family. Let him be offered every assistance towards employing his fuel more economically, but never by any plan which does not insure to him his former supply of fresh air, at the least. Any plan which involves with it a yielding up of a portion of fresh air, should be considered as applicable only to the cases of the destitute, who have no lesser necessities to part with, rather than to sacrifice any portion of so great an one.

Defective as the open cottage fire is in some respects, the ventilation, which it not only permits but vigilantly insures, is a redeeming quality of far greater importance. If the chimney draught were put an end to by the use of close stoves, in the manner recommended by the author, impure and infectious effluvia would not, as now, be hurried away up the chimney ere they had time to excite disease, but they would circulate for hours about the rooms of the poor before they were completely removed through the crevices; and it is too probable that, when an epidemic was lighted up, it would not, as now, commonly attack a few of the inmates only, but would, as in some

countries, waste itself upon the whole family with aggravated force. I will not here anticipate matter belonging to my third division—warming and ventilating by the aid of combustion. Under that head it will be shown, that the favourable comparison the author has drawn regarding the ventilating powers of the thermometer stove will not stand the trial either of careful reasoning or of experiment.

In concluding this portion of my subject, I have to express a hope, that my humble endeavour to defend the general opinions of Englishmen in favour of the free ventilation ensured by open chimneys, against the arguments opposed to them by the author in the treatise before us, especially in Art. 82, upon which I have commented at some length, will not appear to my reader either as uncalled for or unsuccessful. It has been rendered especially necessary, by the promising manner in which the author commences his work as the advocate of ventilation, so copious, that any reader must understand by it a larger, instead of a less, supply than is usual in our dwellings, and may thereby be placed off his guard, and be in all the greater danger of lapsing into the subsequent bias of the author's mind. The vast importance of the questions to the well-being of the community on the one hand, and the weight of the author's authority on the other, do also add to the necessity of a commentary, such as I desire to conduct with right reasoning and candour.

YELLOW FEVER—QUESTION OF CONTAGION.

To the Editor of the Medical Gazette.

SIR,

HAVING seen in a late number of the MEDICAL GAZETTE a letter from my friend, Sir Andrew Halliday, covering another from Dr. William Fraser, of Demerara, setting forth that a highly contagious yellow fever had been devastating British Guiana, I feel it incumbent upon me, from having officially visited those regions, to take this early opportunity of giving as explicit and direct a contradiction to the assumption of that disease being, under ordi-

nary circumstances, either infectious or contagious, as it is possible for me to give.

No question has been so frequently and satisfactorily decided in the affirmative, by the concurrent testimony of experienced medical men, as the non-contagion of yellow fever; yet it has always, after a time, been revived, and the alarm rung again; and this will ever be the case as long as quarantine establishments and boards of health are paid to discover imported contagion, in justification of their own appointments. Men have ever possessed a wondrous facility in bringing themselves to believe what it is their interest to believe; and we might as well look to find the expectant subalterns advocating the reduction of the army, as a quarantine officer failing to discover imported contagion whenever an epidemic disease pervaded the country. They are, in fact, enlisted in the cause. It is their trade to discover it; and to suppose that they would not act up to the principles and motives of that trade, is to suppose what rarely occurs in the affairs of men. But to the point in question.

As in every epidemic where multitudes are in the course of being affected, and every degree of necessary communication must be constantly taking place amongst all classes of the community, no one can possibly discriminate, in the first instance, between the epidemic and contagious current of disease; so Dr. Fraser has taken advantage of this state of things to relate various instances where the disease appeared to have been communicated from direct intercourse, which being the natural law of society every where, he could have no difficulty in doing, for the living or the dead were equally at his disposal to choose the infection from. I acknowledge, then, that his cases would be good enough in the way of proof, if we had not others equally good, which will shew contagion to have been impossible, of which I shall now state some from the official records of the Army Medical Department, when I was last in the West Indies; merely premising, that these are not got up for the occasion, but taken from documents of long standing, that have stood unimpeached and unimpugned for nearly a quarter of a century. It would be an unwarrantable abuse of your valuable

pages to cumber them with a mass of formal documents, I shall therefore adopt the form of narrative abstract, leaving the authenticated writings, which, being public property, may be examined, by all who choose to take the trouble, at the office of the Army Medical Board.

In the summer of 1815, the *Regalia* transport arrived at Barbadoes from the coast of Africa, with liberated negroes, in a very sickly condition from scurvy and dysentery. She was a fine vessel, and on that account had been selected to convey invalided white soldiers to England. I went on board to inspect her, accompanied by Deputy-Inspector Forbes and Dr. Mortimer, then Chief of the Navy Medical Department in the West Indies, now of Haslar Hospital, when, to our astonishment, we found that she had been under what may be called a course of aggravated yellow fever ever since quitting the coast of Africa, every man having been ill with it, and five having died with all the worst symptoms during the passage; that she had been at Barbadoes for four days a few weeks previously, when the captain's wife sickened and died, and the captain himself underwent the same fate on the second day after sailing for the Saints, an appendage of Guadeloupe*; that before returning to Barbadoes she went to Antigua, still unquestioned and unsuspected. The fresh men that had been shipped in room of those that had been lost, regularly sickening and dying themselves in the same proportion; and when inspected by us, the mate, who nine days before had been taken on board from a healthy Newfoundland ship, was lying ill of concentrated yellow fever. Here was the disease and free communication with a vengeance, for there had been no report of bad health, consequently no restraint in any way, nor communication of disease at any of the places where she had been; and it was only by chance, I may say, that we discovered the mate in the above condition, and then all the foregoing was brought to light from unwilling witnesses, who, by dint of cross-questioning, were obliged to confess, that immediately before leaving the coast of Africa a large quantity of greenwood had been cut

and laid in for fuel on the same day, and that as many tons of it then remained as would serve for the voyage to England; that on the second day after sailing the fever broke out amongst the crew, until then perfectly healthy, and two presently died; and that they did all they could by fumigations to stop its course, but these were of no avail. We then ascertained that the ballast was shingle ballast, greatly fouled with mud, and that the ship was comparatively a leaky ship, keeping the ballast, that had not been changed for years, in a state of moisture, and, with the help of the greenwood, almost in a state of fermentation. Fumigations were then attempted by the Agent of Transports; but it would not do, for the cook, an old well-seasoned subject of fifty years of age, who alone had resisted the fever, presently sickened and died with black vomit in our hospital ashore*, and it was not until I got the ballast altogether changed, and the hold subjected to the concentrated heat of many stoves, that the fever was stopped. She was then employed as a transport in the expedition against Guadeloupe, and I saw her three months afterwards, filled I may say to the brim, with rebel French prisoners from the gaols—one of whom having been put on board indisposed, was then dying of yellow fever with black vomit on her decks—proceed the next day to France, without further accident or sickness worthy of notice during the voyage. No yellow fever whatever existed at this time amongst the inhabitants of Barbadoes, the shipping in the bay, or the troops in garrison; and it was not until the beginning of the following year that the first case of yellow fever appeared in Bridgetown, in the person of a youthful stranger recently arrived from Europe; and further, for more than twelve months after the arrival of the *Regalia*, none but rare insulated sporadic cases amongst the new comers were known in Barbadoes.

On the 26th of August, 1816, the Childers brig of war came into Carlisle Bay in a state of the greatest distress with yellow fever; the captain, all the

* It was then the rendezvous of our squadron watching Guadeloupe.

* He was received into the largest and fullest fever ward of our general hospital, where I saw him tenderly nursed, and carefully dissected after he was dead, the report of which last, by Deputy-Inspector Forbes, was recorded at the time, and is now extant.

medical officers, one midshipman, two petty officers, and seventeen seamen, lying ill at the time—four midshipmen, two pursers, and fifteen of the men, having died of the disease during a ten days' passage from Trinidad, and the people continued to fall ill for some time, in numbers varying from three to four daily, until more than five-eighths had passed through the disease. All the sick were promptly removed to our hospitals, where they were received literally with open arms, and in my report dated 8th September I find the following:—"It is most gratifying to report that no sickness of any kind has been communicated to any medical officer or other attendant of whatever description employed about the sick, although the devotion of the medical officers to their duty on this important has been unbounded. The hospital assistants have scarcely ever quitted the hospital yard since the seamen were brought on shore, and two of them are young men recently arrived, for whom I had the most serious apprehension—not that they would be exposed to contagion, which in these climates I have ever found to be impossible under circumstances of due ventilation, discipline, and accommodation, but that fatigue, watching, and exertion, would predispose them to be attacked with the epidemic yellow fever of the country;" yet true it is, that soon after fatal yellow fever began to infest our hospitals, not, however, amongst the attendants on the sick seamen, but chiefly amongst the newly-arrived soldiers of the Queen's regiment. It had existed sporadically * in the town at the distance of little more than a mile, with which there was at all times the freest communication from the beginning of the year, and the garrison, from occupying the better and quarter of St. Ann's, were enabled till then to resist the epidemic influence; but here again we obtained the most decided proofs of its non-contagious nature; all the attendants on the sick (and those of the Queen's were unseasoned) being infinitely less liable to be attacked with the disease than the inhabitants of the barracks who never approached them †.

* The ratio of invasions was about one every ten or twelve days, and then for a long time only amongst newly-arrived Europeans.

† As in the case of the Regalia, the Childers underwent a thorough purification at the dockyard at Antigua, when the ballast was found to be actually in a state of fermenting sludge, and a candle could scarcely be made to burn in the hold. After the purification she became perfectly healthy.

This was satisfactorily proved from official returns, and these proofs are still on record.

The year 1816 was one of the sickly seasons of the West Indies. Towards the close of the year the endemic fever pervaded the whole island of Antigua and even the coloured population began to suffer like those described by Dr. Fraser, in Guiana, from intermittent and remittent fevers. To a white regiment in Monk's Hill Barracks were assigned the dock-yard guards of English Harbour, the most pestiferous probably in the West Indies, where the men, after standing sentry at night, would be seized shortly after with the fever, and expire in all the horrors of black vomit when carried back to Monk's Hill*. This occurred to numbers, but in all that time not a single case of the disease originated in, or was communicated to, the quarters—that is to say, the families of the garrison, the staff, and non-combatants of every kind; all, in fact, who never slept out of it, remained free and untouched. Need I go on with similar narrative, every word of which is taken from official reports and returns prepared in the face of day, and in presence of the army who were its subjects?

I shall now go further back to a period of twenty antecedent years, when our army served in the St. Domingo war, and I was then surgeon of the 67th regiment during the years 1796, 7, and 8. I presume it will not be denied that where so many thousands perished from yellow fever†, the black vomit, Dr. Fraser's own diagnostic must have reigned triumphant. I can take it upon me to assert that in all these three years not a single day passed when it might not have been seen in some quarter or other of the army, and the regiment to which I belonged may be said to have died three times over from the disease; for three times were we filled up and renewed by recruits from England, and drafts from other regiments, yet not a single individual who had been in the country for a twelve-month, whether medical, military, or civilian, ever supposed that contagion could have any thing to do with it. The constant daily experience of their

* It is a small insulated hill, 600 feet high, immediately overhanging the marshes of English Harbour.

† Yellow fever is an absurd name; the best designation being that of the French Creoles, who style it *la fièvre Européenne*.

lives satisfied that it was impossible; and had any learned *newly-arrived* doctor (for I must confess that all were contagionists on first arrival) preached to them such an absurdity as contagion, the very women and drum-boys of the army would have laughed him to scorn, for they all knew, because they had uniformly found that there was no danger whatever to be apprehended from approaching and tendering the kindest offices to their dying comrades; that these offices were, in fact, the safest they could perform, the causes of the disease being exposure to the heat of the sun, to the chill and dews of the night, to fatigue, to anxiety and fear, to intemperance, and, above all, to being unseasoned*: and after all this, is it to be borne that such men as Jackson, and Baneroff, and Burnett, who in various colonies, and with different armies, while living both by sea and land under the direst scourge of the disease, should be branded with the imputation of infidel principles, and classed with Tom Paine, Hume, and Voltaire, because they had honesty and courage first to renounce their own prejudices in regard to contagion, and afterwards sought to disabuse the minds of their fellow-countrymen? Out upon such abominable cant, which can only bring disgrace upon the man who forces it into an investigation of medical truths. I would recommend to Doctor Fraser a more suitable vent for his religious feelings in the example of the celebrated Doctor Rush, of Philadelphia, who, like many others, deceived by the first visitation that he had witnessed of yellow fever, stood forth a strenuous contagionist; but when convinced of his error, on seeing it return during successive sickly seasons, and being on his deathbed, devoutly implored pardon of his God, of his country, and of every friend of science and humanity, for having been instrumental in propagating so pernicious a delusion!

But further. The yellow fever has, at times, fearfully devastated the seaboard of North America during the autumn; but at no other time of such sickly seasons as assimilated its towns to the climate of the tropics, and pains were taken to ascertain the opinion of those medical men who had seen and treated the disease

in regard to its contagious nature, of whom 26 were in the affirmative, and more than 150 dissented; and when we consider that the quarantine staff must, in all probability, have fully comprehended twenty-six officials, we need not wonder at the number stated. When I was last in the West Indies, I in like manner addressed the same query to every individual under my orders, throughout the whole of the colonies; and although the yellow fever was then raging before their eyes, the muster of contagionists was only two. I further proved, from incontestible returns, that the immediate attendants on the sick, whether on sea or land, were less liable to be attacked with the disease than those who never approached them; and if this be not the criterion and touchstone of the existence of contagion, the very *experimentum crucis*, I know not what is. Staff-surgeon Dunkin, Doctor Fraser's respected predecessor in Demerara, stated to me officially at George Town, that during the many years he had been in Demerara, and witnessing various invasions of yellow fever, he never perceived any thing that at all resembled contagion, except once or twice amongst sailors in the foul ill-ventilated holds of merchant ships, under which circumstances it is very well known that all febrile diseases will more or less assume a contagious character,—in fact, generate a contagious atmosphere, which will prevail until ventilation and purity be restored, but no longer. And here I will state an occurrence as related to me by Doctor Dunkin, that took place shortly before my arrival in Demerara, which will prove, I think, to Doctor Fraser, that the worst yellow fever with black vomit may arise without the possibility of its having been imported. Two youthful Europeans arrived in Demerara at a time when the colony was healthy, nothing like yellow fever having been seen for a long while previously. They were welcomed with all the hospitality so characteristic of the West Indies, and at a convivial dinner party in the country, a sportive challenge passed between them, when heated with wine, to run a race. The race was run, but both sickened on the following day, and both speedily perished in all the horrors of the black vomit. Dr. Dunkin declared he never saw two worse cases. They were surrounded by their friends and tenderly

* The Inspectors-General, Bowland, Warren, and Theodore Gordon, are yet alive, and I know will fully bear me out in all the above.

nursed; and I can vouch that no infection proceeded from them, for the colony continued to be healthy; no more than it ever did from the hundreds I am sure I have seen where the matter of black vomit was received upon the hands or upon the clothes, or its exhalations freely breathed by the attendants of the sick*. Doctor Fraser attempts to prove that the late visitation in Guiana was imported from Barbadoes, and differing from any thing in the shape of yellow fever he had ever seen before; but he surely must know, that the mildest form of scarlatina, subjecting the patient every where to no greater annoyance than the *nimia diligentia medicorum*, will, without any cause that we can possibly comprehend, suddenly assume the features of an exterminating plague; or has he never heard of malignant measles, or typhoid pneumonia, without any thing in the seasons or ostensible circumstances that could account for such change in the diseases? and would any one be justified in proclaiming these to be new imported plagues? for such must have been the case with the late yellow fever of Guiana, where the poison ever lurks in one of the deepest swamps of the world, and according to laws of its own, which we can neither understand nor control (for there are more things in heaven and earth than are dreamt of in our philosophy), invadeth where it listeth, there being seasons, and even courses of seasons, in no way differing from others in regard to heat and moisture, when this poison will lie dormant, and then, seemingly without a cause, burst forth amongst the white inhabitants, to the terror and desolation of whole districts. But I have not yet done with contagion. I freely acknowledge that there exist true contagions, and terrible ones too; and, as I have said before, that it may be made possible for temporary contingent contagion to be generated any where; but the yellow fever is none of the first; and when Dr. Fraser attempts

to raise the wild alarm of imported pestilence, he does what in him lies to loosen every social bond—to sever every domestic tie—to cause the sick to be deserted at their utmost need—and to convert men, otherwise humane and kind, into selfish barbarians; for while possessed by the insanity of panic, all reason will be extinguished in the paramount instinct of self-preservation, and its appeals given to the winds. When the Asiatic cholera invaded the British Isles, which is just as much a contagion as the yellow fever of Guiana, shipwrecked sailors were stowed upon the beach, under suspicion that they came from infected places; and wayfaring women, seized with the pains of labour, were, under the same suspicion, thrust from the out-houses where they had sought shelter, and committed to the chances of the public highways. Panic fear ever steels the heart, and, when the cholera prevailed, caused scenes to be enacted through every nation of Christian Europe, that bring disgrace upon human nature; and if now told of the New Zealanders, would scarcely be believed. Happily for Dr. Fraser, and the colony where he resides, the inhabitants of Demerara will not believe him; they will appeal to their own observation and common sense—to the records of past epidemics—and to the testimony of other trustworthy medical men, and then the ban of pestilence will no longer be pronounced against the dying man, nor will the faithful coloured people desert his couch; but were it possible for him to prevail, he would go far to disorganize society, annihilate its commerce, and cause the desertion of its towns, almost as effectually as the cry of mad dog in their streets*.

I may now call myself an old man, for nearly half a century has passed away since I first entered the Medical

* To shew how the American faculty appreciated the vaunted contagion of black vomit, I will here relate an incident I recollect reading in a periodical of the time:—A party of them had procured a chamber-vessel full of the liquid from a dying patient; this they tasted, smelled too, stuffed up their nostrils, into their ears, and in-oculated themselves in every variety of way, until one of them casting his eyes upon a large clyster syringe that chanced to be in the room, filled the instrument with it, and administered to himself a full enema!

* I think it is Dr. O'Halloran (I may mistake the name, for I have mislaid the book, but I am sure of the fact) who relates, that when he entered a town in Spain where the yellow fever was raging, he beheld a soldier dragging a man, evidently far advanced in disease, at the extremity of a long rope. He followed this strange spectacle through deserted streets—no inhabitant daring to appear—until he saw him deposited in a large remote wooden shed, which he then discovered to be the receptacle for the outlawed victims of the disease: and this is the tender nursing Dr. Fraser would establish for his friends in Demerara; for whether he means it or not, such would infallibly be the result, should he once succeed in convincing the people of the reality of imported pestilence.

Department of the Army; but while I have power to raise my voice, or to hold my pen, I never will permit so false and pernicious a doctrine as the contagion of yellow fever to pass unimpaired. This letter is a long one, I fear too long; but I trust, Mr. Editor, you will give it publicity, for it is a letter of defence and retaliation against a gross attack, and on every principle of fairness the reply ought to appear in the same pages from which the provocation issued.—I am, sir,

Your obedient servant,

WM. FERGUSSON, M.D.

Inspector-General of Hospitals.

Windsor, Feb. 24, 1838.

SUGGESTIONS
FOR THE
BETTER REGULATION
OF THE
IRISH MEDICAL CHARITIES,
AND
REMARKS UPON THE BILLS OF LATE INTRO-
DUCED INTO PARLIAMENT FOR
THAT PURPOSE.

To the Editor of the Medical Gazette.

SIR,

To improve the present system of public medical aid in Ireland, and to make it adequate to the effectual relief of the sick poor, it is necessary to amend the law in several very important respects, as well as to supply the defects of former legislation.

A fundamental means for this end is the providing by law for a vigilant inspection of the medical charities, combined with an efficient control. The only security under the existing law for their proper management is to be found in the association of gentlemen who subscribe, and certain *ex officio* governors. But such persons seldom visit the institution to which they belong, or take any part in the administration of its affairs. Even the committee they appoint is, in general, merely nominal; no minutes being kept of its proceedings, or no meetings held, but every thing left to the medical attendant, or the treasurer, who is probably his friend; and it is not to be expected under such circumstances, without the corrective influence of an active and intelligent superintendence on the one side, and a sense of

responsibility on the other, that the medical charities should fulfil their legitimate ends. Moreover, the very materials furnished to the sick poor at these institutions, the proper mode of purchasing, preparing, and preserving these materials, the very accounting for their expenditure, can be understood by professional men alone; while it is easy for the medical or other officer to make such returns in writing to a series of set questions when proposed, and to furnish such a statement and account of the application of the funds, and exaggeration in the number of persons relieved, to be laid before the grand juries at each Assizes, as shall be sufficient to cover abuse. Hence there is an obvious necessity for appointing duly qualified persons to visit and inspect the medical charities, and to report upon their actual state. This necessity has been acknowledged of late: the bills introduced into parliament have all been framed with such a view.

But the medical inspectors to be entrusted with this charge must themselves be subject to control, in order to insure the proper performance of their duties. To make them report to a medical charities' office in Dublin, as was proposed in the last session of parliament, is clearly of no avail. Again, to establish a board of superintendence from among medical practitioners, if they are to act without any remuneration, as is provided by the bill now before parliament, is not likely to have the effect desired. For, supposing such a board to be established, the next thing, in all probability, would be an application to the government for paid medical commissioners as well as paid inspectors, upon the plea of the great sacrifice of time they would be compelled to make in discharge of the duties assigned to them by the bill. But a main objection to the establishment of such a board is, that it would not possess sufficient authority over the inspectors, while it would itself be quite independent of the Poor-law Commissioners, and might thus give rise to conflicting regulations for the management of the sick poor, and the poor in general, which it is intended to obviate.

Another proposition is, to put the medical inspectors upon the staff of the poor-law commission; but this, I conceive, would not be a popular act. Much obloquy, it is to be feared, might attach to such a plan, as it would not fail to be

insinuated that the attempt would thenceforth be made to dole out medical relief upon the contract-scheme, and by the institution of medical clubs, instead of administering it liberally as heretofore; and if public opinion were to set in against the Poor-law Commissioners through this means, it might be directed against the measure itself in the execution of which they were employed, and thus an unnecessary risk would be incurred. But, independently of any consideration of this kind, it should be borne in mind that the medical charities are capable of so much usefulness in Ireland, owing to the prevalence of sickness, and its liability to spread at particular periods, and are, even in their present state, so great an auxiliary means for the alleviation of distress and poverty among the people, that it is incumbent upon the government to watch over them with a jealous eye, and to take care, in extending relief to the poor in general, not to curtail of its fair proportions the relief hitherto afforded to the sick poor—the only species of relief ever vouchsafed to the poor of that country, and to which they have a prescriptive right that ought to be respected.

For these reasons it would seem advisable to conduct the administration of the medical charities *apart* from the poor-law commission—still, not entirely independently of it. A means of connexion might be, to make one of the five medical inspectors, to be appointed under the bills of late brought into Parliament, a chief inspector or medical commissioner, and to associate such chief inspector or medical commissioner, with the Poor-Law Commissioners for the time being; to act together in all matters and things appertaining to the management and medical treatment of the sick poor; and to constitute a board to be vested with the general superintendence and control of the medical charities. By this arrangement the Poor-Law Commissioners would, in effect, exercise such a power as would be sufficient for their purpose, without, however, appearing so much to do it; and the chief inspector, or medical commissioner, being resident for the most part in Dublin, would be able to take cognizance of the reports transmitted to the medical charities' office by the four inspectors, who would be employed in

the investigation of the provincial medical charities.

It is expedient, in the next place, to provide by law for the proper allocation of dispensaries, and for increased hospital accommodation; also, besides raising the funds for this purpose, to render them fixed and secure. Instead of leaving it to one or more interested individuals to originate a dispensary where they please, by procuring an equivalent from the county rates for money alleged to have been subscribed, or to grand juries, to diminish or even annihilate the activity of a charity of which there happens to be no patron at hand, these establishments should be restricted to certain localities and a certain population, and at the same time under such circumstances they should be made compulsory. But as ample powers have been provided, according to the drafts of the bills lately laid before parliament, for changing hospital and dispensary districts, and for raising such sums of money as shall be required for erecting as well as supporting and maintaining the medical charities, there is no occasion for me to insist upon these points.

Another leading defect of legislation is, the exclusiveness of the professional qualification required by law for certain medical officers, and the total omission, on the contrary, of any qualification whatever for the rest.

The surgeon, who is in general the only medical officer attached to a county infirmary, must be a licentiate of the Royal College of Surgeons in Dublin. He has a salary of 100*l.* Irish currency, paid out of the treasury; and he almost invariably holds some other appointment, to the county gaol or lunatic asylum, at nearly the same salary. Moreover, he is frequently provided with apartments in the hospital. The character he thus acquires for surgical skill and dexterity in operating, becomes a monopoly: he has no local competitor, and is enabled, having the entire field to himself, to derive considerable emolument from private practice. While such advantages are secured to this privileged class of practitioners, the medical attendants to the dispensaries, instead of having a fixed salary upon which they can rely, seldom know from year to year what sum they are to receive for their services. It depends

upon the amount of subscriptions and presentments, alike fluctuating and precarious. The funds of the dispensaries have declined so materially in several instances, that they barely suffice for the purchase of medicines; and the means of providing a salary are altogether wanting. Still, the duties of the medical attendants, if well discharged, are very onerous—if they visit the poor, as they ought to do, at their own dwellings. But as they have to depend for their pay upon the chance amount of contributions, and to court subscribers, it is to be apprehended that they will consult the convenience of the rich before the necessities of the poor, and especially as other than humane considerations may have led to their appointment.

The fitness of medical men, whatever may be the class of institutions for which they may be candidates, should be duly certified, and some restriction imposed by law to insure the selection of competent individuals in every instance. Neither should it be any longer permitted to exclude from the county infirmaries the members of the London College of Surgeons, or other persons who shall have obtained a liberal professional education in any university or established school in Great Britain as well as Ireland. But the bill now before parliament not only does this, but more; for while it professes to throw the appointments open, it in fact monopolizes *all* the medical charities for the Dublin Colleges. By the qualification clause, notwithstanding the shew of liberality with which it commences, no person is eligible to the office of surgeon who has not pursued his professional studies in some hospital or medical school for five years, attended hospital practice for three years, and been examined for two hours—this being exactly what is required by the Dublin College of Surgeons, but by none other that I know of. Also, it would tend to the advancement of surgical science and to the benefit of the public, if the number of surgeons to the county infirmaries were increased, as well as the invidious distinction done away with. But more than this is required for the proper regulation of these institutions. They should be converted into general hospitals; physicians and surgeons should be alike appointed, the

beds distributed between them, and all truly necessitous persons admitted, if seriously ill, even without the form of a recommendation, giving a preference always to accidents and fever cases.

However, it is not proposed to interfere with the existing rights of those who are now attached to the County Infirmaries or other medical charities. So long as they shall act in their present capacity, their salaries ought to be secured to them; but there is no sufficient reason why their successors should enjoy the like privileges. True it is, that, for services of a higher order, the rate of remuneration should be increased; but the Infirmaries being placed in towns, where there is opportunity and scope for acquiring private practice, pecuniary compensation is not so much an object to the medical attendant as it is in the country villages, where he depends for his livelihood chiefly, if not entirely, upon the emoluments of the office. Hence, instead of leaving the determination of the amount of salary to the subscribers, in the case of the Dispensaries, which is productive of much abuse, as well as hardship on the part of the medical attendants, and fixing it at an unnecessarily high rate for the County Infirmaries, I think that certain limits ought to be prescribed by law, applicable to all the medical officers in like manner. Thus the salary need not exceed 50*l.* in places containing 10,000 inhabitants. For a less amount of population the salary should be increased; and it should never be less than 100*l.* in the rural districts; while, on the contrary, in places containing 20,000 souls, even a less salary than 50*l.* might suffice; and no pecuniary reward whatever need be given if there be 40,000 souls and upwards, especially if the institution be made available for the purposes of medical education. The distinction conferred by the appointment would insure candidates. It will be necessary, however, to prohibit the medical officers from belonging to more than one institution at the same time, as I met with several instances where two, and with one instance where even four, such appointments were held by one individual.

With a view to encourage private contributions for the support of the medical charities, and to increase their amount, the elections of the medical and

other officers should be vested in the subscribers, as heretofore; also, the number of votes might be made proportionate to the sum subscribed. But the Lord Lieutenant should possess the power of a "veto" in such elections, and, in default of subscribers, should nominate and appoint the said officers, and be able to remove them.

Unless the medical and other officers be brought under control, how will it be possible to carry into effect such rules and regulations as are required for the proper management of the medical charities? This, indeed, is a matter of the first importance, and the administrative machinery to be formed for this purpose should consist of individuals well acquainted with the imperfections and defects, as well as the abuses, which have crept into the present management, or rather through want of management into the present system. Unless an uniform and efficient plan of management be established for the medical charities, but little good, comparatively speaking, will be done by the amendments of the law.

The following are rather matters of *detail* than of principle, and cannot well be defined by law; but full power should be given to the medical inspectors (or any three of them, the chief inspector included), to make such rules and regulations as they shall think fit, respecting the dietary, pharmacy, appropriation of wards and rooms, reception of patients, and respecting the occupation of premises and cultivation of ground belonging to the medical charities; also respecting the description of cases and condition of persons entitled to relief; also respecting the number, denomination, and residence of the medical officers, and the nature and amount of their duties in case of hospital, dispensary, or domiciliary attendance, accidents, vaccination, and midwifery; and the medical inspectors, as aforesaid, should be able to alter such rules and regulations from time to time, as they shall think fit.

The necessity of authorizing the medical inspectors to interfere with the existing management, and to regulate the above very important matters connected with the practical operation of the medical charities, will appear from a concise statement of the principal irregularities and abuses which fell under

my notice as Medical Assistant Commissioner upon the Irish Poor Inquiry.

The medical, or other officer, is purveyor of stores of all kinds for the charity, and upon his own terms, as nothing is obtained by contract; or he becomes the contractor for a particular article of diet, and the dietary is so constructed as to favour its consumption. Frequently no daily diet-table is kept, nor any account of the kinds, quantities, and prices, of the provisions received, issued, and in store.

The medical officer, with his family, occupies the best part of the hospital; he builds for his own accommodation on the premises, or he turns the land belonging to the charity to his own profit.

He resides at a distance of four, six, and even eight miles from the institution of which he has the charge.

He attends only once a week at the dispensary; he does not vaccinate, or he makes a charge for it; he does not attend to midwifery cases; he does not visit out-patients; he delegates the compounding and dispensing of medicines to an ignorant and unprofessional journeyman, or servant; he absents himself from duty without providing an efficient substitute.

The beds are occupied by chronic cases by the month together; by patients with scrofulous ulcers, sore legs, sore eyes, &c.; while those who are suffering from acute febrile, inflammatory, and other internal and very dangerous complaints, are inadmissible into the county infirmaries, according to the existing regulations, and are thus left to perish of disease, for want of that very accommodation which is so needlessly bestowed in the former cases.

It is a common practice for subscribers to insist upon advice and medicines *gratis*, for their servants and dependents of all kinds: so that more than the full worth is had for the money subscribed, and the destitute poor are deprived of a large share of the means intended exclusively for their relief.

In proof of speculation and jobbing in the funds, as well as the chicanery resorted to, to cover abuse in the mode of supplying the drugs, I beg to refer to my report (Dispensaries, Part I., Pharmacy and Management, Appendix B., Poor Inquiry, Ireland).

It still remains to provide against two

very serious evils noticed in my report. For their prevention I would suggest that grand juries should be authorized and required to make presentments for such sums of money as may be necessary to provide for the due instruction and qualifying of a sufficient number of midwives, and that, when this shall have been done, all persons should be prohibited, under pain of a small fine, from engaging in the practice of midwifery, who do not hold a certificate, signed by a lecturer on midwifery in a medical school, or a superintendent of a lying-in charity, and counter-signed by a magistrate or other public functionary; also that all persons, whether medical or otherwise, should be prohibited under like penalty from inoculating with the matter of small-pox.

Lastly, I would suggest that a discretionary power should be accorded to the medical attendant to order certain articles of diet, and even of clothing, to be supplied from the funds of the Dispensary, or other charitable society established for this purpose, and to be distributed to the sick poor under such circumstances, and upon such terms, as the medical inspectors shall determine. If this be objected to as involving the principle of out-door relief, let it be borne in mind that it is almost a mockery of charity to prescribe medicines for the sick, unless they have the means of warmth, and can procure common diluent and demulcent drinks, broths, &c. Physic is a very good thing in its way—by physic I mean drugs—but without such collateral aids it may be worse than useless. The able-bodied poor man may contrive to get enough in the way of sustenance to enable the heart to beat, and to make a little blood for the organ to act upon; but when he is sick and deprived of the power of working, what is he to do unless hospital comforts, or the above requisites, can be procured? I am quite sure that it would be much better, instead of applying large sums to the purchase of medicines, and drugging the poor unnecessarily, as is too often the case, to lay part of the money out in supplying oatmeal, pearl-barley, rice, &c. together with blanketing and flannel dresses. Moreover, I am of opinion that such a principle of relief, if guarded by proper checks, might be acted upon without leading to any great abuse or expense,

and certainly with the most beneficial effects, in the administration of dispensaries.

In conclusion, I beg to state my conviction, the result of experience, that, if the law be amended, and competent persons be selected to carry out its provisions, and make such rules and regulations as suggested, the medical charities being in that case well situated, well supported, and well conducted, will be found a most invaluable means of relief to the sick poor; while the advantages resulting to the country at large, from the establishment of a systematic and efficient plan of public medical aid, will greatly exceed the cost that need be incurred by it.—I am, sir,

Your obedient servant,

W. P. BORRETT,

Fellow of the Royal College of Physicians, London.

Queen Ann Street,
Cavendish Square, March 3, 1833.

EARLY RUPTURE OF THE UTERUS.

To the Editor of the Medical Gazette.

SIR,

I BELIEVE the following case to be one of very rare occurrence (in fact, I am not aware of any similar one upon record); and if you should consider it worthy of insertion in your journal, you will greatly oblige

Your obedient servant,

ROGER SHIRLEY NUNN,

Surgeon to the Essex and Colchester Hospital.

Colchester, Feb. 25, 1833.

On Monday, the 19th of February, I was summoned, about 10 o'clock A.M. to attend Mrs. C., whose husband told me she was extremely ill. Upon visiting her, I found she was in a state of collapse, without any apparent cause, as, upon making a minute inquiry, I learned, that although she was in her fifth month of pregnancy, she had been perfectly well up to the previous evening,—that then, and during the night, she had complained of some pain and uneasy sensations in the abdomen,—and that she had *once* vomited. She had of her own accord taken some castor oil early in the morning, which had operated previous to my seeing her. She died within two hours after my

visit. I had known the woman previously: she was about 25 years of age, and had given birth to one still-born child about twelve months since, when her labour was tedious, but natural. She had since had one miscarriage; in other respects her general health was good. Upon a post-mortem examination, I found the external appearances of the body natural; but in laying open the cavity of the abdomen, a large quantity of coagulated and fluid blood, to the amount of four or five pints, was found covering the hypogastric and pelvic viscera: upon removing this blood, I discovered the membranous envelopes containing the fœtus floating in the abdominal cavity, above and anterior to the uterus, attached, however, to this organ by the funis. On clearing away a mass of coagulum, the uterus was found in its natural position, partially collapsed, having a fissure on its superior and posterior wall, from an inch and a half to an inch and three-quarters in length, immediately over the attachments of the placenta, and through which the membranes, with their contents, had escaped. The placenta remained attached to the fundus uteri, save where it was exposed by the rupture, showing there had been no effort to throw off this body. The body of the uterus was rather thinner and much softer than natural, yet the part ruptured showed no greater indications of disease than the whole substance of the organ; the neck was healthy, and the os uteri closed; the right ovary healthy, the left larger than a pigeon's egg, and hydropic; the bladder was empty, and the rectum free from accumulation; the remaining abdominal viscera quite natural in their appearance.

The peculiarities of the foregoing case would seem to be—the rupture of the uterus at so early a period of pregnancy,—the absence of external violence,—of acute pain, or apparent strong contraction of the womb; nor am I aware of any symptoms in her general health which could at all have led me to suppose that she was suffering from organic lesion of so important an organ; the morbid appearances indicating a softening down of the component tissues of the uterus, rather than actual change of structure, as, judging by external appearances only, it might have been considered healthy.

MEDICAL GAZETTE.

Saturday, March 24, 1838.

“Licet omnibus, licet etiam mihi, dignitatem
Artis Medicæ tueri; potestas modo veniendi in
 publicum sit, dicendi periculum non recuso.”
 CICERO.

CAN QUACKERY BE SUPPRESSED?

WE showed, in our late article on this subject, that quackery, far from being suppressed in France, as supposed by the pamphleteer whom we quoted, is comfortably settled there; and that it lifts up its banner, not merely on dead walls, but in the midst of the very Academy of Medicine; for many of the hours of this learned body are employed in sifting the pretensions of nostrums, and reporting to the Minister whether they are worthy of a patent. In fact, next to England, France seems always to have been the country where quackery has flourished the most; from the days of mere mountebanks to those of more refined impostors.

Addison tells a pleasant story of the appearance of a French quack in Paris, with a boy walking before him, and publishing with a shrill voice, “My father cures all sorts of distempers;” to which the doctor added gravely, “The child says true.”

Or, to turn to recent times, we may find some good observations and amusing instances in the *Dictionnaire des Sciences Médicales*, under the head of *Charlatan*; though, in sad truth, the only examples there mentioned, which occur to us at this moment, are irregularities committed by regular members of our corps. One is a story of a physician of Montpellier, who, when he came to a new place, used to pretend to have lost his dog; and then sent the crier about the town to offer a reward of twenty-five louis for bringing it back, taking care at the same time to mention all the doctor's titles and ho-

nours. The townspeople naturally thought that the physician must be both rich and great, to offer so large a reward; and though the dog never was found, patients very frequently were. This facetious story, where the charlatan appears to have had his diploma, tends to lead us to the painful doubt whether the public are not sometimes induced to throw off their allegiance to regular practitioners, by the difficulty of distinguishing them from the intruders, in any thing more material than the possession of a parchment. In such cases, where the regular troops fight with very irregular arms, one is inclined to reverse an old question, and, instead of asking

“*Quid domini facient, audent cum talia fures?*”

to inquire what will the thieves do, when the masters venture on such acts?

Come we now to Austria, and the other states of Germany, where, according to our mistaken fellow-labourer, quackery is suppressed.

Supposing for a moment the assertion to be true, and that not a quack were to be seen from the Rhine to the Baltic, we should still have to weigh the enormous price paid for the exemption, and to consider whether the people of England could be brought to submit to it. This price is neither more nor less than the abrogation of liberty, in speech, writing, or action, and the consequent conversion of men into infants six feet high. In such a state of things the iron hand of authority which attempts to crush the charlatan, presses very hard on the regular practitioner, and intermeddles with him in matters where in Britain no one would dream of the possibility of government interference. Thus in Hanover, and we believe in other German states, the government prescribes the number of punctures to be made in vaccination; and the mode of isolation of persons labouring under small-pox or

scarlet fever, which is enjoined in Prussia, is such as could not be attempted by any but a despotic government. The regulation, however, is evaded—a proof how little even the most absolute authority can do, when opposed to the manners and instincts of the people.

After all, the great difficulty is to ascertain what is really done, and how the laws work, in countries where the press is not free, and where the government attempts to place fetters even on the intercourse of society, and the ordinary topics of conversation*.

In a country like Austria, where even the Prussian journals are deemed too liberal to be circulated, no man can tell what is the actual effect of a given regulation, or pretend to say that a thing does not exist merely because it is not mentioned. Just imagine, if in our own country we were to trust to the garbled accounts and one-sided panegyrics of those who live by a system, or, in other words, were to take official statements for the whole truth, what fantasies we should receive for facts!

Not unlike this are the accounts which the higher classes in such countries palm upon hasty travellers, especially upon those who being ignorant of the language of the people, see every thing through the dim medium of interpretation†.

Thus we are led to the inevitable conclusions that it is very doubtful whether quackery is suppressed in the countries where the pamphleteer supposes this happy phenomenon to have

* A German told us, that at Baden it was deemed *dangerous* to talk about Casper Hauser; that is to say, for some reason, the fate of this singular being was a sore subject with the reigning family, and therefore padlocks were put upon Baden tongues.

† Thus a late traveller represents a prison, in which criminals of the blackest dye are confined at Vienna, as a very pleasant kind of residence—so clean, so wholesome, and the bread so good (just as the water-gruel is with us.) Count Somebody, who showed her about this walled paradise, hinted at only one drawback upon the perfect happiness of the inmates; they are whipped periodically.

occurred; and that it is still more questionable whether the measures adopted for the purpose *there*, would be tolerated one moment *here*. Our well-meaning ally, of Bath, however, feels no doubt upon the subject, and says, that "to argue that measures successfully adopted in every other European country are inapplicable to the free institutions of Great Britain, is absurd and fallacious. If a British government is unable to adopt regulations which enlightened experience has shown to be essential for the preservation of the public health, then its freedom is a curse, and its existence a fiction."

We fear that few among medical men, and hardly any of the laity, will coincide with the opinions expressed in these zealous paragraphs. It is a lamentable thing, unquestionably, that foolish people should be induced to fuddle themselves with drams, under the names of balms and elixirs; and that when they have thus dried up the small portion of brain with which they were originally blessed, they should doggedly bring on an enteritis with colocynth and gamboge in hygienic doses. Yet this mania can never destroy the British constitution, though it may their own; and we cannot agree with the author that the existence of the former is in danger of being reduced to a fiction by any quantity either of purgatives or stimulants.

If, indeed, it were a common thing to hear of Lynch-law being executed on helpless patients; if they were tied to trees while a boxful of No. 1, or No. 2, was forced down the œsophagus; we, too, might cry out for the *paternal* care of the Austrian government: but while no force is exercised upon our deglutition, we must be content with the mingled yarn of liberty, and take the evil with the good. After asserting that the law in continental Europe protects the rights of the qualified practitioner, the author of the pamphlet

says, "This is also the privilege of the legal profession in England, which allows no one to assume the title and powers of advocate except in his own cause." We wonder that he did not see that unqualified advocates can be prevented from pleading in court, but cannot be prevented from giving opinions at their chambers; just as unqualified practitioners of physic do not get appointments in the army or navy, but practise on those who accidentally fall into their clutches. The official discouragement is the same in both cases; only that, from the custom of the professions, the unqualified chamber-counsel would get little or no practice, as his clients, the attornies, would be aware of his ignorance; while the mock-doctor, addressing himself directly to the multitude, and not through any such critical medium, is less easily detected. In the lower walks of the law, however, we believe that a good deal of business is done among the poor and uneducated by unqualified attornies.

Another comparison might be instituted with the remaining learned profession; and we should see here, likewise, how difficult it is to abolish quackery by law. We believe that in this present year, 1838, no one proposes to put down the Jumpers or Irvingites by force. If the war against uneducated empiricism should ever be carried on with the spirit which the Bath pamphleteer desires, it would assuredly be requisite to consider the patients as accomplices. The possession of an embrocation of which he could give no satisfactory account, would subject a sick man to fine or imprisonment; and the excise must be empowered to seize antibilious pills, like *run* brandy or silks. To adopt the question of a former writer, "Will my Lady Bonnet be allowed (like the master of a house under the Conventicle Act) to physic her household and sundry poor

neighbours, provided that her hospital does not exceed twenty beds? Shall we hear of the triumphant seizure of a cask of pennyroyal water? or will rhubarb pies be searched, charged on oath with being made with the root instead of the stalk?" That these severities, if feasible, would do something towards suppressing charlatanism, we do not doubt; the difficulty would be to get them enacted; the impossibility, to put them into execution.

The author whom we have so often quoted does not date his pamphlet from Utopia, and yet his *form of petition for the abolition of quackery* contains the following clause:—"It is therefore humbly proposed, that all patent, quack, and secret medicines, be abolished." To ask for the withdrawal of patents from nostrums is no more than reasonable; but to ask Parliament to abolish all quack and secret medicines, is to ask them to abolish the frailty of human nature; it is humbly to propose, that no knaves be permitted to set traps, nor any fools to fall into them.

We can hope, therefore, for improvement on this score, not from the establishment of a medical inquisition, but,

1st, From the more equitable administration of the laws; so that quacks convicted of the most culpable homicide, or of the unpardonable offence of keeping an unlicensed madhouse, may not be let off almost unscathed.

2dly, From the more general diffusion of sound knowledge, the best preservative against the artifices of the medical impostor.

3dly, Our surest hope would be founded on the disuse of quackish tricks among regular practitioners; and we could appeal to the public with more confidence, if doctors did not, like him of Montpellier, cry dogs which they had not lost, and advertise books which they had not written.

ON ANIMAL MAGNETISM.

No. IV.

THERE now remain for our notice only the most marvellous of the phenomena said to be produced by animal magnetism. Before entering upon them it is of importance to observe, that all the supernatural powers said to be acquired in magnetic somnambulism have seemed to be enjoyed by the founders and early adherents of several sects of religious fanatics, and by those supposed to be either inspired, or possessed by evil spirits, to have sold themselves to the devil, or to have been bewitched. These powers, too, they have generally exhibited during fits of convulsions, trembling, delirium, &c., in short, while in a state of extasis often so similar to that which the magnetizers absurdly call somnambulism, that the same description might serve for either.

The faculties attributed to the possessed and inspired, have been—speaking and understanding of unknown tongues; sudden exaltation of the intellectual faculties; knowledge of future events, and of circumstances occurring at distant places; knowledge of thoughts unexpressed; great development of physical force, and a power of remaining for a long time suspended in the air; all which powers have been also ascribed to the magnetized. In like manner both have always arisen under strong mental excitement in highly irritable persons, and most especially in hysterical women; both have become, as it were, epidemic from imitation, so that in all instances, while a general character has been preserved, there have been some slight peculiarities arising from the copying of the model which the first case by accident or design had presented. Both have been constantly influenced by impressions on the mind.

We shall now proceed to examine these powers only as attributed to somnambulists, remarking, that whoever believes that they are possessed in one set of cases, is bound to believe them to have been present in the others (which are by description their very homologues,) and that, on the other hand, if proved deceptions in the one, we have every reason for suspecting fallacy in the other. No rational man believes now in possession, or in fanatic inspiration, or sorcery. Is there more

ground for believing the power of the weird-sister, mesmerism?

To begin with the faculty of prophecy. There is no evidence that any one of the hundreds of these predictions ever came to pass, except those which related to the return of hysterical or epileptic fits, or of imaginary sensations, or of sleep, either in the prophetess herself, or in the person prophesied to, and that these proved frequently true we are not inclined to doubt. For we know that at whatever time an hysterical girl wishes or expects a paroxysm, she can have one—they lie so near the surface.

M. Bertrand gives an amusing account of how his patients prophesied the returns of their fits and their recoveries. They always fixed a time at which, or very near to which, some clock would strike within their hearing, and the event took place *by that clock*; for if it was altered, the event was delayed or accelerated in correspondence with it. We do not mean to say that in all these cases the patients were intentional impostors—they deceived themselves; for believing that at the time a fit would occur, a real one doubtless did, and that they should hear the clock strike during their sleep or somnambulism is not extraordinary; the expectation of it occupied their minds on falling asleep, and their attention and hearing were therefore alive to it. A similar thing is observable in most persons; if they go to bed extremely anxious to be up at a certain hour, they are sure to wake at that time, and very often at the precise minute on which they had fixed, especially if that be a *striking* minute—as one of the quarters. Such is the summum of magnetic prophecy.

The next power we shall notice is, that of knowing what is occurring at miles distance, in places altogether removed from the senses, and unknown except to the mental eye. There is no reasoning on such a thing as this; but if we believe such statements as that a girl at Strasbourg could, during her sleep, accurately describe Paris, which she had never seen or heard of, except vaguely, or could relate exactly what, at the moment she was speaking, was occurring in a house in the next street, from which nothing was sensible to others, then we have no right to disbelieve that the two Taliacotian noses at Brussels dropped off at the moment

that their original owner died in Naples; or that rubbing healing ointment on a sword would cure the wound it had made; or that applying styptics to a shirt would check the hæmorrhage from its owner's body. For, let it be remembered, these things are not the inventions of those who parodied on them; but the three absurdities we have just mentioned were deemed so well authenticated, that they formed a principal support of Maxwell's theory of a universal fluid, which Mesmer copied, and published as his own. *Yes! from these things Mesmerism sprung.*

It is in this pretended faculty of seeing what is going on at vast distances, that the magnetic practices resemble most closely the tricks of conjurors and magicians. From time immemorial, magicians have been in the habit of exhibiting in mirrors or in clouds of smoke not only the future destinies of those who came to consult their horoscopes, but the forms of those whom each most desired to see, representing them as engaged in the very act which they were at the same instant performing perhaps in a distant land. Our present space will not permit us to do more than allude to this, the favourite practice of the heroines of romances, and refer the reader to Sir D. Brewster's *Natural Magic*, and Sir W. Scott's *Demonology*, for cases, and the explanation. At present it seems to be seriously practised only in Egypt, as described first (we believe) by Lord Prudhoe and Major Felix, and lately by Mr. E. Lane, in his *Account of Modern Egypt*.

With regard to the power of understanding the unexpressed thoughts, M. Bertrand says that the patients were *very seldom* able to do it, and that when they could, it was only by guessing from the expression of his face, as he proved by acting a particular thought by which he was always able to deceive them. It is well known that the same method of judging, or rather guessing, the thoughts, was practised in the declaring of oracles, and by the pretended inspired. As a proof of the difficulty of detecting such impositions, we may mention, that, for a long time, M. Bertrand was inclined to think that by his will he could, as M. Puysegur thought he did, direct the thoughts of his patients to whom it was conveyed without words or looks.

The capability of detecting magnetized things—as water, dresses, trees, &c.—requires still shorter notice. Whenever the patients had to choose *the one thing* out of many, they made mistakes: this happened in several instances before the first Commissioners, and M. Bertrand amusingly describes how he deceived his patients by sending them unmagnetized letters, &c.; which, being supposed by them to be replete with virtue, always produced the usual effects.

We come, lastly, to the incredible statement that the senses may be transferred from their proper organs to the epigastrium, the tips of the fingers, occiput, &c. When we say that we disbelieve the accounts of these cases *in toto*, we must again remind the reader that we do not deny the assertions of magnetizers only, for these fables were told long before their time, and instances are said to have occurred since, in those who have never been magnetized at all. The first magnetic cases were those of M. Puysegur, of whose insanity we have so little doubt that we cannot regard any statement he makes as worth the least consideration. However, while the effects he was said to have produced were the subject of constant conversation at Lyons, there occurred, in M. Petetin's practice, eight cases of catalepsy, in all of which there was transference of the senses. Now this fact explains itself; no other physician, we believe, ever had eight cases of catalepsy, for it is so rare that Cullen doubted its existence, and believed all the cases ever related to have been impostures; and in no case that any one else ever saw was this incredible phenomenon present. The conclusion is clear: eight hysterical women heard of people who had fits, and with them supernatural powers: by the common tendency to imitation, which has constantly made such affections epidemic, they modelled their fits on the new plan, and pretended, or perhaps believed, that they too had the same faculty of seeing without eyes, which (with that love of deception which is so common among hysterical women, that it is sometimes quite characteristic of the disease), they lost no opportunity of exhibiting, and neglected no artifice by which they might appear so endowed. From this time the possession of these powers has been an

object desirable of attainment to every somnambulist; and we have no doubt that every case has been based on deception, either with actual imposture both in the magnetizer and the patients, as in M. Berna's case, or by the patient deceiving herself and those around her, or wilfully carrying on an imposition either for mischief-sake or gain.

A few of the phenomena referred to transference of vision are clearly explicable by circumstances we have already alluded to; as by the apparent exaltation of intellect which some seem to acquire during sleep, when every thing that could distract their attention is removed. Such, among many, are the cases in which, in cerebral disorders, a language long forgotten is again remembered; and those in which things overheard, and apparently forgotten, are recollected, as in an instance mentioned by Coleridge, in which an ignorant servant girl, during delirium, repeated long passages on theology in Latin, Greek, and Hebrew. She had been formerly in the house of an eccentric clergyman, who was fond of walking up and down the kitchen reading aloud. Dr. Abercrombie relates many similar instances. To others we have a clue in the cases where, in dreaming, persons have circumstances revealed to them which, while awake, were hidden, as in that, amongst several given by the same author, in which the gentleman was correctly told, by the shade of his father, where some important papers he had long searched in vain for were laid. Indeed, it is commonly observed that during sleep, with the mind undistracted, the memory is far more lucid than when the mind is harassed in the day. The annals of somnambulency are full of such cases, and every old woman has a store of authentic histories of prophecies, and omens revealed during dreams and sickness; they are the parallels of the magnetic statements, and the same explanations serve for both. The magnetizers, however, have far surpassed the tales of the rock-staff, and have brought, as we have seen, statements on which reasoning (if possible) would be useless. It can only be said, that they are contrary not only to common sense, but to every fact, and law of physical and vital science. If they be true, every thing else is false; the form and colour of objects have no relation to light; light

itself is a chimera, yet matter of the greatest density can neither resist its passage, nor divert its course; the complicated apparatus of the eye is a superfluous ornament; a brain and nerves mere playthings for philosophers to waste their time on.

This, then, is our case. Every credible effect of magnetism has occurred, and every incredible one *is said* to have occurred, in cases where no magnetic influence has been exerted, but in all which, excited imagination, irritation, or some powerful mental impression, has operated: where the mind has been alone acted on, magnetic effects have been produced without magnetic manipulations: where magnetic manipulations have been employed unknown to the patient, and therefore without the assistance of the mind, no result has ever been produced. Why, then, imagine a new agent, which cannot act by itself, and which has never yet even seemed to produce a new phenomenon?

CHEMISTS GIVING EVIDENCE AS SURGEONS.

To the Editor of the Medical Gazette.

SIR,

YOUR publication has hitherto exposed any evil practices towards our profession which have been communicated to you; I therefore do not apologize for troubling you on the present occasion, as I think it part of the duty of a journalist to protect that profession to which he is allied.

If you will refer to the *Times* newspaper of Wednesday last (or Thursday), you will find the details of an inquest held near Golden Square, in which a chemist, in Piccadilly, is related as having been called in to the object of the inquiry there narrated, and his evidence before the jury is given at full length. This witness is styled a "surgeon." I only know the individual by name, but believe he is a very respectable chemist; and as long as he does not travel out of his proper path, there is no fear of his name being brought before the public; but when a person who has not received a medical education, has undergone no medical examination, and belongs to neither of the three medical institutions of the kingdom, renders himself amenable to the law, by taking on himself the responsibility of a medical practitioner, which I can prove has been done by the individual in question, to my own knowledge, in cases of grave importance,

I do not think the Colleges are fulfilling their duties in neglecting to prosecute such a case, nor any member of the profession acting up to the oath he takes on entering his profession, in which he swears to maintain and uphold the respectability of his profession, if he allows it to pass by unnoticed. I take up this question as one of medical policy, and not as affecting an individual; and if he should be prosecuted for meddling with subjects unfitted for his craft, he must remember that the object of the writer of this letter is to do justice to his brethren, as well as to the public; for who can say that a medical man might not have saved the life of the deceased this is merely mentioned for argument, for we are aware no medical man could have done so in the present instance, or have found (after an examination of the body) some other cause of death than that stated by this *sci-atisant* practitioner?—I am, sir,

Your old subscriber,

CYCLOPS.

March 3, 1858.

P.S.—Another instance of a chemist giving medical evidence at an inquest, not three months since, has come to my knowledge; but as the inquiry did not appear in the papers, I abstain from further comment.

HOLLOWAY'S UNIVERSAL OINTMENT.

To the Editor of the Medical Gazette.

SIR,

AN old patient of mine, afflicted with prurigo, expressed a wish to have my opinion of a patent medicine called "Holloway's Universal Family Ointment, or the Prince of Remedies." As I have, in a long practice, uniformly condemned all secret nostrums, I unhesitatingly considered that it would be derogatory to the profession to countenance any of these numerous pretended remedies for the cure of every ill which "flesh is heir to." I therefore, without any investigation, advised my patient to have nothing to do with it. I had lost sight of the circumstance altogether till this day, when a placard, with a frontispiece displaying several ulcerated legs and other embellishments, was put into my hands, by a dispenser of such delicacies, in the public streets. I generally refuse such presents, but seeing a curious set of pictures, I perused the document, and carried it home. But, judge my surprise, when I saw the respectable names of Sir Benjamin Brodie, Mr. Mayo, &c. appended to the list of recommendations for the "Prince of Remedies." I confess I was puz-

zled at this, having been taught to revere the names of those men who, by deep study and important discoveries in their profession, have laid down a character for themselves, *perennius are*. If this ointment has such wonderful virtues, and is so highly recommended by the principal members of our profession, perhaps it would not be unbecoming in me, as a humble member, and eager to seek information, to request, through the medium of your journal, to be apprized of its composition, in order that I may, with others, give my patients the benefit of so great a discovery.

I am, sir,

Your obedient servant,

ROBERT SEMPLE,

Member of the Royal College of Surgeons in London, &c. &c.

Rufford's Row, Islington,
8th March, 1835.

[Mr. Semple will find at page 832 of the present volume a letter from Sir Benjamin Brodie, expressive of his indignation on perceiving the use which had been made of his name in connexion with this nostrum. With regard to Mr. Mayo, as he is made to say that he has found the ointment of use "in ALL the cases in which he has tried it," we cannot but suppose that his testimonial, like that of Sir Benjamin, has been falsified.—ED. GAZ.]

ON THE TREATMENT OF SYPHILITIC BUBOES BY SETON.

By PROFESSOR LEVICAIRE,
Of the Marine Hospital, Lyons.

DR. L. states that he has employed the seton most successfully. His plan is, as soon as he perceives that the bubo contains matter, to pass a strong, round, straight, long needle, carrying a thick thread, in the direction of the fold of the groin. The points of entrance and exit are those at which the gland first begins to soften. He permits the seton to remain for only twenty-four or twenty-eight hours in quiet, and sometimes to promote irritation, and prevent the too rapid healing of the openings, moistens it with a weak caustic solution, and for the first days lays on an emollient poultice. When this is no longer necessary, he dresses it with a handful of cotton (unwrought) in order to promote the exit of the matter, the adhesive inflammation, and the development of granulations. This is supported by a bandage round the loins, and exercises a very gentle, steady pressure. Dr. L. thinks every thing disadvantageous which pro-

motes the absorption of the matter. The matter here escapes along the seton; the walls of the abscess come gradually together; the air cannot penetrate through the opening, which is small, and filled by the seton, and the seton causes a healthy action, by means of which granulations are favoured. No cicatrix remains behind, and only three or four days are sometimes necessary for the healing of the bubo.—*Bulletin Gen. de Ther.* April, 1837, and *Dublin Journal*.

A GENERAL BILL

OF THE

BURIALS, WITHIN THE CITY OF LONDON, AND BILLS OF MORTALITY,

From Dec. 13, 1836, to Dec. 12, 1837.

DISEASES AND CASUALTIES OF THE YEAR.

<i>Diseases.</i>			
ABSCESS	118	Brain	184
Age and Debility ..	2605	Lungs and Pleura ..	506
Apoplexy	342	Influenza	468
Asthma	1031	Insanity	164
Cancer	75	Jaundice	23
Childbirth	171	Jaw, locked	3
Cholera	16	Liver, diseased	159
Consumption	3259	Measles	577
Constipation of the		Miscarriage	23
Bowels	11	Mortification	152
Convulsions	1676	Paralysis	144
Croup	107	Rheumatism	24
Deafness or Teeth-		Scrofula	9
ing	379	Small-pox	277
Diabetes	7	Sore Throat and	
Diarrhea	57	Quinsey	34
Dropsy	691	Spasm	53
on the Brain ..	504	Stone and Gravel ..	9
on the Chest ..	64	Stricture	10
Dysentery	12	Thrush	51
Epilepsy	31	Tumor	31
Erysipelas	50	Veneral	5
Fever	710	Worms	1
(Intermittent or		Unknown Causes ..	2851
Ague)	5	Casualties, as under, 305	
(Scarlet)	189		
(Typhus)	191		
Fistula	6		
Gout	46		
Hæmorrhage	28		
Heart, diseased ..	142		
Hernia	15		
Hooping-cough ..	647		
Hydrophobia	3		
Indigestion	15		
Inflammation	1631		
Bowels & Stomach	226		

Casualties.

Drowned	91
Died by Visitation	
of God	52
Excessive Drink-	
ing	4
Found Dead	11
Killed by various	
Accidents	107
Murdered	1
Poisoned	5
Suicides	34

Buried { Males 10,605 } Total 21,063
 { Females 10,458 }

Of the number buried were,

Under 2 years of	60 and under 70 ..	2246
age	70 and under 80 ..	1860
2 and under 5 years	80 and under 90 ..	710
2003	90 and under 100 ..	84
5 and under 10 ..	100	1
774	100	2
10 and under 20 ..	100 and 102	1
755	103	2
20 and under 30 ..	108	1
1555		
30 and under 40 ..		
1937		
40 and under 50 ..		
2204		
50 and under 60 ..		
2038		

OF

DRUGS ON SALE IN THE ENGLISH MARKET,

With their Prices and several Duties.

(From the Official Returns, March 20, 1838.)

	PRICE.			DUTY.			DUTY PAID	
	£	s.	d.	£	s.	d.	In 1838 to last week	Same time last year.
Aloes, Barbadoes, D.P. c	12	0	0	30	0	0	} B.P. lb 0 2 } F. lb 0 8 }	14,174 24,467
Hepatic (dry) BD. c	5	0	0	14	0	0		
Cape, BD. c	2	8	0	—	—	—		
Aniseed, Oil of, German, D.P. lb	0	9	6	0	9	6	F. lb 1 4	
E. I. lb	0	7	0	0	7	6	E. I. 1 4	254 328
Asafoetida, B.D. c	2	10	0	5	0	0	c 6 0	35
Balsam, Canada, D.P. lb	0	1	3	0	1	4	lb 0 1	329 401
Copaiba, BD. lb	0	6	6	—	—	—	c 4 0	34 67
Peru, BD. lb	0	4	3	—	—	—	lb 1 0	461 142
Benzoin (best) BD. c	25	0	0	50	0	0	c 4 0	23 20
Camphor, unrefined, BD. c	9	0	0	—	—	—	c 1 0	119 165
Cantharides, D.P. lb	0	5	6	—	—	—	lb 1 0	4,833 6,821
Caraway, Oil of, D.P. lb	0	9	0	—	—	—	lb 4 0	291
Cascarilla or Eleutheria Bark, D.P.C. lb	1	15	0	—	—	—	lb 0 1	3,240 1,836
Cassia, Oil of, BD. lb	0	7	0	—	—	—	lb 1 4	1,241 7,88
Castor Oil, East India, BD. lb	0	0	6	0	0	10	c 1 3	} 1,336 1,671
West I. (bottle) D.P. 1½ lb	0	2	3	—	—	—		
Castoreum, American, lb	1	15	0	—	—	—	} lb 0 6	256 147
D.P. Hudson's Bay lb	1	0	0	1	4	0		
Russian lb	—	—	—	none	—	—		
Catechu, BD. Pale c	1	8	0	—	—	—	} c 1 0	1,796 10,152
Dark c	3	0	0	—	—	—		
Cinchona Bark, Pale (Crown) lb	0	2	0	0	3	6	} lb 0 1	21,582 48,405
BD. Red lb	0	2	0	0	4	0		
Yellow lb	0	2	0	—	—	—		
Colocynth, Turkey lb	0	2	6	0	4	0	} lb 0 2	1,192 3,212
D.P. Mogadore lb	0	3	0	—	—	—		
Calumba Root, BD. c	1	4	0	2	5	0	lb 0 2	8,441 4,898
Cubebs, BD. c	5	0	0	—	—	—	lb 0 6	8,997 8,858
Gamboze, BD. c	5	0	0	15	0	0	c 4 0	28 37
Gentian, D.P. c	1	4	0	—	—	—	c 4 0	203 248
Guaiacum, D.P. lb	0	1	0	0	1	8	c 6 0	16
Gum Arabic, Turkey, fine, D.P. c	8	0	0	9	0	0	} c 6 0	1,865 974
Do. seconds, D.P. c	5	0	0	7	0	0		
Barbary, brown, BD. c	3	0	0	3	3	0		
Do. white, D.P. c	4	15	0	—	—	—		
E. I. fine yellow, BD. c	3	0	0	3	10	0	c 6 0	562 596
Do. dark brown, B.D. c	1	15	0	2	5	0	c 6 0	2,436 171
— Senegal garblings, D.P. c	4	15	0	5	0	0	c 6 0	142 60
— Tragacanth, D.P. c	8	0	0	12	0	0	c 6 0	3,197 810
Iceland Moss (Lichen), D.P. lb	0	0	2½	0	0	3	lb 0 1	5,741 3,627
Ipecacuanha Root, B.D. lb	0	1	9	0	2	0	lb 1 0	8,789 7,899
Jalap, BD. lb	0	2	3	—	—	—	lb 0 6	9,111 12,195
Manna, flaky, BD. lb	0	4	0	0	5	6	} lb 0 3	2,463 11,706
Sicilian, BD. lb	0	1	7	—	—	—		
Musk, China, BD. oz	1	0	0	1	8	0	oz 6 0	515 405
Myrrh, East India, BD. c	5	0	0	14	0	0	c 6 0	25 43
Turkey, BD. c	2	0	0	11	10	0	} lb 2 6	162 294
Nux Vomica, BD. lb	0	8	0	0	9	0		
Opium, Turkey, BD. lb	0	13	6	—	—	—	lb 1 0	7,132 8,561
Peppermint, Oil of, F. BD. lb	1	1	0	—	—	—	lb 4 0	57 341
Quicksilver, BD. lb	0	3	6	—	—	—	lb 0 1	56,783 66,184
Rhubarb, East India, BD. lb	0	2	6	0	3	3	lb 1 0	8,789 7,899
Dutch, trimmed, D.P. lb	0	3	6	0	4	0	} F. lb 1 0	2,475 1,936
Russian, BD. lb	0	8	3	—	—	—		
Saffron, French, BD. lb	0	18	0	—	—	—	} lb 1 0	1,109 1,580
Spanish lb	0	19	0	—	—	—		
Sarsaparilla, Honduras, BD. lb	0	1	0	0	1	9	lb 0 6	21,053 22,679
Lisbon, BD. lb	0	2	0	—	—	—	} lb 2 6	2,143 2,584
Scammony, Smyrna, D.P. lb	—	—	—	—	—	—		
Aleppo lb	0	12	0	0	15	0	} E. I. lb 0 6	18,895 25,058
Senna, East India, BD. lb	0	0	3	0	0	4		
Alexandria, D.P. lb	0	1	6	—	—	—	} Other sorts 0 6	17,557 20,066
Smyrna, D.P. lb	0	1	0	0	1	3		
Tripoli, D.P. lb	0	1	0	0	1	3		

‡§§ BD. In Bond. — c. Cwt. — B. P. British Possessions. — F. Foreign. — D. P. Duty paid.

ACCIDENTS ADMITTED AT THE LONDON HOSPITAL.

Surgeon, MR. SCOTF.

Assistant-Surgeon, MR. HAMILTON.

Feb.	Sex.	Age.	Case.
21.	F.	16	Burn.
22.	M.	19	Punctured hand.
	F.	19	Contused knee.
	F.	60	Injured hip.
	F.	53	Contused side.
23.	F.	4	Bite of a dog.
	F.	19	Fractured tibia.
21.	M.	21	Fractured tibia and fibula.
	M.	40	Contused foot.
24.	F.	10	Scald.
	F.	44	Hernia.
	M.	23	Injured leg.
	M.	31	Fractured fibula.
	M.	24	Dislocated humerus.
25.	M.	19	Sprained ankle.
	M.	24	Sprained ankle.
26.	M.	16	Retention of urine.
	M.	39	Retention of urine.
	F.	6	Burn.
	F.	24	Injured abdomen.
	M.	38	Cut head.

In-patients 22

Out-patients 37

Total 59

Surgeon of the Week, MR. LUKE.

Assistant-Surgeon, MR. CURLING.

Feb.	Sex.	Age.	Case.
27.	M.	7	Contusion on the face.
	M.	66	Fractured ribs.
	M.	57	Strangulated inguinal hernia, operated on.
	M.	7	Severe burn.
	F.	38	Cut head.
28.	M.	5	Fractured tibia.
	F.	14	Injured groin.
	M.	1	Fractured femur.
	M.	42	Wounded vein.
	M.	54	Contused knee.
March			
1.	M.	60	Fractured femur.
	M.	40	Contused foot.
	M.	7	Severe scald.
	M.	23	Contused elbow, succeeded by severe inflammation.
	M.	50	Fractured ribs.
	M.	37	Fractured leg.
	M.	39	Fractured ribs.
	M.	14	Fractured leg.
2.	F.	35	Contused leg.
	M.	30	Severe contusions.
	M.	7	Burn.
	M.	20	Sprained ankle.
3.	M.	21	Fractured leg.
	M.	5	Extensive burn.
	F.	37	Contusions on the head.
4.	M.	37	Sprained ankle.
5.	M.	30	Scald on the foot.
	F.	33	Contused foot.
	F.	48	Fractured leg.
	F.	23	Strangulated femoral hernia, reduced.

In patients 30

Out-patients 38

Total 68

[We regret that we have been obliged to omit the remarks for want of space.—E.G.]

WEEKLY ACCOUNT OF BURIALS.

From BILLS OF MORTALITY, Mar. 20, 1838.

Age and Debility 32	Hooping Cough 13
Apoplexy 4	Inflammation 12
Asthma 16	Brain 4
Childbirth 2	Lungs and Pleura 7
Consumption 41	Insanity 7
Convulsions 23	Liver, diseased 1
Croup 1	Measles 6
Dentition or Teething 5	Miscarriage 1
Dropsy 12	Mortification 4
Dropsy in the Brain 2	Paralysis 1
Fever 23	Small-pox 7
Fever, Intermittent, or Ague 1	Unknown Causes 70
Fever, Scarlet 1	Casualties 3
Fever, Typhus 7	
Heart, diseased 4	

Increase of Burials, as compared with }
the preceding week } 47

CONCLUSION OF THE VOLUME.

Our readers will perceive that we have given an additional sheet with this number, and postponed the Index till next week. By this arrangement we have been enabled to complete Mr. Hawkins' valuable set of Lectures on Tumors, and to bring to a close several papers, the commencements of which had either previously been given, or which refer to subjects discussed in the present volume.

We have to apologize to many correspondents for the delay—quite unavoidable, however—in the publication of their papers. Dr. Borrett's letter, which is of more recent date than some of those still postponed, has been inserted this week because it refers to a subject which is at the present moment under the consideration of Parliament.

The Lectures of Dr. Williams and Dr. Southwood Smith, interrupted during our last two numbers by press of matter, shall be resumed with the commencement of our ensuing volume; in which we shall also continue those of Sir Benjamin Brodie and Mr. Lawrence, and give the papers of Dr. Clutterbuck, previously announced.

NOTICE.—We cannot insert Dr. Turnbull's remarks on Ipecacuanha: we are not aware of their containing any point not already known.

IRISH APOTHECARIES' HALL.

[THE following was received very soon after the publication of the letter to which it is an answer. The delay in its insertion has been caused by what proves to have been a mis-understanding on our parts as to the wishes and intentions of the writer.—*Ed. Gaz.*]

To the Editor of the Medical Gazette.

SIR,

As I was the author of the letter headed "Queries for Apothecaries," in the *Dublin Evening Mail* of 1st December last, and signed "A Graduate in Medicine, Surgery, and Midwifery, of Dublin, Edinburgh, and London," which, however, was only called forth by a series of violent resolutions against the Irish College of Surgeons, passed on the previous 20th of November, and published in Saunders's Dublin News-letter, of 22nd Nov., by a voluntary "Society of Apothecaries" in this city, you will, I am sure, allow me to reply to a letter in your 18th number, viz. of 20th January, 1838, (pages 655—659) signed "A Professor in the School of the Apothecaries' Hall of Ireland," which attempts to answer, or rather to evade, my former one.

As your pages 667-8 of the same number, under the heading "Irish College of Surgeons—Pharmacy Laws," copy correctly an advertisement which appeared in all the Dublin papers, giving an abstract of our objects, I shall merely refer your readers to them, as sufficiently explanatory of what we really wish and intend to do, which will save space, and avoid repetition, as well as prevent misrepresentation.

According to your requirement, I supply you with my name, (which is probably, however, well enough known already here, in this controversy), though I cannot see, in a question of this kind, which all depends on general reasoning, and on arguments founded on printed documents, not at all on facts, stated on the writer's authority, on either side, how real signatures can be of any value, or add any weight; and, to prevent personalities, I should have thought it wiser, in such a matter, not to sign or require them. For brevity's sake, I shall call the writer in your pages the "Professor," as he calls me the "Graduate of three colleges."

To those parts of his letter which contain personalities about the "Graduate," it would perhaps be as well not to make any reply, except by merely quoting them; but my general plan is, not to leave any thing material unanswered.

As to the "Professor's" insinuation in this sentence, "How, I would ask, is a member of any public body to promote its honour? Is it by being idle, and not exerting the talents with which he may have been endowed? Such a course might serve the purposes of others, perhaps of the 'Graduate,' but it would reflect little credit on himself or his College." My answer is briefly this:—I much suspect, that I have been at any period of my life as little idle as the "Professor," at least, and have perhaps too made more by private or public practice, than any one or two, or possibly three, of his whole body of professors, including himself; and he knows, or should have known, that I have filled three laborious professional

appointments under Government, and others in public institutions. Let him, then, say, was I idle, when in four years only of one of these Government situations, I had to prescribe, as the books showed, 35,296 times, and to pay besides 16,161 visits to the poor, at their own houses? Has he done as much in his whole life? Thus I could negative the insinuation of idleness, without any of the other proofs of activity, energy, diligence, and perseverance, which I might multiply ten-fold. It is very easy for young men to set up as lecturers, if private practice or public duties do not better occupy their time; and a very wise plan it is of "*advertising*" oneself.

To his remarks on the style or grammatical composition of that letter, I shall offer no reply, except to say, let the public read both, (allowing for a few typographical errors by the newspaper printer), and after having thus seen which can write the best English, or the most consecutive and condensed arguments, let them decide. In fact, however, the chief fault of my letter, I should think, was, that its English and its arguments were so plain, as to be incapable of being mistaken or evaded. If, indeed, you would consent to republish my former letter, of which, at the time, I furnished you with a copy in the newspaper, in that hope I should think it quite needless now to answer the "Professor" at all.

Any one reading it will see that he misstates its design, ignorantly or intentionally, when he says, "The main design of the writer of this letter appears to be two-fold; 1st, to throw odium upon the apothecaries' branch of the medical profession; and 2ndly, to promote thereby the attainment of an object at present desired by a party in the Irish College of Surgeons, by degrading it and its members from the station they have hitherto held." As to party or parties in the College, he must know that there are at present none, and that all our late proceedings on the "pharmacy laws" have been unanimous. And as to myself, he knows that I never, at any time, joined any party in the College, on any question, but voted on different sides on various occasions, according to my own views of duty to the College, the profession, and the public. But you will see, by and by, that he

here but echoes what the apothecaries' resolutions had said, though he declares, "I am not in their counsels, nor am I their advocate." True it is, that I held up to just odium and ridicule the exacerbatd resolutions of the volunteer "Society Apothecaries;" it is also true, that I exposed the underhand proceedings of certain apothecaries who try to worm themselves into practice without license or diploma; most true it is, too, that I pointed out the shortcomings and oversteppings of their own "act of incorporation" (of 1791), by the "directors" of the "Apothecaries' Hall;" with the assumptions and encroachments, neglects and omissions, of all three parties, and well each deserved and has felt my reproof; but of the *genuine apothecaries*, as individuals, and of their calling as a needful and useful "profession," I spoke respectfully in different parts of my letter, as follows:— "You (the 'Society of Apothecaries') misrepresent us wholly. The person we highly approve of, and wish to benefit as much as ourselves, is the 'apothecary,' truly named and truly so, as created by that act." "It is the right of the apothecary, by it, to make up our prescriptions." "Did we not solicit your, and others', and all help," only "for the removal of the abuses and imperfections in the existing laws, for regulating the business of an apothecary," and that solely by means "wisely" chosen, when we added "information and suggestions, calculated to forward these objects, shall receive every attention?" "We will do no injury to apothecaries, their Hall, or charter," (or, rather, 'act of incorporation,' for they never had a charter). "We wish well to the Apothecaries' Hall." "We wish its charter (act) improved and confirmed." "To help to protect you against druggists and chemists, now beginning, as in England, to usurp your rights, we are willing to lend our aid." "You say, we wish to 'become apothecaries.'" "Nay, but we wish to make you so, as your charter (act) does." "Be, however, 'assured' of this, gentlemen, we have no enmity to *bona fide* apothecaries. Are they not a necessary branch of our own profession? We have no wish to annul their charter (act); we think it imperfect, defective, inadequate. There are faults, too, in our own (charter). Should you, then, hastily oppose

our taking measures to correct both?"

"And now, gentlemen, take my friendly advice. Know your friends, the surgeons and physicians, from your enemies, the druggists, chemists, Medical-Hall keepers, and unlicensed apothecaries. In future, keep your temper," &c. "Gentlemen, what we want is an equitable general law, to regulate the whole profession, with the education and practical duties of physicians, of surgeons, and of apothecaries, to make mutual inroads impossible, to make pharmacists, druggists, and Medical Halls, keep their proper sphere." "Allow me, with all due personal respect, of course, for you, as individuals, to subscribe myself," &c. And yet the "Professor," in the face of all this, asserts, the surgeon wishes to "wrest from the apothecaries their means of livelihood, if he could, and "to become an apothecary, if he can;" and to "convert the College of Surgeons into a corporation of drug vendors;" and also says, we are "grovelling guardians of the science and honour of surgery;" and that each wishes to "sell a sixpenny draught to a pauper, who cannot give him a fee, and therein find remuneration for his professional service."

As to my assertion, that the persons lecturing at Apothecaries' Hall were, except the one, or perhaps two, who were or are apothecaries, "renegades from the sworn interests and honour of our medical and surgical professions and colleges;" though I did not, as you see, thereby charge them with perjury, as the "Professor" says I did, but only with renegadism or desertion in the day of battle, yet he himself actually tries to prove, as to himself individually, at least, what I had not intended to do as to any; for he quotes the oath that he took by our "charter," as follows:—"I do solemnly swear, that I will observe and be obedient to the statutes, by-laws, and ordinances, of the Royal College of Surgeons;" which "charter," and which "by-laws, statutes, and ordinances," so sworn to, I understand alone and clearly to fix the "sworn interests and honour of the professions and Colleges;" though he cavils as much with the expression as with the duty. Does he not know, and did he not know, before he applied to be appointed at the Hall school, that on the "5th November, 1832," it was "unanimously resolved" by the College,

of which he was a "member," as follows?—

"Royal College of Surgeons of Ireland.—At a meeting of the College, 5th November, 1832, a letter from the Secretary of Apothecaries' Hall having been read, requesting to be informed whether a certificate of attendance on a course of lectures, delivered in it, will be received as a part of the course of education, required by the College, and as a qualification from candidates for letters testimonial, the College unanimously adopted the following resolutions":—

"Resolved—That it appears to the College that the Apothecaries' Hall was established, by the Act of the 31st of Geo. 3. (1791.) cap. 34, for the purpose, as the preamble declares, of obviating the "many and great inconveniences which have arisen from the want of a Hall, amply supplied with medicines of the purest quality, prepared under the inspection of persons well skilled in the art or mystery of such preparations;" and also, because "frequent frauds and abuses have been imposed and practised on many of his majesty's subjects, by the ignorance and unskillfulness of persons pretending to the art and mystery of an apothecary, to the injury of the fair trader, the disappointment of the physician, and the imminent hazard of the lives of his majesty's subjects;" and also, "for the purpose of enabling" the corporation of apothecaries "to create a fund for the erection of such Hall, with such authority, &c. as may in future prevent such frauds and abuses, and thereby tend to preserve the health of his majesty's subjects;" and likewise, as the 22d section of the said Act sets forth, "to prevent any person from opening a shop, or acting in the art or mystery of an apothecary, in Ireland, until he shall have been examined as to his qualification and knowledge of the business," by its "directors."

"Resolved—That the College having been advised by counsel, that the directors of the Apothecaries' Hall are not authorized, by the Act above noticed, to establish a school of medicine or surgery, or to appoint professors, for instructing students in those branches; and being satisfied that the application of the funds of the company, or the appropriation of the Apothecaries' Hall, to such a purpose, is a departure from and

a perversion of, the objects contemplated in that Act, which was constructed as much for the advantage of the physician and surgeon, and the safety of the public, as for the regulation and maintenance of the corporation of apothecaries; the College will not receive as qualification, from candidates for letters testimonial, any certificates granted after this date, from teachers lecturing in, or appointed by the Apothecaries' Hall, but on the contrary will use its utmost efforts to prevent the Apothecaries' Hall from being converted to purposes which the legislature never contemplated." This refusal to join them in an illegal act is one chief cause of their hostility. But we saw clearly through their schemes at once.

Did not the "professor," in the teeth of that unanimous determination of his College, since seek and obtain his professorship a year or two ago? and, what is more, is he not pledged, as a member of the College, by his oath, and still more, if an examiner, to refuse the very certificates that he may give as "professor" at the Hall?

Let your readers judge, whether the "professor" spoke quite correctly, or without concealment of the implied meaning of the above, when he said, this is a "calumny," "an unfounded and disingenuous calumny;" and "there does not exist statute, by-law, or ordinance of the Colleges of Surgeons or Physicians, which forbids a member or licentiate from becoming a professor in any school which may be open to him." Does this unanimous resolution and ordinance not do so, both actually and implicitly, by declaring the whole school illegal? Does this not, both in words and virtually, incapacitate the College members from doing justice to the College and their may-be pupils, if at that school? It may be said, this is unfair. It is not. For the principle on which the College acts is this: any one or more persons may establish a school; and if well conducted, it will be recognized by the College as freely as its own; or the apothecaries may, as individuals, combine to establish a school, with voluntary funds, and if good it will be recognized; but if their corporation diverts a part of its funds to this object; or if they call it a part of their incorporated rights; we believe they infringe on their act, and invade our charter;

and if they further enforce attendance at such illegal school on their apprentices, by the fear of a still more illegal examination, they defraud and oppress them; and the College know this to be the case, and will not be parties to it. Aware of all this, he applied for and fills a professorship in that very school, so that contradictory duties entangle him now. If, he says, he can thus, as I conceive, "strain his conscience to swallow both gnat and camel," I can only pity what I should call both bad taste, and a want of a clear understanding of what I believe to be the meaning of the English words of his oath, though he may believe the contrary. He also says that he swore, "I will, to the utmost of my power, endeavour to promote the reputation, honour, and dignity of the said College." And is he now doing so, by leaguening himself with the violent and indecent resolutions of the Apothecaries' Hall, or of the Society of Apothecaries of Dublin, or of the apothecaries of Cork, Limerick, Galway, and Sligo, so stirred up by the itinerant "Professors" of the Hall, to violence? All I can say is, that his conscience differs very materially from mine. For what are the words that he and his party sanction, and of which he says, he "only hopes that they will continue, as they have commenced, to defend themselves, without forgetting that something is due to the station which they hold in the opinion of society." Let us see; have they done so? The "Secretary of the Hall," in his advertisement of 17th Nov. 1837 (see *Dublin General Advertiser*, 18th Nov. 1837) says, "the College wish to injure the reputation, and trench on the legal rights, of the apothecary profession." "The governor and council of Apothecaries' Hall," by resolution of 24th Nov. 1837, (see *General Advertiser*, 25th Nov. 1837) "unhesitatingly pronounce the words, 'mischievous' and 'dangerous,' applied by the College, as altogether unwarranted and untrue." Now, listen to the words of the College "notice." They were merely these: "a committee was appointed to prepare a petition to parliament, praying for the removal of the abuses and imperfections of the existing laws for regulating the business of an apothecary in Ireland, which are found to be mischievous and dangerous to the public, and oppressive,

injurious, and degrading to the medical profession." This the Apothecaries' Hall call also "threat," "overbearing, and intrusive," and "a monopolizing spirit," and "false statements and injurious attempt." Their "circular, also, of the 7th Dec. 1837, calls it "presumptuous threat of spoliation and monopoly." The "Society of Apothecaries," by resolution of 20th Nov. 1837, (see *General Advertiser*, 25th Nov. 1837,) "with indignation repel" it, as "a jealous and intolerant spirit," "denounce" it, &c. as the "inconsiderate act of a disappointed and headstrong party, which degrades the College." "Threat," "usurpation," "dangerous," "monopoly," "aspersions," "indiscretion," and "abortive," are the only words they can find.

The Cork apothecaries, misled by the itinerators from Dublin, by resolution of 20th Dec. 1837 (see *Cork Constitution* and *Dublin Evening Mail*, of 5th January, 1838,) "have seen, with surprise and indignation, the College of Surgeons' advertisement, most unwarrantably and falsely aspersing the apothecaries' professional character," "attribute to unworthy motives" this "attempt to exclude from practice," by "unjust and invidious exclusion, and desire to protrude the apothecary." The "Galway apothecaries" (see *Galway Patriot* of 17th January, 1838,) similarly misled and misinformed, "have seen with surprise and indignation," &c., "denouncing our practice," they "deprecate and despise the imbecile and futile attempt," and "denounce the attempt as unworthy," "fling back the unworthy, untrue, and libellous imputation," and "desperate project," "engendered by public neglect, and nurtured by chagrin and professional disappointment, and ill-concealed jealousy of the obscure licentiates of the College of Surgeons," who are "inexperienced surgeons," and "unemployed surgeons," and with "total incompetency of the surgeons," "dangerous exercise of the science of physic," and "ignorance of surgeons," want to be "importers of English and Scotch apothecaries."

The Sligo apothecaries, also misled as above (Resolution of 17th January, 1838; see *Dublin Evening Post*, 27th January, 1838,) "view with indignant scorn the opprobrious and unprofessional language," the "futile attempt," of

which "envy and disappointment are the sole actuating cause." Now, please to read over again the College of Surgeons' advertisement, and see if it contains one word justifying such language, either in the part I have already quoted, or in the only other sentence which it contained, as follows: The committee are "also to prepare a bill in conformity with that petition, to be introduced next session, to enable all persons legally authorized to practise medicine or surgery in Ireland, to dispense medicine to their own patients; and further, to prevent them, or any other person who practises medicine or surgery, from compounding or dispensing the prescriptions of other practitioners." Did we wish to impose any restriction on others more than on ourselves?

When the "Professor," as leagued with such men, asks "How have the profession, and Colleges of Physicians and Surgeons, become degraded, by their members becoming professors of the Apothecaries' Hall?" I answer, first by those members who had no regard to the first part of their oath, "to observe and be obedient to the statutes, by-laws and ordinances;" or to the second part, and "to promote the reputation, honour, and dignity of the College, to the utmost of their power," and secondly, by being parties to a system for imposing upon the public under the name of a "license" or a "certificate" "to open a shop" from the Hall (which they now call a "diploma,") a class of imperfectly uneducated practitioners, as their *curriculum* of 9th October, 1837, proves they must be, which may be run through by attending the lectures at two different schools, in nine consecutive months, or at the utmost at one school in eighteen months, after which they get this unauthorized diploma. *Sunt quos curriculo pulverem olympicum collegisse juvant* may well be applied to such a dusty gallop after lectures, to collect this diplomatic suffrages of this would-be College of Pharmacy, and of all other sciences.

Let the Professor say, how can the theory and practice of chemistry, anatomy, physiology, and dissections, materia medica, pharmacy, and therapeutics, theory and practice of medicine, and surgery, forensic medicine or medical jurisprudence, midwifery, clinical lectures, and hospital, &c. &c. be all con-

densed into such a short space of time? and how can any man issue from such a hurried course, fit to carry on all four branches of a difficult profession, as he actively and potentially asserts, by joining that school, and authorizing its advertisements, and giving his co-operation and certificates?

As to the general question in discussion, I need, however, say little on it, as my letter in the *Dublin Evening Mail* put it in so true a light that it is unnecessary for me to try to do it better in your columns; especially, too, as the "Professor's" letter has been already answered, I believe, as to this point, by some "member of the College," in a late number of your *GAZETTE*; and I sent you, the other day, another letter of mine, in the *Dublin Evening Packet* newspaper, of 25th January, 1838, containing a ransacking exposition of the apothecaries' excesses and neglects, shortcomings and transgressions, of their own "act" of incorporation of "1791." I trust you will give it a place in your journal, and I believe your readers will acknowledge it, at least, is unanswerable. Perhaps, indeed, it would not be too much to request your insertion of both these documents, from the newspapers, together with this rejoinder, which would entirely prevent the necessity of my ever trespassing on your columns again on this subject, which, I think, is entirely exhausted in these three letters.

The "Professor" misstates the fact, when he says, "the apothecary is compelled, as well by the wants of society as by the position in which he is placed by others, rather than by himself, to act in the double capacity of the prescriber and dispenser of medicines." Such is not the fact, nor are such the causes. The apothecaries' attempts to become prescribers as well as compounding, are the result of a systematic plan of their own, to quit their legal position and become unlicensed (or, in a few instances, licensed) general practitioners, and displace all above them; so leaving no pure apothecaries to make up, as their act directs, the prescriptions of physicians.

The "Professor" says, "the Hall have accordingly long required the candidates for its license to attend certain courses of medical lectures." I do not at all believe this assertion to be true,

except for a very few late years; and am quite certain, that for nearly all the years since their act of 1791, they never thought of any such requirement. Let him prove they did, and also state when they began to demand each special course. He adds, "the number of these courses has been from time to time increased, until at present a certain certificate of attendance upon each of the branches of medical science included in the ordinary medical and surgical curricula, is required." He should have added, as their advertisement of "9th October, 1837," does, which would explain his words "a certain certificate," "the bare minimum in the different branches has been required;" which lets us into the secret of what a contemptible "curriculum" it is, and how obviously adopted merely to deceive the public by the name of so many lectureships. But will "the Professor" please to state by what section, sentence, or word of the "Apothecaries' Act of 1791," they are authorized to require maximum, or minimum (and not Zero), of any such thing? And do not its sections 18, 22, 24, 25, 27, expressly state the contrary; and that a seven years' apprenticeship, after a preliminary examination as to their school education, and subsequent good answering on pharmacy, are all that is legally requisite? If they require more, cannot any one, by mandamus from the Court of Queen's Bench, compel them to examine and admit him? Was not a young gentleman who was lately rejected by them, on such illegal examination as to other matters, passed on appeal, by the "College of Physicians," after a mere examination in pharmacy, as their Act (23d section) directs? Can he deny or disprove these facts? Will he show any clause, or phrase, or word, in the "Irish Apothecaries' Act," at all like those in the "English Apothecaries' Act," of 12th July, 1815 — viz. "55 George III. chap. 194;" which says, in "sections 14, 15, the court shall examine all persons applying to them, for the purpose of ascertaining their skill and abilities in the science and practice of medicine," &c. &c. and he shall produce testimonials, &c. &c. of a sufficient medical education, &c. &c.?"

The truth is, that all English and Scotch medical men, and all publications

in London and Edinburgh, mistake wholly this Irish question, from not knowing what the word apothecary means in Ireland, and supposing it to mean the same in England. Let me explain it. The apothecaries of Ireland are, by Act of 24th of June, 1791, precisely what the chemists of England are now—namely, mere compounders; and there is no resemblance at all between them and the present apothecaries of England, whose Act of 1815, 55 Geo. III., chapter 194 (by section 14, 15, especially), obliges them to be educated for the purpose of becoming what they really are—"general practitioners in medicine, and apothecaries;" and not the mere manipulators of others' (namely, physicians' and surgeons') prescriptions; as it was clearly intended the Irish apothecaries should be, by the first section of their Act of 1791, which incorporates them, 1st, to prevent the "injury of the fair trader;" 2dly, to prevent "the disappointment of the physician;" and 3dly, to prevent "the imminent hazard of the lives, &c." by "frequent frauds and abuses, imposed and practised on many of his Majesty's subjects," by the "ignorance and unskilfulness of divers persons pretending to the art and mystery of an apothecary;" and the many and great inconveniences which have arisen, first, from the want of a Hall amply supplied with medicines of the purest quality, prepared under the inspection of persons well skilled in the art and mystery of such preparations;" and secondly, from the previous non-existence of a body "to inspect and direct all chemical and compound preparations and experiments."

The Professor says, "But they say the school of the College is a chartered school. A chartered school, indeed! Chartered in an Irish sense of the word,—i. e. a school without a charter. Where is its charter? It has not one: the School of Physic has a charter, but it has not. Albeit it has been set forth in medical almanacks as one of two chartered schools with which Dublin is provided, and as "established by act of parliament. It was established by a body having a charter, and so has the school of the Apothecaries' Hall, but no more. That charter was originally granted, as I will show in another com-

munication, for another purpose, and contained no mention whatever of a school, from one end to the other." To pass over his ridicule of his own country, which reminds one of the proverb about the "dirtiness of the bird that befouls its own nest," I say the precise reverse is the fact, and he does not seem to know the difference between a charter and an act. The school of the College of Surgeons is established, not by act of parliament even, but by royal charter. The College of Physicians has a charter, but the School of Physic (as now completed), I believe, has not, but is established only by an act of parliament. The apothecaries have not any charter at all, nor does their act authorize a school. Has he ever read the College of Surgeons' charter, to which he signed his name and swore to conform? Does it not say, "Whereas, King George the Third, duly considering that the regulation of the profession of surgery was of the utmost importance to the public, and highly necessary to the welfare of mankind, and that the public sustained great injury from the defects in the system of surgical education in Ireland, and that the regularly-educated surgeons of the city of Dublin (who had become a numerous and considerable body) found themselves incompetent, from the want of a charter, to establish a liberal and extensive system of surgical education in our said kingdom, by letters patent of 11th February, 24th year of his reign (1782, I think), did establish a College or Corporation of Surgeons, in the city of Dublin, by the name of the Royal College of Surgeons in Ireland. And whereas this wise and benevolent design and intention of our royal father has, from that period to the present day, had the most beneficial influence by improving the profession of surgery, and thus promoting the welfare of the nation at large, and particularly by providing a sufficient number of properly-educated surgeons, as well for the service of the public in general as for that of our army and navy.

"And whereas we are graciously pleased to approve of the said institution and foundation, and conceiving that certain alterations may be made in said charter, so as to constitute a corporation in our said city of Dublin, of

regular, able, learned, and experienced practitioners in surgery, endowed with powers, jurisdictions, and privileges, convenient and requisite for enforcing a due course of regular education for the apprentices and students of surgery, previous to their tendering themselves to the College for examination; and further empowering the corporation to create a fund (payable by their members, licentiates, apprentices, and others, applying for instruction and examination) sufficient for keeping the several buildings and *schools* of said College in proper repair; enlarging them when required, and supplying the library and museum thereof with suitable books and anatomical preparations, as well as for discharging all salaries and defraying all other expenses which the said College may incur. Know ye, therefore, that we, of our special grace, certain knowledge, and mere motion, in compliance with the humble petition of the President and Members of said Corporation in College assembled, and upon the surrender made by the said President and Members, of the aforesaid letters patent, granted by our royal father which we have graciously accepted, and according to the tenor and effect of our letters, under our privy signet and royal sign manual, bearing date at our Court, at Windsor, the 2d day of June, 1828, in the ninth year of our reign, and now enrolled in the Rolls of our high Court of Chancery of Ireland, have granted, ordained, constituted, and appointed, and by these presents for us, our heirs and successors, do grant, ordain, constitute, and appoint, &c." Let the "Professor" now say, are not the *schools* of the College *chartered*, and recognized as a part of the charter, and all their expenses, too, provided for by it?

Let him produce one word from the "Irish Apothecaries' Act of 1791," which in words, or by implication, or in any way whatever, speaks of, or suggests, or intimates, or implies, a school of any kind (except each Master's shop), in connexion with the Hall, as the College of Surgeons' charter expressly does.

The "Professor" mystifies the whole question when he says, we "object to the apothecaries educating themselves as well as possible in all branches;" or

that we object to any one "lecturing to them or for them;" or to their becoming general practitioners, by taking out, in addition to the apothecaries' license, medical and surgical diplomas in the usual way. We never did. All we object to is, their depriving us of the services of well-educated apothecaries, by turning away their own and their pupils' time, and their funds, to objects unauthorised by their Act, and compelling their apprentices to attend lectures, and produce certificates, which it does not require or authorise; and still more do we object to their attempting to compel us still to send our prescriptions to them, after they have tried to become rival practitioners, and ceased to be willing to attend to their business of making up our prescriptions as pharmacopolists.

The "Professor" says, that the "Graduate of the Three Colleges," "stands forth the intemperate partisan of one, and the vilifier of another of them." I hope it is no shame to be a member of three Colleges, in the three capitals of the empire. He then adds, "How, let me ask, is such conduct to be reconciled with a due regard to the interests and honour of these corporations?" and then closes by a sentence, of which I profess myself wholly unable to understand the meaning of one part:—"Perhaps he may say my signature is only assumed and fictitious. I answer, it matters not; light objects indicate the current of the air. This circumstance, trifling as it may be, proves how little principle is concerned in the matter, and how dead to its obligations is he who voluntarily places himself in such a position." This seems to assert, that he himself is as light as a feather tossed by the wind; and that, in signing his name, he has little principle, and is dead to the obligation of that little. It just strikes me, however, that perhaps he meant to speak of me, as embarrassed by connexion with three Colleges. I have never felt so, for the duties to all three are identical, namely, to support their honour and dignity; and this is best, and truly, and only done, by supporting the honour and dignity of the whole medico-surgical profession, as one complete whole. But let him say, is not every one employed by the Apothecaries' Hall obliged, by section eleven of their Act, to "swear

that he will be true and faithful to the said Governor and Company." &c. &c. If he has taken this oath, it is inconsistent with his previous oath at the College; for the Hall Directors are systematically pursuing illegal plans to injure the College and its schools, and to interfere with medical and surgical education. If the Directors have not administered this oath to him and his colleagues, they all acknowledge thereby the illegality of their school and professorships, for they are obliged by their Act to make all persons "employed by them" take it; and they thus, too, confess, they have no right to employ any persons but "inferior agents and servants."

As to intemperance, let the reader judge, after perusing all his letter, and all of mine, which is intemperate. As to the Edinburgh University, I did not speak at all of its curriculum or character. As to his assertion of my vilifying the London College, he states what is not true; my only words were, "I am a member of both (Dublin and London Colleges of Surgeons), and know that the London course of study and nature of examination are very inferior to those in Dublin." Did I not speak truth? Does the "Professor" not know it? The one I passed in 1815, the other in 1818, and therefore have the best reason to know it; and I fully believe it to be so still (though in a less degree), from their inferior curriculum in London, the less time taken up in examination, the fewer examiners of each candidate, and the passing of men there who had failed here: this I regret. This is no vilification, but truth simply stated, that a defect may be corrected.

Our College has no monopoly, either by charter or by law. An Act of Parliament says, that all surgeons to country hospitals in Ireland, shall be graduates of our College. This is the act, not of us, but of the legislature, on a conviction that it is due to our superior education. But not so the Apothecaries' Act, which, by a vile monopoly, excludes every English and Scotch apothecary, however well educated, from practising in Ireland, unless examined again in Dublin. No word of our charter excludes any surgeon from Ireland; nor do we even require an apprenticeship. The only principle for

which we contend is, that studies and examination should be identical and equivalent in all the Colleges and Universities; and as soon as they are, all exclusive privileges and monopolies will necessarily cease, as they ought. Let this principle be fixed by law; and let any man who has passed a proper examination before any proper College, be equally privileged in all others, and also not liable to be subjected to a second examination on crossing the Tweed or the Channel, and we will offer no opposition to the legislature's abolishing its own act, above mentioned. Is not that fair? Would not there be then a free trade, and free competition in College education, and no favour or monopoly? That is all our wish.

I end by the "Professor's" "Alas! how weak and fallible an attribute is reason, when blinded by intemperate zeal and overweening selfishness." Has he not become intemperately zealous for his own interest, as connected with his professorship at the school of the Apothecaries' Hall, which my letters have helped to put in jeopardy? Had he no selfish object in seeking it? Did they never previously, and do they not now, call him in to consultations, as a *douceur* for his aid of their plans? Does he lecture gratis? Does he get no *quid pro quo* in this way or any other? Let him point out one single instance in which I have sought any selfish object, either in the College or out of it, or in any other act of my public or private life. What possible selfish interest could I, or any of us have, in thus exciting the hostility of most of the apothecaries? Has he none in currying favour with them? Can apothecaries not serve or disserve physicians or surgeons in various open and concealed ways? My zeal is for the good of the profession, and the safety of the public, and against my own pecuniary interest, as he well knows.

I have the honour to subscribe myself
Your obedient servant,

A GRADUATE IN MEDICINE, SURGERY,
AND MIDWIFERY, OF DUBLIN,
EDINBURGH, AND LONDON.

Jan. 31, 1835.

IRISH COLLEGE OF SURGEONS.

To the Editor of the Medical Gazette.

SIR,

I owe it equally to you and to myself to address to you this letter, which I have been prevented from doing earlier by the circumstances which have intervened since my last.

In saying that you had been led into error, I had no intention of preferring a complaint against you, and therefore I used the word "led," because I felt assured that the cause of the error lay elsewhere.

At such a distance you cannot understand the precise position of the several parties, and I suspect that you are under the impression that the College of Surgeons in Dublin is constituted similarly to that in London. Such is not the case. In the former there is no permanent "ruling body," or Council, as there is in the latter. The ruling body of the Dublin College is the majority of the day; that majority may not be the same on two successive days; it *may* even, with exception of the officers of the College, consist of different individuals on different days, and the majority of one day may, according to prescribed forms, undo or counteract the acts of that of the preceding one: such is the constitution of our College. It is, in fact, a democratic one, but devoid of that check which is so necessary to the safety of all democratic constitutions, viz. responsibility. We are responsible only to ourselves, *unless we violate our charter or the laws of the realm*. Next, sir, our committee is not our ruling body, but our responsible servant. We have many committees appointed for particular purposes, but never authorised to assume the "authority of the College" itself, and the acts

of our committees never become acts of the College until they have been submitted to it, and received its adoption and sanction. It appears, then, to me, that I can no more be said to have entered the lists against the College in any sense, than a member of either party in the House of Commons can be said, because of his opposition to the other, to have entered the lists against the House.

Now, with regard to the letter of which I have complained, you have done me a great service by stating the fact that it was dated "Royal College of Surgeons, Dublin," and signed "C. O'Keefe, Registrar." These are the ostensible guarantees for the authenticity of any communication having been made by the authority of the College; and you were, in my opinion, fully authorised, under the circumstances, to say that it was so. But it has since appeared that it was *not* done by authority of the College; at the time, my letter had not been brought under its consideration, and that it was the act of the committee, to which it thought fit to attach the name and authority of the College. My letter has been made the subject of discussion in the College during the past week. The course and result of the discussion, however, I refrain from adverting to at present, in order that my adversaries may not have cause to complain; but I am at liberty to state, inasmuch as he has avowed himself, that the author of the reply to my first letter is Dr. Maunsell, Professor of Midwifery in the School of the College of Surgeons.

I conclude, reminding you that his pledge is as yet unredeemed.

I have the honour to remain,

Your obliged and obedient servant,

B. ALCOCK.

Dublin, March 17, 1838.

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LONDON :
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